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AND

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BY WILLIAM NICHOLSON.

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PREFACE.

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Soho Square, London, May 1, 1806.

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Singular Form
of Saturn

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JANUARY, 1806.

ARTICLE I.

On the Cauſe of Fairy Rings. By JOHN GOUGH, *Eſq.*

To Mr. NICHOLSON,

SIR, *Middleſhaw, December* 7, 1805.

YOU publiſhed in the firſt page of your ninth volume in octavo, a letter to me from the Rev. Jonathan Wilſon, vicar of Biddulph, in which the appearance of a patch of ground recently blaſted and torn up by lightning was deſcribed. The obſervations of this ingenious and accurate gentleman promiſed to throw light on the natural hiſtory of fairy-rings, provided they were continued, and in this expectation, I took the liberty in a note ſubjoined to the copy of his letter printed by you, to requeſt his future remarks on the ſubject, drawn from repeated inſpection of the place affected by the lightning. The following is an extract of a letter from Mr. Wilſon, containing his obſervations relative to the ſubſequent appearances of the patch, with ſome thoughts which are certainly an improvement in one theory of fairy-rings, that has received the patronage of ſome writers. This letter is dated November 1, 1805, and after ſome prefatory matter, proceeds thus,

Mr. Wilſon's remarks begin.

"In conſequence of your eſteemed favour of the 14th of Auguſt, 1804, I went on the 2nd of September following, to view the place which the lightning had ſtruck, being accompanied by the farmer of the grounds.

The place not eaſy to be found.

The affected ſpot was not then very eaſy to be diſtinguiſhed, as the injured thiſtles were generally overtopped at the time, but we had no doubt of its true ſituation, upon finding the place where we formerly dug in purſuit of the imaginary ſtone.

Slight veſtiges of the lightning.

Some dead graſs appeared, but it was confined to the ſpace within which the roots had been plowed up by the electric fluid. The verdure was undoubtedly brighter about the hole, and the farmer was willing to attribute the flouriſhing ſtate of this circle of herbitage to the lightning; but for my part, I aſcribed it to what had dropped from his cows, rather than any thing that had fallen from the clouds.

Theſe veſtiges not permanent.

"I have not been able to perceive the leaſt difference between the part ſtruck, and the reſt of the field, during the courſe of the preſent year; my obſervations muſt therefore be acknowledged not to favour the hypotheſis, which ſuppoſes fairy-rings to be formed at firſt by lightning.

The explanation by grubs improved.

"I never ſaw a fairy-ring, and therefore may ſeem badly qualified to write on the ſubject; but from what I have read, it appears to me, that the circle of decayed graſs is cauſed by the innumerable grubs, which are ſaid to lay concealed under the ring among the roots of the herbitage; I alſo ſuppoſe, that the funguſes commonly ſeen on fairy-rings, give a preference to theſe circles on account of the abundance of dead vegetable matter to be found in them; amongſt which various ſpecies of fungi are known to grow. To this I may add, that the interior circle of dark green graſs is owing to the dung, and ploughing of thoſe animals in the preceding year; and the reaſon which compels theſe grubs or their offspring to puſh forward from the centre, ſeems to be this; every creature we know of has an averſion to working in its own excrement, or that of its own ſpecies."

Reflections on theſe obſervations.

The obſervations of Mr. Wilſon, ſtated above, ſeem to demonſtrate, that a patch of herbage is not invariably converted into a fairy-ring by a powerful ſtroke of lightning; conſequently if electric diſcharges from the atmoſphere be the primary cauſes of theſe circles, they require the aſſiſtance of ſome peculiarity in the ſoil to give permanency to the appearance.

ance. Moreover the circular figure of these phenomena embarrasses the electrical hypothesis with a second difficulty; for the tracts of discoloured grass, actually produced by lightning, are but seldom bent into rings, as they more frequently assume a zigzag, or else a ramified form, and are, I believe, of but short duration; which shews, the roots of the herbage are not destroyed, unless where the earth is torn up. The theory which attributes these circles of withered grass to the running of a fungus, has little or no foundation; because these imperfect plants, generally speaking, attach themselves to dead vegetables, consequently their presence in fairy-rings is nothing more than an appearance which is subsequent to the destruction of the herbage upon them. As for the lively verdure of the grass on the interior edges of these circles, I believe it may be explained upon general principles, without the agency of lightning or fungi. For if the herbage of a patch of ground be destroyed root and branch by a cause which does not remove the remains of it, the place will be covered, in process of time, with a fresh crop of plants, possessing superior luxuriancy and verdure. The causes of this vigorous vegetation appear to be the following; the dead roots and stems rot and manure the soil which produced them: this source of fertility receives an additional supply from a succession of fungi, which grow and decay on the surface of the ground; as well as from the excrement and exuvia of the grubs, which take up their abode among the withered roots; lastly, the soil is rendered more porous by the decay of the vegetable remains, and thereby becomes more permeable to the air, which increases its fertility not a little. The last position seems to be confirmed by the circumstance of plants thriving better in unglazed, than in glazed flower pots. The following facts may be adduced in corroboration of what has been here advanced. A small piece of ground was covered, in June, with common salt, which had been rubbed upon the corps of a drowned man; the herbage of this place died in a short time; but was succeeded the next summer by a new crop, the verdure of which distinguished it for some years from the surrounding grass. As common salt is esteemed a manure, perhaps the following instance will be called preferable to the former. Many woods in this country, especially those about Windermere, are cut

Difficulties of the electrical theory.

Fungi not the cause of fairy rings.

Superior verdure of these rings explained.

Dead plants, &c. manure the ground.

The air increases vegetation by acting on the roots.

Proofs of the preceding theory.

down about once in fourteen years, and converted in charcoal, for the use of the iron works. This is done throwing the branches into large heaps; which are th covered with green turf, and set on fire. These piles wood continue burning for several days, in consequence which, the roots in the ground beneath them are complete charred; and the pit-stead, as the place is called, has verdure left upon it. The loss however is repaired in course of a few years by nature herself, where art does interfere, and the spot is clothed with a fresh coat of herbag consisting of herbs remarkable for their size and flourishi appearance. This instance of vigorous vegetation on grou that has been completely burned, in all probability, is occ sioned by the texture of the soil; which is adapted to ret moisture, and admit the air; unless we suppose the incorru tible substance of charcoal to afford a species of manure.

The preceding hints may perhaps incite some of your read to study the natural history of fairy-rings with greater diligenc especially to search after the true cause which blasts these ci cles; for when this is discovered, we shall be able to re-clot them with superior verdure, on rational principles.

I remain, &c.

JOHN GOUG

II.

Observations on the singular Figure of the Planet Saturn. By WILLIAM HERSCHEL, *LL. D. F. R. S.*[*]

Examination of the striking phenomena of the planet Saturn.

THERE is not perhaps another object in the heavens th presents us with such a variety of extraordinary phenome as the planet Saturn: a magnificent globe, encompassed by stupendous double ring: attended by seven satellites: orn mented with equatorial belts: compressed at the poles: tu ing upon its axis: mutually eclipsing its ring and satellites, a eclipsed by them: the most distant of the rings also turni upon its axis, and the same taking place with the farthest the satellites: all the parts of the system of Saturn occasiona

[*] Philosophical Transactions,' 805.

reflecti

reflecting light to each other: the rings and moons illuminating the nights of the Saturnian: the globe and satellites enlightening the dark parts of the rings: and the planet and rings throwing back the sun's beams upon the moons, when they are deprived of them at the time of their conjunctions.

It must be confessed that a detail of circumstances like these, appears to leave hardly any room for addition, and yet the following observations will prove that there is a singularity left, which distinguishes the figure of Saturn from that of all the other planets.

It has already been mentioned on a former occasion, that so far back as the year 1776 I perceived that the body of Saturn was not exactly round; and when I found in the year 1781 that it was flattened at the poles at least as much as Jupiter, I was insensibly diverted from a more critical attention to the rest of the figure. Prepossessed with its being spheroidical, I measured the equatorial and polar diameters in the year 1789, and supposed there could be no other particularity to remark in the figure of the planet. When I perceived a certain irregularity in other parts of the body, it was generally ascribed to the interference of the ring, which prevents a complete view of its whole contour; and in this error I might still have remained, had not a late examination of the powers of my ten-feet telescope convinced me that I ought to rely with the greatest confidence upon the truth of its representations of the most minute objects I inspected. Its body oblate. Reasons why the greater peculiarities of its figure were overlooked;

The following observations, in which the singular figure of Saturn is fully investigated, contain many remarks on the rest of the appearances that may be seen when this beautiful planet is examined with attention; and though they are not immediately necessary to my present subject, I thought it right to retain them, as they show the degree of distinctness and precision of the action of the telescope, and the clearness of the atmosphere at the time of observation.

April 12, 1805. With a new 7-feet mirror of extraordinary distinctness, I examined the planet Saturn. The ring reflects more light than the body, and with a power of 570 the colour of the body becomes yellowish, while that of the ring remains more white. This gives us an opportunity to distinguish the Very perfect observation, in which the circumference was seen to be flattened in four regions.

ring from the body, in that part where it crosses the disk, by means of the difference in the colour of the reflected light. I saw the quintuple belt, and the flattening of the body at the polar regions; I could also perceive the vacant space between the two rings.

Observations on Saturn by which its singular figure is ascertained.

The flattening of the polar regions is not in that gradual manner as with Jupiter, it seems not to begin till at a high latitude, and there to be more sudden than it is towards the poles of Jupiter. I have often made the same observation before, but do not remember to have recorded it any where.

April 18; ten-feet reflector, power 300. The air is very favourable, and I see the planet extremely well defined. The shadow of the ring is very black in its extent over the disk south of the ring, where I see it all the way with great distinctness.

The usual belts are on the body of Saturn; they cover a much larger zone than the belts on Jupiter generally take up, as may be seen in the figure I have given in Plate I.; and also in a former representation of the same belts in 1794.*

The figure of the body of Saturn, as I see it at present, is certainly different from the spheroidical figure of Jupiter. The curvature is greatest in a high latitude.

I took a measure of the situation of the four points of the greatest curvature, with my angular micrometer, and power 527. When the cross of the micrometer passed through all the four points, the angle which gives the double latitude of two of the points, one being north the other south of the ring, or equator, was 93° 16′. The latitude therefore of the four points is 46° 38′; it is there the greatest curvature takes place. As neither of the cross wires can be in the parallel, it makes the measure so difficult to take, that very great accuracy cannot be expected.

The most northern belt comes up to the place where the ring of Saturn passes behind the body, but the belt is bent in a contrary direction being concave to the north, on account of its crossing the body on the side turned towards us, and the north pole being in view.

There is a very dark, but narrow shadow of the body upon the following part of the ring, which as it were cuts off the ring from the body.

* See Phil. Trans. for 1794, Table VI. page 32.

The

The shadow of the ring on the body, which I see south of the ring, grows a little broader on both sides near the margin of the disk.

Observations on Saturn, by which its singular figure is ascertained.

The division between the two rings is dark, like the vacant space, between the ansæ, but not black like the shadow I have described.

There are four satellites on the preceding side near the ring; the largest and another are north-preceding; the other two are nearly preceding.

April 19. I viewed the planet Saturn with a new 7-feet telescope, both mirrors of which are very perfect. I saw all the phenomena as described last night, except the satellites, which had changed their situation; four of them being on the following side. This telescope however is not equal to the 10-feet one.

The remarkable figure of Saturn admits of no doubt: when our particular attention is once drawn to an object, we see things at first sight that would otherwise have escaped our notice.

10-feet reflector, power 400. The night is beautifully clear, and the planet near the meridian. The figure of Saturn is somewhat like a square or rather parellelogram, with the four corners rounded off deeply, but not so much as to bring it to a spheroid. I see it in perfection.

The four satellites that were last night on the preceding, are now on the following side, and are very bright.

I took a measure of the position of the four points of the greatest curvature, and found it 91° 29′. This gives their latitude 45° 44′,5. I believe this measure to be pretty accurate. I set first the fixed thread to one of the lines, by keeping the north-preceding and south-following two points in the thread; then adjusted the other thread in the same manner to the south-preceding and north-following points.

May 5, 1805. I directed my 20-feet telescope to Saturn, and, with a power of about 300, saw the planet perfectly well defined, the evening being remarkably clear. The shadow of the ring on the body is quite black. All the other phenomena are very distinct.

The figure of the planet is certainly not spheroidical, like that of Mars and Jupiter. The curvature is less on the equator and

Observations on Saturn, by which its singular figure is ascertained.

and on the poles than at the latitude of about 45 degrees. The equatorial diameter is however confiderably greater than the polar.

In order to have the teftimony of all my inftruments, on the fubject of the ftructure of the planet Saturn, I had prepared the 40-feet reflector for obferving it in the meridian. I ufed a magnifying power of 360, and faw its form exactly as I had feen it in the 10 and 20-feet inftruments. The planet is flattened at the poles, but the fpheroid that would arife from this flattening is modified by fome other caufe, which I fuppofe to be the attraction of the ring. It refembles a parallelogram, one fide whereof is the equatorial, the other the polar diameter, with the four corners rounded off fo as to leave both the equatorial and polar regions flatter than they would be in the regular fpheroidical figure.

The planet Jupiter being by this time got up to a confiderable altitude, I viewed it alternately with Saturn in the 10-feet reflector, with a power of 500. The outlines of the figure of Saturn are as defcribed in the obfervation of the 40-feet telefcope; but thofe of Jupiter are fuch as to give a greater curvature both to the polar and equatorial regions than takes place at the poles or equator of Saturn which are comparatively much flatter.

May 12. I viewed Saturn and Jupiter alternately with my large 10-feet telefcope of 24 inches aperture; and faw plainly that the former planet differs much in figure from the latter.

The temperature of the air is fo changeable that no large mirror can act well.

May 13. 10-feet reflector, power 300. The fhadow of the ring upon the body, and of the body upon the ring, are very black, and not of the dufky colour of the heavens about the planet, or of the fpace betwen the ring and planet, and between the two rings. The north-following part of the ring, clofe to the planet, is as it were cut off by the fhadow of the body; and the fhadow of the ring lies fouth of it, but clofe to the projection of the ring.

The planet is of the form defcribed in the obfervation of the 40-feet telefcope; I fee it fo diftinctly that there can be no doubt of it. By the appearance, I fhould think the points

points of the greatest curvature not to be so far north as 45 degrees.

Observations on Saturn, by which its singular figure is ascertained.

The evening being very calm and clear, I took a measure of their situation, which gives the latitude of the greatest curvature 45° 21′. A second measure gives 45° 41′

Jupiter being now at a considerable altitude, I have viewed it alternately with Saturn. The figure of the two planets is decidedly different. The flattening at the poles and on the equator of Saturn is much greater than it is on Jupiter, but the curvature at the latitude of from 40 to 48° on Jupiter is less than on Saturn.

I repeated these alternate observations many times, and the oftener I compared the two planets together, the more striking was their different structure.

May 26. 10-feet reflector. With a parallel thread micrometer and a magnifying power of 400, I took two measures of the diameter of the points of greatest curvature. A mean of them gave 64,3 divisions = 11″,98. After this, I took also two measures of the equatorial diameter, and a mean of them gave 60,5 divisions = 11″,27; but the equatorial measures are probably too small.

To judge by a view of the planet, I should suppose the latitude of the greatest curvature to be less than 45 degrees. The eye will also distinguish the difference in the three diameters of Saturn. That which passes through the points of the greatest curvature is the largest; the equatorial the next, and the polar diameter is the smallest.

May 27. The evening being very favourable, I took again two measures of the diameter between the points of greatest curvature, a mean of which was 63,8 divisions = 11″,88. Two measures of the equatorial diameter gave 61,3 divisions = 11″,44.

June 1. It occurred to me that a more accurate measure might be had of the latitude in which the greatest curvature takes place, by setting the fixed thread of the micrometer to the direction of the ring of Saturn, which may be done with great accuracy. The two following measures were taken in this manner, and are more satisfactory than I had taken before. The first gave the latitude of the south-preceding point of greatest curvature 43° 26′; and the second 43° 13′. A mean of the two will be 43° 20′.

June

Observations on Saturn, by which its singular figure is ascertained.

June 2. I viewed Jupiter and Saturn alternately with a magnifying power of only 300, that the convexity of the eye-glass might occasion no deception, and found the form of the two planets to differ in the manner that has been described.

With 200 I saw the difference very plainly; and even with 160 it was sufficiently visible to admit of no doubt. These low powers show the figure of the planets perfectly well, for as the field of view is enlarged, and the motion of the objects in passing is lessened, we are more at liberty to fix our attention upon them.

I compared the telescopic appearance of Saturn with a figure drawn by the measures I have taken, combined with the proportion between the equatorial and polar diameters determined in the year 1789;* and found that, in order to be a perfect resemblance, my figure required some small reduction of the longest diameter, so as to bring it nearly to agree with the measures taken the 27th of May. When I had made the necessary alteration, my artificial Saturn was again compared with the telescopic representation of the planet, and I was then satisfied that it had all the correctness of which a judgment of the eye is capable. An exact copy of it is given in Plate IX. The dimensions of it in proportional parts are,

The diameter of the greatest curvature -	36
The equatorial diameter - - - - -	35
The polar diameter - - - - - -	32
Latitude of the longest diameter -	43° 20.'

The foregoing observations of the figure of the body of Saturn will lead to some intricate researches, by which the quantity of matter in the ring, and its solidity, may be in some measure ascertained. They also afford a new instance of the effect of gravitation on the figure of planets; for in the case of Saturn, we shall have to consider the opposite influence of two centripetal and two centrifugal forces: the rotation of both the ring and planet having been ascertained in some of my former Papers.

* See Phil. Transf. for 1790, page. 17.

Facts

III.

Facts and Observations on the medical Respiration of gaseous Oxide of Azote. In a Letter from Dr. Beddoes.

To Mr. NICHOLSON.

SIR,

Dr. Pfaff's paper on respiration* will probably draw the attention of the scientific towards the gaseous oxide of azote, which has been too much neglected in a medical point of view. I was only sorry to see that he proposes to use it in melancholia. No combination of ideas can be more obvious than the application of an agent which has so frequently proved exhilarating, and never yet been observed to be followed by exhaustion where it did exhilarate, to a complaint, in which depression of spirits is a striking circumstance. But I am apprehensive that the first thoughts of inexperience here (as so often happens) will prove illusory, and that this project will not be followed by the expected advantage in many cases of melancholia. For if it be true that there is no real distinction between mania and melancholia, as far as the sensorium is concerned, and that the vivacity of ideas in melancholia answers to the violence of muscular actions in mania, as I have endeavoured to shew in my *Essays on Health*; is there not ground to apprehend that the actions of the brain, already too strong, will be increased by this gas, or the diseased contemplations rendered more intense?

Dr. Pfaff's experiments on respiration.

Proposes gaseous oxide of azote in melancholy madness,

If there be any state of melancholia in which it may be of service, this will probably happen when the nervous system is falling into debility, in consequence of having been kept too much on the stretch.

Cautions against its unguarded use, from theory;

But I do not here warn against gaseous oxide from mere theory. The manager of a lunatic asylum near Bristol, respectably known to the public, concurred with me some years ago in the opinion which I expressed to him concerning its probable advantage in melancholia; and a patient that had been under his care inhaled it fairly without benefit. The administration was tried in two other cases as fruitlessly: Indeed

from experience.

Cases.

* Philos. Journal, XII. 249.

I dis-

I discontinued it in one, from some indications of an aggravation of the symptoms. I was by this time alive to suspicion, having thought much on the subject, and reasoned myself into the idea that it would often do injury upon the above-mentioned principle. It has long been my opinion, and there are striking observations on record to prove that hidrogen, hidro-carbonate, azotic, or carbonic acid gases, would be more likely to answer in active insanity under whatever form. These observations I shall take occasion to quote hereafter.

Use in palsy of one kind.

The very first time I witnessed the effects of gaseous oxide on a person in health, I concluded that it would be a remedy in certain cases of palsy. A patient who had emerged from apoplexy with the loss of the power of one side of his body, was accordingly put under a course of the gas. The result completely answered expectation. The case was most carefully watched; and on withholding the gas, the symptoms repeatedly grew worse, and *vice versa*. After the patient's recovery, he was kept under inspection for a considerable time, and did not relapse. This has been confirmed by other results; and in palsy, where the brain is primarily affected, I expect that Dr. Pfaff will find either a cure or great relief to follow the use of this gas in a respectable proportion of cases.

In another kind of palsy.

I have very fairly tried it in palsy apparently from cold, beginning at the extremities and creeping from muscle to muscle, without good or bad effect. There is a case of this kind, related by Dr. Kentish, with the patient's name, and corroborated by testimony superior to all exception in *Considerations on factitious Airs (Johnson)* in which a perfect cure was obtained from oxigen gas; and I have since learned by experiments carefully repeated before various philosophical observers, that *in essential respects, oxigen gas and gaseous oxide act in a very different, nay opposite manner upon the living fibre.*

Of oxigen,

These experiments I hope to publish before midsummer.

From palsy, analogy led me to other cases of debility. I fully tried gaseous oxide in dropsy of the chest (anasarca of the lungs), but without good or bad effect. I was much disappointed, conceiving that in dropsy (at least in one species) we have a paralytic state of the lymphatics. But I have been since assured by a physician, that for some dropsies he has

in dropsies;

found

found a remedy in this gas. There are dropsies which doubtless depend on excess of exhalant action. These are easily distinguished; and they require bleeding as much as pleurisy.

In debility, arising from residence in hot climates and from intense application to business, I have known gaseous oxide completely successful after an infinity of remedies, Bath and other waters, had been tried in vain. in other f of debility

The particulars of these cases are also destined for publication: But I resolved to wait for some years after the use of the gas; for I have found that a single circumstance vitiates a large proportion of our medical records. Patients after an apparent recovery fall again into the same complaint; and there are other considerations, which I shall for the present pass over.

If Mr. Pfaff uses gaseous oxide in palsy, he will probably sooner or later see a phenomenon as extraordinary as any in galvanism, and which after it has been described by a philosopher of high reputation, will become equally celebrated. This is the instantaneous restoration of voluntary power over a limb deprived both of motion and feeling by palsy succeeding to apoplexy, while the patient is respiring gaseous oxide. This was witnessed in common with myself, by several respectable persons; and among others by some of your philosophical acquaintance, if I do not mistake. It was in the case of Mr. G. a member of the last parliament, who completely recovered: But as other means were afterwards adopted, I do not impute the result to the gas, which however, when used alone, was visibly of great service: for I have no idea of claiming for a remedy under scrutiny any cure, if other powers have been called in at the same time. Gaseous o has given v tary power palsied part while inha

I transmit these observations to you, Sir, in preference to the Editor of any Medical Journal, because I think them likely to meet the eye of Dr. Pfaff sooner in your Journal. I should be extremely sorry that he should set out wrong in his trials, because the fault will be imputed to the power itself, and not to its misapplication; and the disabled will still be left to languish and be cut off, notwithstanding we have a remedy at hand. First wish effect of t statement.

I have another reason. I most sincerely wish any thing I could say would hasten the period, which *must* arrive, when medical science shall not be merely what the Germans call a A second.

Brod-

Brod-wissenschaft, or pursued only for a livelihood. If philosophical men without a profession would take it up, it is I think certain, that it must soon become both more efficient and more liberal. Any study is capable of interesting the feelings; and most surely that of the laws of the organic world is as much so as any other. Opportunities of anatomical, chemical, and *clinical* information are at hand. A person so prepared will, heaven knows, with ardour and industry soon acquire all that is useful in medical practice. Let him then, animated with no other motive than the pure desire of benefiting his fellow men, apply himself to the improvement of medicine. It is impossible that he should not succeed as fully as our Tennants, our Hatchetts, and Chenevix's have done in chemistry; for it is not its inherent difficulty, but collateral circumstances, that retard the progress of this art. Many apothecaries, for example, and old women in general, who are the great controulers of the destiny of physicians, would by no means allow the use of gaseous oxide in palsy, though the patient in the course both of nature and of ordinary medication be sure to die, and perhaps in a very miserable manner. But the philosophical cultivator of medicine, without troubling himself about the good opinion of the one or the other, would proceed on his career under the guidance of the collective light of science and of humanity.

…s of liberal …ofity ex- …ed to study …icine.

…cdote.

N. N. advanced in years, of a thickset stature, and with a short neck, shewed signs of palsy many years ago. The writer of these lines warned his friends of the danger. Concurring in this apprehension, Dr. Ingenhousz proposed to him to inhale oxigen gas, a practice familiar to that accurate philosopher, and by which he hoped the constitution might be recruited. The execution of the idea was deferred. Meanwhile the gaseous oxide was discovered to be respirable, and its power in palsy was to a degree ascertained. The writer now pressed the use of this gas with the utmost earnestness. The patient saw it taken by others: He himself consented to inhale it, when behold! the distress of a lady present, as excited by some apprehended imaginary bad consequences, put off the inhalation. The predicted paralytic seizure arrived: but there was ample time still for the use of the oxide. I proposed that another patient, situated as similarly as possible, should be sought; and that if he consented upon the credit

credit of the successful exhibition, and upon my responsibility, to use the gas, the result should determine as to its employment in the case first in question. At the same time, I stated from the average course of paralytic attacks in general not immediately fatal, that a little apparent amendment would take place, and the stroke return with additional violence. My proposal was acknowledged to be highly reasonable; but that plan of routine treatment was followed which is so much more advantageous to the idle and unscientific of our profession than it is to the sick, and the patient died of a return of his complaint. Such is probably the condition of thousands of the diseased at this moment! Rather than use a recently proposed plan not in the Pharmacopœia, or seek a new one in analogy, we persevere in painful or disgusting means, from which, on the faith of long experience, no good of any sort can be expected for the sufferer. May the rising generation of natural philosophers exercise their talents and their benevolence in putting an end to so crying an evil!

I am, Dear Sir,

Respectfully your's,

THOMAS BEDDOES.

Clifton, *Dec.* 13, 1805.

P. S. A case in your Journal, where a gentleman accustomed to breathe gaseous oxide for amusement, experienced very disagreeable feelings on one particular occasion, seems to me clearly referable to hysteria. Now the trials at the Pneumatic Institution, as related in Mr. Davy's *Researches*, had clearly shewn that in the predisposed, gaseous oxide is a specific for exciting an hysteric paroxysm. Perhaps in the individual whose case is related by himself in the Journal, no obvious predisposition, either temporary or permanent, existed: Nothing to this purport is stated. But that the affection was simply hysterical cannot I think be doubted by any one conversant both with hysteria and the administration of gaseous oxide. It seems to be strongly marked by that idea of immediate danger, which is so common in hysteria. Dr. Garnet very unnecessarily, and, I believe, very mistakenly, called up the whole Brunonian theory on the emergency. It led him, however, to give cordials; and they were proper. A tea-spoonful of sal volatile, from

On effects of gas. ox. as stated in this Journal.

Their real nature.

from time to time, would probably have anfwered without the Brunonian theory. But it is certainly the bufinefs of the phyfician to avoid gafeous oxide in the hyfterical, as it is wine in thofe who labour under acute inflammation. If your correfpondent who related his own feelings could fpecify any caufe which might have rendered him nervous, or ftate the fact whether he was fo or not, it would give fatisfaction to the prefent writer, and perhaps alfo to future inquirers.

Caution regarding particular fubjects. Quære.

Remark on dangerous diforders.

To interdict a remedy becaufe its ufe requires difcrimination, would, in many diforders, be leaving the fick to certain deftruction. I imagine that the outcry againft fuch means as gafeous oxide, will arife from thofe who daily ufe the moft hazardous remedies, and who are enabled to do it without reproach, becaufe they are put into a phial, and the patient and his friends never trouble themfelves about the nature of the articles which they are receiving into the ftomach.

IV.

Abstract of Obfervations on a diurnal Variation of the Barometer between the Tropics. By J. Horsburgh, *Efq. In a Letter to* Henry Cavendish, *Efq. F. R. S.** *Read March* 14, 1805.

SIR, *Bombay, April*, 20, 1804.

Tropical variation of the barometer.

WHEN I was in London at the conclufion of the year 1801, I had the pleafure of being introduced to you by my friend Mr. Dalrymple, at which time he prefented you with fome fheets of meteorological obfervations, with barometer and thermometer, made by me in India, and during a paffage from India to England.

Being of opinion that few regifters of the barometer are kept at fea, efpecially in low latitudes, I have been induced to continue my obfervations fince I left England, judging that, even if they were found to be of no utility, they might at leaft be entertaining to you or other gentlemen, who have been making obfervations of a fimilar nature.

During my laft voyage I have employed two marine barometers, one made by Troughton, the other by Ramfden,

* Philofophical Tranfactions, 1805.

and

and a thermometer by Frazer. These were placed exposed to a free current of air in a cabin, where the basons of the barometers were 13 feet above the level of the sea. Tropical variation of the barometer.

The hours at which the heights of the barometers, and thermometers were taken, viz. noon, 4 hours, 10 hours, 12 hours, 14 hours, and 19 hours, were chosen, because at these times the mercury in the barometer had been perceived to be regularly stationary between the tropics, by former observations made in India in 1800 and 1801. It was found that in settled weather in the Indian seas, from 8 A M to noon, the mercury in the barometer was generally stationary, and at the point of greatest elevation; after noon it began to fall, and continued falling till 4 afternoon, at which time it arrived at the lowest point of depression. From 4 or 5 P M the mercury rose again, and continued rising till about 9 or 10 P M, at which time it had again acquired its greatest point of elevation, and continued stationary nearly till midnight; after which it began to fall, till at 4 A M it was again as low as it had been at 4 afternoon preceding; but from this time it rose till 7 or 8 o'clock, when it reached the highest point of elevation, and continued stationary till noon.

Thus was the mercury observed to be subject to a regular elevation and depression twice in every 24 hours in settled weather; and the lowest station was observed to be at about 4 o'clock in the morning and evening. I remarked that the mercury never remained long fixed at this low station, but had a regular tendency to rise from thence till towards 8 in the morning and about 9 in the evening, and from those times continued stationary till noon and midnight.

In unsettled blowing weather, especially at Bombay during the rains, these regular ebbings and flowings of the mercury could not be perceived; but a tendency to them was at some times observable when the weather was more settled.

In the sheets, which I formerly presented to you, were evinced these elevations and depressions twice every 24 hours within the tropics, in steady weather, as had been observed by Mess. Cassan and Peyrouse, by Dr. Balfour of Calcutta, and others. But since my last arrival in India, I have observed that the atmosphere appears to produce a different effect on the barometer at *sea* from what it does on *shore*.

Tropical variation of the barometer.

As I am ignorant whether this phenomenon has been noticed by any person before, I will here give you an abstract of my journal, shewing how the barometer has been influenced during the whole time since I left England, which will enable you to form an idea whether I am right in concluding that the barometer is really differently affected at sea from what it is on shore, at those places in India where the observations have been made.

The first sheet begins with the observations made on board ship, in my voyage from London towards Bombay, in the months of April and May, 1802.

From the time of leaving the Land's End, April 19th, the motion of the mercury in barometers was fluctuating and irregular until we were in latitude 26° N, longitude 20° W, on April 29th; the mercury in barometers then became uniform in performing two elevations and two depressions every 24 hours, (which for brevity in mentioning hereafter I will call equatropical motions.) From latitude 26° N to latitude 10° N, the difference of the high and low stations of the mercury in the barometers was not so great, as it was from latitude 10° N across the equator, and from thence to latitude 25° S. Within these last-mentioned limits, the difference of high and low stations of the mercury in the barometers was very considerable, generally from five to nine hundred parts of an inch, both in the daily and nightly motions.

When we reached the latitude of 28° S, longitude 27° W, June 7th, the mercury in barometers no longer adhered to the equatropical motions; but then, as in high north latitudes, its rising and falling became irregular and fluctuating during our run from latitude 28° S, longitude 27° W, (mostly between the parallels of 35° and 36° S,) until we were in latitude 27° S, and longitude 51° E, on the 11th of July. The mercury then began to perform the equatropical motions, and continued them uniformly, during our run from the last-mentioned position, up the Madagascar Archipelago, across the Equator, until our arrival at Bombay July 31st. 1802.

August 6th, 1802. When the barometers were placed on shore in Bombay, the mercury, for the first six days, appeared to have a small tendency towards performing the equatropical motions, but not equally perceptible as when at sea, the difference between the high and low stations of the mercury in the

the barometers being great to the day we entered the harbour of Bombay. From the 12th of August to the 22d the mercury could not in general be obferved to have any inclination to perform the equatropical motions, although at times a very fmall tendency towards performing them might be perceived. Tropical variation of the barometer.

On the 23d of Auguft the barometers were taken from the fhore to the fhip. Immediately on leaving Bombay harbour, Auguft 26th, 1802, the mercury in the barometers performed the equatropical motions, and continued them with great uniformity, during our paffage down the Malabar coaft, acrofs the bay of Bengal, in the Strait of Malacca, and through the China Sea, until our arrival in Canton river on the 4th of October. When in the river, the mercury became nearly ftationary during the 24 hours, except a fmall inclination at times towards the equatropical motions, but they were not near fo perceptible as at fea; this change taking place the day we got into the river.

During our ftay in China, the barometer on fhore, at Canton, had very little tendency towards the equatropical motions, throughout the months of October and November that we remained there. At times, while in China, a fmall inclination towards performing the equatropical motions appeared: but, as in Bombay, the difference of rife and fall was of fo fmall a quantity, as to be frequently imperceptible.

December 2d, 1802. On our departure from Canton river, the equatropical motions were inftantly performed by the mercury, and with great regularity continued during the whole of the paffage to Bombay, until our arrival in that harbour on the 11th of January, 1803.

On January 18th, the barometers were placed on fhore, and did not appear in the fmalleft degree fubject to the equatropical motions; although, with great regularity, they had been performed while at fea, even to the day we entered the harbour. One of the barometers was left on board for a few days, and, like that on fhore, feemed to have no tendency towards the equatropical motions. During the months of February and March, in Bombay, the mercury was nearly ftationary throughout the 24 hours. But about the latter part of March the mercury feemed to incline towards the equatropical mo-

Tropical variation of the barometer.

tions in a very ſmall degree; and, during the month of April, and to the 20th of May, this ſmall tendency of the mercury to perform the motions appeared at times, but was hardly diſcernible, the riſe and fall being of ſo ſmall a quantity. From the 18th of January to the 20th of May, the mercury in the barometers was in general ſtationary, except a very ſmall tendency towards the equatropical motions at times. At other times ſome change in the atmoſphere diſturbed the mercury from its ſtationary poſition; but this was ſeldom the caſe, as it was then the fair weather ſeaſon, or north-eaſt monſoon.

We ſailed from Bombay on the 23d of May, 1803. The inſtant we got out of the harbour, the mercury in the barometers conformed to the equatropical motions with great regularity, and the difference between the high and low ſtations was very conſiderable during the whole of the paſſage to China, excepting a few days in the eaſtern parts of Malacca Strait, where the land lay contiguous on each ſide of us; the difference between the high and low ſtations of the mercury was not then ſo great as in the open ſea. On clearing the Strait, and entering the China Sea, the equatropical motions were performed in greater quantity, and continued regular during our paſſage up the China Sea, until July 2, 1803. We then entered Canton river, and the equatropical motions of the mercury in barometers entirely ceaſed.

From July 8th to September 7th, the barometers were placed on ſhore in Canton, during which time the mercury appeared to have no tendency towards performing the equatropical motions; but it inclined to a ſtationary poſition, except when influenced by changes of weather. After the barometers were taken from Canton to the ſhip, we were four days in getting clear of the river, in which time the mercury inclined to be ſtationary, excepting that a ſmall inclination towards the equatropical motions ſeemed to evince itſelf at times. But no ſooner had we cleared Canton river, September 13, 1803, than the mercury in the barometers began to conform to the equatropical motions, of two elevations and two depreſſions every 24 hours, at equal intervals of time (although we were near the land until the 15th September. And the mercury, with great regularity, continued to per-

form

form the equatropical motions, from September 13, 1803, the day we cleared the river of Canton, until October 13, when we entered Sincapore Strait, excepting a small degree of irregularity, which affected the mercury on the 22d September, when it blew a gale on the coast of Isiompa.

Tropical variation of the barometer.

October 13, 1803. On entering the Strait of Sincapore, which is about 3½ leagues wide, the mercury in the barometers was then a little obstructed, and did not perform the equatropical motions, in the same quantity of rise and fall, as when we were in the China Sea. But on the following day, October 14, when we had passed the narrow part of the Strait, the mercury conformed to those motions with regularity until October 21, when we arrived in the harbour of Prince of Wales's Island; then a great retardation took place in the equatropical motions; for, during the time the ship remained in the harbour, from October 20 to November 5, 1803, the mercury in barometers seemed only in a small degree subject to them, the difference between the high and low stations of the mercury, being in general not more than half the quantity, that takes place in the open sea, or at a considerable distance from land. Where the ship lay at this time in the harbour, the land, on one side, was a full quarter of a mile distant, and on the other side about 1½ mile.

On November 5, being clear of the harbour of Prince of Wales's Island, the equatropical motions were instantly performed by the mercury, in the usual quantity experienced at sea, which continued with uniformity until December 3. On this and the following day, the mercury fell considerably during our passage over the tails of the sands at the entrance of Hoogly river, in latitude 21° 06′ N; and on December 5, the day of the moon's last quarter, a gale of wind commenced from N N E, with much lightning and rain in the night. During the latter part of this day, the mercury began to rise, and there soon followed a change of settled weather. When we were in the lower part of the river, the mercury appeared to conform in a small degree to the equatropical motions; but when well up the river, at Diamond Harbour, the mercury inclined to be nearly stationary during the 24 hours, as has formerly been observed to happen in Canton river, Bombay harbour, &c.

On

Tropical variation of the barometer.

On January 13, 1804, after we had cleared the river Hoogly, the mercury in the barometers began to perform its motions with uniformity, which continued during the passage to Bombay, until our arrival there on February 12. The barometers being then placed on shore, the mercury inclined to a stationary position, without evincing any propensity towards the equatropical motions from the 12th to the 18th February, 1804, as has been noticed in the foregoing description, to happen frequently, on entering a harbour from sea.

On February 18, 1804, the meteorological journal ceases, at which time it comprises the observations of 22 months, having commenced April 6, 1802, in Margate Road.

I have taken the liberty of sending you this abstract from the journal, to exhibit the apparent difference of the mercury in the barometer at sea, from what has been observed on shore, at those places mentioned in the preceding description. As I have not seen any account indicating the phenomenon, I thought it might be interesting to you, or other gentlemen of the Royal Society to forward this imperfect abstract, the journal itself being too cumbersome to send home at present. But as I am in expectation of returning to England by the ships from China next season, I hope I shall be enabled to present you with the meteorological sheets alluded to.

I am, &c.

J. HORSBURGH.

P. S. Since I wrote the foregoing abstract, I have received a letter from my friend Mr. Dalrymple, intimating that a copy of the meteorological journal itself would be acceptable, which has induced me to transmit to him the original sheets, with a request to deliver them to you. I regret that I could not find leisure time to make out a fair copy, to have sent to you, in place of the original sheets in their rough state.

Bombay, June 1, 1804.

Second

V.

Second Communication on Artificial Tan. By CHARLES HATCHETT, *Esq. Abridged from the Philosophical Transactions for* 1805.

§ I.

THE artificial tan*, procured as described in the first communication (*see our Vol. XII. p. 327*), had been named tannin by Mr. Hatchett; but the objection having being made to this, that tannin was destroyed by the nitric acid, while the artificial tanning substance was actually formed by it, induced Mr. Hatchett to expunge the word *tannin* wherever it had been applied to the latter. It also induced the author to make the following experiments on the comparative effects produced by nitric acid on those substances which contain most tannin, and also some others in which a tanning substance has been produced, under circumstances in some respects different from those described.

Name of the artificial tanning substance altered.

§ II.

Although it is not absolutely asserted that the tanning substance is indestructible by nitric acid, yet the following experiments prove, that to produce this effect must at least be the work of much time and difficulty.

1. Twenty grains of the artificial tan were dissolved in half an ounce of strong nitric acid, of the degree of 1.40; the solution distilled till the whole of the acid came over, which acid was returned back on the residuum, and the distillation repeated three times in this manner. Care was taken not to overheat the residuum; and then, when examined, did not appear to have suffered any alteration in its properties.

Experiments to prove that the artificial tan is nearly indestructible by nitric acid.

* In several parts of the abridgement of Mr. Hatchett's papers, the artificial tanning substance has been called *the new tan* and *artificial tan*, and tanning matter *tan*, for the sake of brevity. It was thought necessary to mention this, as the name tan is usually appropriated to oak bark in a certain state; which, with singular impropriety, is that in which it contains least tanning matter, after having been used in the tanners' pits.—ABR.

2. Ten

Experiments continued.

2. Ten grains of the new tan, mixed with ten grains of white ſugar, diſſolved in half an ounce of nitric acid, was diſtilled to dryneſs. The reſiduum was not changed by the gelatinous or other re-agents.

3. This experiment was the ſame as the former, only that gum arabic was employed in place of ſugar. The reſult was the ſame.

4. The precipitate from a ſolution of iſinglaſs, with which the artificial tan had been mixed, was well waſhed with diſtilled water and then dried. In this ſtate it was digeſted in ſtrong nitric acid, by which a dark-brown ſolution was formed; which was evaporated to dryneſs, and the ſubſtance, diſſolved in boiling diſtilled water, was examined by nitrate of iron, acetite of lead, muriate of tin, and ſolution of iſinglaſs, with all of which it threw down copious precipitates, ſimilar in all reſpects to the artificial tan, which had not been ſubjected to the proceſs deſcribed.

5. Some of the precipitate of iſinglaſs by the new tan was diſſolved in muriatic acid, and evaporated to dryneſs: of this boiling diſtilled water diſſolved only a part; and the ſolution, of a dark beer colour, did not precipitate gelatine, though it acted on muriate of tin and ſulphate of iron; for with the former it gave an aſh-coloured precipitate, and with the latter a ſlight depoſit of a reddiſh-brown.

6. As boiling water diſſolved only a part of the iſinglaſs precipitate in the former experiment, the remainder was treated with nitric acid; after which, on being evaporated to dryneſs, it was found to be completely ſoluble in water, and precipitated gelatine as copiouſly as at firſt.

7. Twenty grains of the new tan was diſſolved in half an ounce of muriatic acid: The reſiduum, after evaporation to dryneſs, appeared in every reſpect unchanged.

The author here makes the obſervation, mentioned at the concluſion of the former paper, relative to the ſolutions of the new tan not becoming mouldy like thoſe of galls, ſumach, and catechu, and ſeeming to be completely imputreſcible.

And having thus aſcertained the unchangeable nature of this ſubſtance, he made the following comparative experiments on galls, ſumach, Pegu cutch, kaſcatti, common cutch, and oak bark.

8. Twenty

8. Twenty grains of powdered galls were diſſolved in half an ounce of ſtrong nitric acid: The reſiduum from this ſolution evaporated to dryneſs, and then diſſolved in boiling water, did not produce the ſmalleſt effect on diſſolved gelatine.

Experiments on galls, ſumach, cutch, kaſcatti, Pegu cutch, and oak bark.

The experiments on to No. 13. did not produce any tannin.

9. The reſiduum of a ſtrong infuſion of galls, treated as No. 8.

10. Iſinglaſs precipitated by infuſion of galls, diſſolved in ſtrong nitric acid, and examined as No. 4.

11. Twenty grains of ſumach diſſolved in half an ounce of ſtrong nitric acid, and treated as No. 8.

12. Twenty grains of Pegu cutch (which contains much mucilage) ſubjected to a ſimilar proceſs, by which much oxalic acid was obtained.

13. Twenty grains of catechu, called kaſcatti, treated ſimilarly, had, together with the four foregoing experiments, all the ſame reſults as No. 8, not any of them ſhewing any tannin.

14. Twenty grains of common catechu, diſſolved in ſtrong nitric acid, evaporated to dryneſs, diſſolved in water, and examined by iſinglaſs, depoſited a tenacious film inſoluble in boiling water, evidently compoſed of gelatine and tannin.

15. Twenty grains of oak bark treated in the ſame way, depoſited alſo an inſoluble film on the ſides and bottom of the veſſel.

16. Infuſions of galls, ſumach, and oak wood, of equal ſtrength, were mixed with nitric acid, in the proportion of half an ounce meaſure of each to one drachm of the acid, and did not then render iſinglaſs ſolution turbid.

But infuſions prepared from oak bark and the artificial tan, and managed in the ſame way, continued to precipitate the gelatine, until four drachms of the nitric acid had been added to each half ounce of the infuſion.

Theſe reſults ſhew that artificial tan is the moſt indeſtructible, but that the other tanning ſubſtances have conſiderable varieties in this reſpect. The tannin of oak bark reſiſts nitric acid longer than that of galls, ſumach, kaſcutti, or Pegu cutch. This laſt is replete with mucilage, and yields much oxalic acid, as before deſcribed: it ſeems alſo to be the moſt deſtructible of all the kinds of catechu: From theſe facts the author was induced to add the ſugar and gum to the artificial tan,

tan, to promote its deftructibility; and expreffes his belief that mucilage or gum renders the fubftances that contain it more deftructible in the nitric acid, and in fome cafes alfo prevents or impedes the formation of the tanning fubftance; which difference he thinks to be caufed by the mucilage being in a ftate of chemical combination in thofe bodies.

§ III.

Experiments on the artificial tan.

A and B. When fulphuric or muriatic acid was added to a folution of the new tan, it became turbid and depofited a brown precipitate, which was foluble in boiling water, and was then capable of precipitating gelatine; in which particulars it refembles the tannin of galls and other vegetable fubftances.

C. Carbonate of potafh, added to a folution of the new tan, deepened the colour; the liquor became turbid, and depofited a brown magma.

D. Five grains of dried artificial tan were diffolved in half an ounce of ftrong ammonia: the whole was then evaporated to drynefs; and being diffolved in water was found not to precipitate gelaten, unlefs a fmall portion of muriatic acid was previoufly added.

E. Another portion diffolved in ammonia was diftilled: At firft ammonia came over, and afterwards a yellow liquor, that had the odour of burned horn. The refiduum was infoluble in water, to which it only gave a flight yellow tinge.

On diftillation it has an odour of burned horn, and yields ammonia.

F. The object of this experiment is to fhew the ftrange property of the new tan, of giving products analogous to animal matter (of which it yielded the odour in combuftion on former trials), though prepared itfelf from vegetable fubftances. Some prepared from dry vegetable charcoal was diftilled: Firft a little water came over, then a little nitric acid, then a very fmall portion of a yellowifh liquor: The fire being then raifed, the veffels fuddenly became filled with a white cloud, and fo great a torrent of gas was almoft explofively produced as to overfet the jar: This gas, by its fmell, appeared to be ammonia, and was formed into the cloud by the nitric acid vapour in the veffels. The next jar of gas, which came flowly over, was carbonic acid, except a very fmall part which feemed nitrogen gas. A bulky coal remained, that on incineration gave 1¼ grains afhes, which confifted principally of lime.

G. Fifty

G. Fifty grains of this substance were dissolved in four ounces of water and precipitated by isinglass solution; eighty-one grains of which became thus combined with forty-six of the new tan. The remaining portion was not precipitated, and was therefore separated on a filter and evaporated to dryness. It was a light brittle substance of a pale cinnamon colour, which, though composed of inodorous substances, had however a strong smell itself of oak bark; which is remarked as a singular circumstance; and this smell became stronger when the substance was put into water, in which it instantly dissolved.

The solution was very bitter; acted but slightly on dissolved isinglass; produced a brown precipitate with sulphate of iron, and with muriate of tin a black one; had no effect with nitrate of lime; but with acetite of lime gave a copious precipitate, of a pale brown colour. This substance appeared to be the tanning matter in the state of extract.

§ IV.

Several unsuccessful attempts were made to form the tanning matter by oxi-muriatic acid. It therefore appeared, that though a variety of it could be produced by the action of sulphuric acid on resinous substances, yet nitric acid was the most effective agent.

Attempts to form tanning matter by oxi-muriatic acid unsuccessful.

The author suspecting that the new tan might be formed from bodies not absolutely converted into coal, and not being able to get any touch-wood, which he first thought of trying for this purpose, made the following experiment with indigo, which he knew to contain much carbon.

Artificial tan, it is suspected, might be formed from substances not charred.

One hundred grains of indigo, with one ounce of nitric acid diluted with a double quantity of water, was (when the effervescence had subsided), placed in a sand-bath for several days till evaporated to dryness.

Experiments on indigo with this view.

The residuum, of an orange colour, was in great part dissolved by three ounces of distilled water poured on it, and gave a solution of a deep yellow, and intensely bitter; which, with the sulphate of iron, deposited a slight pale-yellow precipitate, and with nitrate of lime, a small white precipitate, having the character of oxalate of lime: With muriate of tin a copious white precipitate, that changed to a yellowish-brown;

brown; and with acetite of lead a beautiful deep lemon-coloured precipitate, which may probably prove useful as a pigment.

Ammonia rendered the colour much deeper, and with it deposited a large quantity of fine yellow spiculated crystals, which did not precipitate lime from its solutions. Their flavour was very bitter.

It produces tanning matter.

Lastly, when this solution was added to dissolved isinglass, it became turbid, and deposited a tough elastic insoluble film, and possessed the characters of gelaton combined with tanning matter.

Almost all vegetable bodies yield tanning matter, when subjected to repeated distillations with nitric acid.

By this experiment the possibility of producing tanning matter from bodies not converted into coal was fully ascertained; and the author has since discovered that though indigo yields this matter more readily than most other vegetable bodies, yet almost all produce it when subjected to repeated distillations with nitric acid.

Resin yields it by this treatment.

A. The common resin did not produce the tanning substance with nitric acid, but by the aid of sulphuric acid, as before related; yet upon this nitric acid being repeatedly abstracted from it, its solution in water formed a tough yellow insoluble precipitate with dissolved gelaten, similar to that by solution of indigo, and with other re-agents produced the following effects.

With sulphate of iron, after 12 hours, it produced a slight yellow precipitate. With nitrate of lime no effect. With muriate of tin, after 12 hours, a pale brown precipitate. And with acetite of lead a very abundant precipitate of a yellowish white colour.

On repeating this experiment, the author remarked that during each distillation nitrous gas was produced, while the acid which came over was weakened, which made the cause of the change in the properties of the resin evident. The following are the results of experiments tried with other resinous substances.

As do likewise stick lac,

B. Stick lac, treated as described, copiously precipitated gelaten.

—and balsam of Peru,

C. Balsam of Peru during the process afforded some benzoic acid, and gelaten was precipitated by the aqueous solution.

Benzoin

Benzoin, after the sublimation of some benzoic acid, yielded a residuum, which yielded with water a pale yellow solution, of a very bitter flavour. —and benzoin,

This solution with sulphate of iron produced a slight pale yellow precipitate. With nitrate of lime, no effect. With muriate of tin in solution, a small quantity of brownish white precipitate. With acetite of lead, a copious pale yellow precipitate. And with solution of isinglass, a dense pale yellow insoluble precipitate.

E. Balsam of Tolu afforded benzoic acid, and the solution of the residuum precipitated that of gelaten.

F. One hundred grains of dragon's blood in powder mixed with one ounce of nitric acid, evolved much gas; an ounce of water was then added; and the digestion in a sand bath being continued, after it produced chasiccation on the dry yellow mass that remained, a brilliant feather-like sublimate arose, which weighed rather more than six grains and had the aspect, odour, and properties of benzoic acid. —and dragon's blood,

The residuum, of a brown colour, formed with water a gold coloured solution, which was not affected by nitrate of lime: But with sulphate of iron, and with muriate of tin it formed brownish yellow precipitates; and with acetite of lead one of a lemon colour.

Gold was precipitated by it in the metallic state, and the containing glass coloured purple; and with dissolved isinglass it produced a deep yellow insoluble deposit.

As dragon's blood simply exposed to heat did not produce any benzoic acid, the author is inclined to believe, that in the first experiment this acid was obtained as a product, and not as an educt.

G. Gum ammonia gave a brownish yellow, bitter astringent solution; which with sulphate of iron became of a darker colour, but produced no precipitate. —and gum ammonia,

With nitrate of lime, a slight precipitate. With muriate of tin and acetite of lead, copious yellow precipitates; and with gelaten a bright yellow insoluble deposit.

H. Assa fœtida yielded a solution which precipitated gelaten in a similar manner to that described. —and assa fœtida.

I. Solutions of elemi, tacamahac, olibanum, sandarach, copaiba, mastich, myrrh, gambauge, and cacutchonc, although Solutions of elemi, tacamahac, &c. did [illegible] affect gelaten,

though they precipitated the metallic solutions, did not affect gelaten; but possibly might have done so, if the process had been more frequently repeated.

—nor that of sarcocol, —nor of gum Arabic, —nor of tragacanth, —nor of manna.

K. Sarcocol also produced similar results.

L. Gum Arabic afforded oxalic acid but no tan.

M. Tragacanth yielded much of saclatic acid, of oxalic, and of malie acid, but not the least tan.

N. Manna gave oxalic acid, part of which sublimed in the neck of the vessel.

Its residuum formed a brown solution, which produced precipitates of the following colours: With sulphate of iron, a pale yellow; with muriate of tin, a pale brown; with acetite of lead, a brownish white. From nitrate of lime, oxalate of lime was copiously precipitated by it; but with isinglass solution no effect was produced.

Liquorice solution precipitates gelaten.

O. Nitric solution of liquorice yielded precipitates with sulphate of iron and muriate of tin, after twelve hours, slight brown. With acetite of lead, a brownish red. With nitrate of lead, a brown. And with gelaten, one of a yellowish brown, insoluble, and similar to other precipitates from it by tan.

Guiacum solution gave a slight precipitate with gelaten, which was soluble in water.

P. On guiacum nitric acid acted with great vehemence and speedily dissolved it: The residuum was almost totally soluble in water; and this solution produced effects on the metallic salts similar to those recited; but with gelaten formed a slight precipitate, which was speedily dissolved by boiling water. The remainder of the solution evaporated gave a large quantity of crystalized oxalic acid; so that in this respect guiacum was similar to the gums, and unlike the resins.

§ V.

Experiments on several roasted vegetable substances, which do not affect gelaten.

As many vegetable substances when roasted yield a liquor by decoction, resembling solution of artificial tan, the author tried those similarly prepared, of dried peas, horse beans, barley, and wheat flour, none of which gave any precipitate with gelaten.

Coffee gives a precipitate with it, which is again soluble in water.

The decoction of coffee also gave no precipitate till after several hours, and then one soluble in boiling water; but this might be occasioned, the author thinks, from want of some particular nicety which may be required in roasting such bodies

5 so

so as to make them yield tan; which opinion was corroborated by experiments made by decoction of chicoree (probably endive) root, prepared in the same manner, which produced a precipitate with gelaten after some time, though not at first, which was apparently dissolved in boiling water, but deposited again in its original state, on cooling. The author therefore is inclined to believe that the tanning substance is really developed in many vegetable matters by heat alone; but that a certain degree of heat, not easy to determine is absolutely necessary for this effect.

A small quantity of nitric acid added to any of the decoctions just mentioned, and evaporated to dryness, produced a residuum, having all the properties of the tan produced from coal.

Nitric acid added to these decoctions gives tanning properties to their residue

§ VI.

The production of a variety of the tanning substance before mentioned, by the action of sulphuric acid on the resins, amber, &c. suggested the following experiments on camphor; the results of which tend to increase the knowledge of its properties.

Experiments on Camphor with Sulphuric Acid.

The only facts hitherto related relative to the effects of sulphuric acid on camphor, are that a brown or reddish brown solution is formed, from which water precipitates the camphor unchanged; but this only happens at a certain period of the operation; for if it be longer continued, the following effects will be produced.

A. One ounce of concentrated sulphuric acid was added to one hundred grains of camphor, which dissolved gradually, after first becoming yellow; in about an hour, the liquor having progressively changed to reddish brown, brown, and at last blackish brown, much sulphureous acid gas was produced, and continued to encrease during four hours, when the whole appeared a thick black liquid, having no other odour but that of sulphureous acid; after two days the production of the gas was much diminished; the containing alembic was then put in a sand bath, moderately hot, by which more sulphureous gas was obtained; but this soon abated; at the end of two days more, six ounces of water was gradually added, by which the liquor

Experiments with sulphuric acid on camphor

liquor changed to a reddifh-brown, a coagulum of the fame colour fubfided, the odour of fulphureous acid gas was immediately annulled, and was fucceeded by one which much refembled a mixture of oils of lavender and peppermint.

An odour yielded by it like oils of lavender and peppermint.

The whole was then diftilled gradually, when the water came over impregnated with the odour laft mentioned, accompanied by a yellowifh oil, which floated on the top, and was computed to amount to three grains.

B. When the whole of the water had come over, there was again a flight production of the fulphureous acid gas; two ounces of water were then added, and the diftillation continued (without the recurrence of the former odour) till a dry blackifh brown mafs remained; this mafs was well wafhed with warm diftilled water, by which nothing was extracted; but two ounces of alcohol digefted on it for 24 hours formed a very dark brown tincture.

The refiduum was digefted with two ounces more alcohol, and the procefs repeated till the alcohol ceafed to act.

The refiduum had now the appearance of a compact fort of coal in fmall fragments, which were well dried, and after being expofed to a low heat in a clofe veffel, weighed fifty-three grains.

C. From different portions of the alcohol folution, added together and diftilled in a water bath, a blackifh brown fubftance was obtained, which had the appearance of a refin or gum with a flight odour of caromel, and weighed 49 grains.

The products obtained from the 100 grains of camphor treated with fulphuric acid, were,—

Products from camphor and fulphuric acid.

	Grains.
A. An effential oil, having fomewhat of an odour of a mixture of lavender and peppermint, about	3
B. A compact and very hard fort of coal, in fmall fragments, - - - - - -	53
C. A blackifh-brown fubftance, of a refinous appearance, - - - - -	49
	105

The increafe of weight of five grains is attributed partly to water retained by the laft fubftance, and partly to oxigen united to the carbon.

The fubftance C had the following properties:

1. It

1. It was bitter and astringent, had the odour of caromel, and formed with water a dark-brown solution.

2. This solution produced very dark-brown precipitates with sulphate of iron, acetite of lead, muriate of tin, and nitrate of lime.

3. Gold was precipitated by it in the metallic state from its solution.

4. By solution of isinglass the whole was precipitated; so that after four hours a colourless water only remained.

Substance C (from camphor) precipitates gelaten.

This precipitate was nearly black, and was insoluble in boiling water: from whence, and its effects on skin, it was evidently a variety of tanning matter much resembling that obtained from resinous bodies by sulphuric acid.

But this sort of tan had less effect on skin than that procured from carbonaceous substances by nitric acid, and its precipitate from gelaten was more flocculent and less tenacious.

However, when a small quantity of nitric acid was added to the solution of the substance obtained from camphor, and when the residuum, after evaporation to dryness, was dissolved in water, a reddish-brown liquor was formed, which acted in every respect similar to the tanning substance obtained from the varieties of coal by the nitric acid.

§ VII.

The three varieties of artificial tan.

From the experiments related, it appears that three varieties of the tanning substance may be formed.

1st. That produced by nitric acid with any carbonaceous substance, whether vegetable, animal, or mineral.

2d. That formed by distilling nitric acid from common resin, indigo, dragon's blood, and various other substances.

3d. That which common resin, elemi, assafœtida, camphor, &c. yield to alcohol, after they have been previously digested with sulphuric acid.

Remarks on them.

On these products the author makes the following remarks:

The first variety is the most easily formed. From some experiments made purposely it appears, that, after making allowance for a small quantity of moisture and of nitric acid remaining, 100 grains of vegetable charcoal yield 116 of the dry tanning substance.

100 grains charcoal yield 116 tanning matter.

Carbon the base of the tanning matter.

From the manner in which it is produced, carbon is evidently the base and predominating essential ingredient in this substance.

It also contains oxigen, hidrogen, and nitrogen.

From § III. experiment F, it also appears that the other component parts are oxigen, hidrogen, and nitrogen; for when the artificial tan was distilled, ammonia and carbonic acid were obtained, exclusive of a small portion of a yellow liquor that appeared to be of an oily nature, from being insoluble in water and alcohol.

It has an odour like animal matter when burned, and one of oak bark when precipitated as in G, § III.

Many of the properties of the tanning substance prepared from coal by nitric acid are very remarkable, particularly those noticed in § III. experiment F; of its having the odour of animal substances when burned, though prepared from vegetable matter; and in experiment G, of the precipitate having the odour of oak bark, though the component materials were inodorous.

It resembles vegetable tannin in most properties.

But its most extraordinary properties are those in which it so nearly approaches the vegetable *tannin*, which it perfectly resembles in its solubility in water and in alcohol, in its action on gelaten and on skin, in its effects on the metallic solutions, on the alkalis, and on the earths.

Difference between it and tannin.

The sulphuric and muriatic acids also affect its solutions, as they do those of tannin; and the only marked difference between artificial tan and tannin is, that the former is produced by nitric acid, while the varieties of the latter are more or less destroyed by it; but here it must be remembered, that even the varieties of tannin do not accord in the degree of destructibility.

Second variety of artificial tan.

The second species of the tanning substance is obtained from a variety of vegetable bodies before recited, by digesting and distilling them with nitric acid. It is therefore not so readily prepared, and the quantity of it produced is less in proportion to the substance from which it is prepared.

Theory of its formation.

As resin and some other bodies do not afford it until they have been repeatedly treated with nitric acid, and as, during each operation, nitrous gas is produced, while the strength of the acid which comes over is diminished, the author thinks it almost certain that the tanning substance is formed in consequence of part of the oxigen of the nitric acid becoming combined with the hidrogen of the original body, so as to form

water; and the carbon being thus in some measure denuded, is rendered capable of being acted on gradually by the nitric acid, in a manner nearly similar to what takes place when it has been previously converted into coal.

The precipitates of this tanning substance from gelaten are always pale or deep yellow, while those formed by the first species are constantly brown; which induces the author to believe that the different colours of the precipitates depend on the state of the carbon of the tannin.

Quantity of artificial tan less from resins than from coal, accounted for.

The quantity of artificial tan obtained from resin and other bodies, was always less than that from coal, or even from the same bodies previously converted to coal in the humid way by sulphuric acid. The cause of this seems to be, that a number of other products are simultaneously formed with the tanning substance, all of which require more or less carbon as an ingredient; so that, according to the affinities which prevail, some bodies afford but little, and others none of it.

Its proportions from different substances.

The greatest proportion of this substance was yielded by indigo, common resin, and stick lac.

The quantity obtained from assafœtida and gum ammoniac was less.

Benzoic acid when formed counteracts the formation of the tan; and oxalic acid, when produced, prevents it entirely.

Benzoin, balsam of Tolu, balsam of Peru, and dragon's blood, were inferior to the former in this respect; so that the production of benzoic acid seemed to counteract the formation of the tanning substance. But oxalic acid, when formed in any considerable quantity, seemed absolutely to prevent the formation of this substance: for gum arabic, tragacanth, manna, and guiacum, which produced oxalic acid in abundance, yielded no tanning matter.

Common liquorice seems to be an exception; but the author supposes that the small quantity of tan produced by it, was formed by the action of the nitric acid on a portion of uncombined carbon, which being in a state approaching to coal, is probably the cause of the blackness of the common liquorice.

Third variety of artificial tan.

The third variety of the tanning substance appears to be uniformly produced during a certain period of the process; but by a long continuance of the digestion there is reason to think it is destroyed.

Substances, such as gums, which yield much oxalic acid, do not apparently afford any of this tanning matter.

The energy of its action on gelaten and skin is certainly inferior to that of the first variety, into which however it may easily be converted by nitric acid.

From the mode of its formation, there does not appear to be any evidence of its containing nitrogen like the first and second varieties, and perhaps the absence of nitrogen may be the cause of its less powerful action.

Experiments of Messrs. Biggin, Proust, and Davy on tannin, noticed.

In the course of the communications on this subject, Mr. Hatchett notices the experiments on tannin by Mr. Biggin, the great contributions of M. Proust to the elucidation of its nature and properties, and the very great extension of, and valuable additions to the same, from the ingenious labours of Mr. Davy, particularly his discovery of the singular fact that terra japonica, or catechu, consists principally of tannin.

Medicine, arts, &c. may derive great benefit from farther investigations of gums, resins, &c.

The author also greatly recommends the farther investigation of the nature of the gums, resins, balsams, and gum-resins, by every possible method; and is of opinion, that medicine, arts, and manufactures may derive many advantages from it, and the mysterious processes of vegetation probably receive considerable elucidation.

VI.

On carbonised Turf. From a Report made to the Prefect of Police (at Paris) on the Methods employed for reducing it to this State. By MM. CALLIAS *and Co**.

The use of turf very ancient. It produces no deleterious effects.

THE use of turf for domestic fuel is of a very ancient date: Some of the most eminent men of science have pronounced that it does not produce any deleterious effects. Without citing the examples of England (Ireland), Scotland, and Holland, where great quantities of it are consumed, we will confine ourselves to the use made of it in France, in the *(ci-devant)* provinces of Flanders, Artois, and Picardy.

Its use is now tolerated in Paris to relieve the scarcity of wood: the lime-burners, plaster-bakers, brick-makers, and washers, make great use of it both in the city and its vicinity; and it has never been perceived that those who lived within

* Sonini's Journal, Tom. II. p. 324.

the

the influence of its smoke, have experienced any bad effects from it. The commissioners (employed to make this report) observe, that the great volume of smoke which is disengaged on the commencement of its combustion, is only caused by a great portion of water contained by the turf, which is expanded into steam by the heat; soon afterwards this smoke is combined with an acid analogous to that of vegetable substances, which, far from making the air deleterious, tends on the contrary to neutralize the vapours of infection which it may contain. It is true that sometimes, for an instant, the turf in combustion exhaled an odour of empyreumatic oil, in the form of gaseous vapours, but this odour is by no means injurious to the animal organisation, but, on the contrary, is beneficial in nervous affections.

Its smoke is caused by contained water; with an acid vapour, which removes infection.

But if this odour is disagreeable when the turf is burned in towns, villages, and private houses, this complaint cannot take place when it is burned in the open air at a distance from all habitations, which will be effected by its previous carbonisation, as managed by MM. Callias and Co. therefore the company merit the public protection.

Charing prevents all the unpleasant effects of its odour.

In 1785 the French Government took a great interest in what related to the carbonisation of turf, and granted 80,000 francs to a company to erect a furnace for this purpose on the ground of the Capuchins. The method of this company was that of extinguishing, but their plan did not succeed, and the works were abandoned.

Company at Paris for charing turf, assisted by the government, did not succeed.

A new company tried, some time after, the same enterprize, at its own expence: the method of operating in closed vessels was proposed; the experiments made were on a great scale, and were attended with a success that was certified by the commissioners of government; a memoir printed in 1790, by M. Morclot, contained these facts, with a statement of the superiority of turf-charcoal over that of wood. But the disastrous events of the revolution put an unhappy end to this enterprize which promised so well.

Another company—promised well—but the revolution caused its failure.

At present MM. Callias and Co. offer to the public an additional species of fuel to that hitherto in use, a charcoal of a new process, the materials of which are spread with profusion over the territory of France, and the consumption of which, being substituted for that of wood, will at the same time be an object of economy to individuals, and of incalculable advantage

Callias and Co. charr turf by a new process.

The use of turf charcoal. It will be cheap,

and prevent the destruction of the forests.

vantage to the management of the forests. Timber for the construction of houses and furniture, and timber for ship-building, daily increase in price, because they become more scarce. Some of the forests have become reduced, as the fresh growths in them do not keep pace with the destructive instrument that overturns them; some of them are entirely destroyed, and the ground converted into ornamental gardens; and thus each year, each month, each day, conducts us insensibly to a most alarming dearth of timber. Already the price of fire-wood is tripled, and Paris is on the eve of being deprived of a combustible which, as yet, has not been replaced to advantage?

The carbonization used by Callias is very perfect.

The commissioners compliment MM. Callias and Co. whose method of carbonisation is peculiar to themselves, and calculated conformably to the laws of combustion in its two first stages; that is to say, before the arrival of its third degree, or that of absolute combustion. MM. Callias and Co. by their method, direct the carbonisation at their pleasure and in an invariable manner; they are always sure of obtaining a perfect charcoal, without smoking-pieces, and without any risk of forming it into a pyrophorus, which sometimes happens in the carbonisation performed in closed vessels. Their manner of proceeding is also very economical; and what proves that they work with intelligence is, that they daily improve, and already are able to save ten hours out of 48 in each carbonisation.

Experiments made with Charcoal of Turf.

Turf-charcoal yields more heat than wood-charcoal.

1st. The charcoal of turf kindles a little slower than that of wood, but when it is once in compleat ignition, it throws out much more heat; its flame is also more elevated, and it yields no odour, except a very slight one of sulphur, which ceases when it is fully lighted.

Causes water to boil four times as speedily.

2. Charcoal of turf, in equal quantity with charcoal of wood, caused a given quantity of water to boil four times; while that of wood caused it only to boil once. The first is then superior to the second in a quadruple proportion.

3. To prove that turf charcoal emits more heat than wood charcoal, the following experiment was made.

It fused 11 oz. of gold in eight minutes; wood-charcoal did the same in sixteen

With turf charcoal, in a goldsmith's furnace, eleven ounces of gold were fused in eight minutes, which with wood charcoal was not performed in less than sixteen minutes. The gold lost nothing of its malleability in the fusion with the turf; but,

on

on the contrary, it was neceſſary to add ſome reductive flux, to that fuſed by the wood charcoal, in order to reſtore the malleability which it had loſt.

minutes. The malleability of the gold was preſerved.

4. Iron made red-hot by charcoal of turf in a forge, became more malleable; which proved that it gave none of its carbon to the metals with which it came in contact.

Iron heated by it becomes more malleable. It laſts longer than wood-charcoal.

5. Finally, Turf charcoal laſts longer in a ſtate of ignition than charcoal of wood, and its heat is conſtantly equal.

Concluſion.

1. The odour of turf in combuſtion is noways deleterious. This truth has been confirmed by the moſt diſtinguiſhed chemiſts; and is beſides proved by the conſtant uſe made of this fuel in the *ci-devant* provinces of Flanders, Artois, and Picardy.

Its odour not at all unwholeſome.

2. It is deſirable that the carboniſation of turf may be encouraged, on account of the great advantages which may reſult from the uſe of this new ſpecies of charcoal, both for private conſumption and for large works.

Its uſe ought to be encouraged,

But the greateſt matter in its favour is, that its uſe tends to diminiſh the felling of the foreſts, whoſe extenſion ought to be promoted by every means poſſible, and which nothing tends ſo much to deſtroy as the uſe of wood charcoal.

and will preſerve the woods.

VII.

Account of the Cataracts and Canal of Troellhætta, in Sweden, (from a Work relative to them by Colonel SKIOELDEBRAND. *Publiſhed in one Volume Quarto, at Stockholm.)*

THE cataracts of Troellhætta produce one of the fineſt effects which nature affords in Europe. The river of Gothie is the only outlet of the vaſt lake of Wener, navigable through its whole extent. This river, which falls in the North ſea near Gothemberg, as ſoon as it departs from the lake, which is much more elevated than the ſea, rolls its waters with impetuoſity, and daſhes them againſt ſteep rocks, whoſe reſiſtance forms a ſucceſſion of cataracts, which without being individually very high, form altogether a moſt ſtriking object. The imagination is the more affected by this ſight, as the ſurrounding ſcenes are of a dark and melancholy character, conſiſting of grey rocks crowned

The cataracts are formed by the river Gothie ſoon after its departure from lake Wener.

They form a very ſtriking object.

Ruins of locks which had been formerly constructed in the bed of the cataracts.

crowned with ancient firs, and of frightful precipices, formed by the bursting of the locks and banks, which the fury of the water has overturned. These last were constructed in the bed of the cataracts, in order to render the river navigable through its whole length; but this daring work of man could not resist the reiterated efforts of nature, and therefore it was necessary to have recourse to another plan.

The canal passes by the side of the cataracts. Is partly cut in the rock. Its breadth, depth, number of locks, time of excavation, and cost. The extent of its navigation, and number of vessels which have passed.

The canal newly constructed passes by the side of the cataracts, and its bed is partly formed in the natural rock, and partly in a marshy soil. It was began in 1794, and finished at the end of six years, in 1800. Its breadth is 22 feet, and its depth six feet and an half. Its locks are eight in number, and its cost amounted to the sum of 59833 pounds sterling, which was collected by subscription. By means of this canal there is a continued navigation, without any interruption, from the province of Wermeland to Gothemberg. In 1802 the number of vessels which had passed this canal amounted to 1360, which is at the rate of 1190 each year.

VIII.

Letter from H. B. K. *on the Production of Nitrous Acid, and other Facts**.

To Mr. NICHOLSON.

SIR,

Experiments announced. Carbonate of potash in water was galvanized, and emitted carbonic gas.

AS Mr. Accum has not answered my paper, he therefore knows of no experiments which shew the formation of the nitrous acid; but anxiously impressed with the subject, I have been performing some experiments, which I think will throw great light upon the cause of the nitrous acid appearing in electrical experiments.

The potash became capable of deflagration like nitre; and solution of silver shewed the presence of muriatic acid.

I passed the galvanic fluid through a watery solution of the carbonate of potash, made by distilled water, confined in a glass tube where no atmospherical air could have access to it, and I found a great production of air come from the solution, which upon examination was pure carbonated air; and

* See our Journal, X. 105, 214, and XI. 105.

then

then examining the solution by dipping a piece of paper into it; upon its being dipped the paper shewed evident signs of nitre upon it, and when burned it detonated the same as nitre would have done; and also with the solution of silver the alkaline solution gave some faint indications of the marine acid being present in it. That the solution should give indications of possessing both the nitrous and marine acids is not so surprizing; as we have the same products in firing oxigen and hidrogen gases, according to the foreign experiments, principally the nitrous acid, but with it a small quantity of the marine acid.

Remark. These two acids are found on the common detonation of oxigen and hidrogen.

I then filled the tube (after washing it clean) with pure distilled water, and sent through it the galvanic fluid; and I observed a generation of airs, which, upon examination, appeared to be the airs usually formed in these experiments, as they exploded.

Pure water galvanized gave these gases.

After this I filled the tube with a solution of pure potash in distilled water, and no air came from it upon galvanising it; if any, it was carbonated air: But upon examining the solution, it gave clearly the same indications of possessing the nitrous and marine acids, as the carbonated solution of potash did in the first experiment.

Pure potash and water gave no gas by galvanising, but indicated the two acids of the first experiment.

Now exempt from all hypotheses, let us examine these interesting facts: The carbonated potash had its carbonic acid air expelled, clearly from the acid or acids; as we know that it could not part with its carbonic acid air, but from the action of a stronger acid. Also another more essential fact it proves to us, that the generation of the peculiar airs, what are called oxigen and hidrogen gases, are owing to the acids; for when the potash was in the water as to arrest or attract them, there were neither of these airs produced; and upon examining the distilled water (in the experiment in which they were produced) after their production, there was no acid in the water, but it was pure distilled water. Therefore, beyond a doubt, the nitrous acid is essentially concerned in the production of these peculiar oxigen and hidrogen gases: indeed Mr. Cruickshanks says, that upon fusing these airs, he found in the residuum the nitrous acid.

Remarks. The carbonic acid was expelled from potash by a stronger acid produced. Whence it is inferred that the oxigen and hidrogen arose from these acids.

These experiments were performed by two short gold wires attached to each end of the galvanic pile. But upon placing a pretty long iron wire to the silver end of the pile instead of the

The conducting wires were gold.

the gold one, a little hydrogen gas was produced; even when the potash was mixed with the distilled water, though there was none when it was a gold wire.

I hope Mr. Nicholson you will not refuse the insertion of these interesting facts in your Journal, for I have made the relation of them as brief as possible that they might not occupy too much room *.

H. B. K.

London, August 15.

IX.

Report of M. DEBUC's *Memoir on Acetic Acid, made by M. M.* PLANCHE *and* BOULLAY, *by Order of the Society of Pharmacy at Paris* †.

M. Debuc repeated M. Badollier's process to obtain acetic acid from acetate of lead by sulphate of copper.

M. Debuc saw in the *Annales de Chimie*, No. 109, a method of M. Badollier, apothecary at Chartres, for obtaining acetic acid very readily from a mixture of equal parts of sulphate of copper and acetate of lead by a moderate heat.

Relying on this process M. Debuc made an exact mixture of two pounds of sulphate of copper and an equal quantity of acetate of lead, which he exposed in a distilling apparatus on a sand bath to a moderate fire, which he increased by degrees during the operation, which lasted for six hours: the product obtained was 26 ounces. It was given to a manufacturer, without examination, and being used in his business produced an effect entirely contrary to the acid extracted from crystals of copper.

The product used in manufacture produces an effect contrary to the common acetic acid.

This circumstance determined M. Debuc to repeat the process as before, and examine its results carefully: In which he observed the mixture of the two salts to become pastey, which is easily explained by the difference of the concentration of the acid in the sulphate of copper, from that in the sulphate of lead. The products of this experiment were,

The process again repeated, and its results carefully examined.

* I am extremely sorry that this communication was by mistake placed among papers already printed; which alone has caused the delay in its appearance.—W. N.

† Annales de Chimie, Tom. LIV. p. 145.

1. Four

1. Four ounces of water slightly acidulated. Products of this process.

2. Four ounces of a liquor more acid than the first, and which M. Debuc compares to good vinegar of *Saumür*.

3. Eighteen ounces of a very limpid liquor, with a lively and penetrating odour of acetic acid mixed with sulphurous acid.

The residue, weighing 38 ounces, appeared to M. Debuc in different layers more or less red, according to their distance from the bottom of the retort; and he found the upper part covered with a whiteish powder, slightly inclined to a citron colour, in which he recognised the presence of sulphur. The residue is in layers of different colours.

Barytes, the muriate of lime, and the acetate of lead formed immediately considerable precipitates with the third product. Precipitates formed with the third product by different salts.

M. Debuc observes that the decomposition of the acetate of lead by the sulphate of copper may be easily explained; but that here there is a production of sulphurous acid, and a decomposition of the sulphuric acid from the absorption of its oxigen by the vinegar; which is a singular phenomenon, that has no agreement with the affinities of the acidifying principle for the *acidifiable* and *salifiable* bases; he leaves the explanation of this matter to more experienced chemists, and only notices that the transportation of the oxigen of the sulphuric acid to another base, suggests the idea, *that acetic acid is superoxigenated vinegar*. Sulphurous acid being produced, induces M. Debuc to suppose that acetic acid is super-oxigenated vinegar.

M. Debuc succeeded in freeing his third product from the sulphurous and sulphuric acids, by letting it remain for about 24 hours, on twelve grains of salt of tartar, and about two ounces of black oxide of manganese pounded fine, and after that distilling it slowly; by this rectification he obtained a pound of pure acetic acid of a lively and agreeable odour, and of about 10 degrees specific gravity; which is one degree less than that of radical vinegar well rectified, obtained from acetate of copper. M. Debuc's method of freeing the third product from sulphurous and sulphuric acids. Acetic acid produced, one degree weaker than the common kind.

The author concludes from this,

1. That the product of two pounds of acetate of lead, treated with an equal quantity of sulphate of copper, is twenty-six ounces; of which four ounces is acidulated water, an equal portion strong vinegar, and eighteen ounces acetic acid altered by the sulphurous and sulphuric acids.

2. That the eighteen ounces, forming the third product, rectified as recited, does not differ from that drawn from crystals of acetate of copper, but by its less density. M. Debuc concludes that the acetic acid produced only dif-

3. That

fers in strength from the common kind, and may be substituted for oximuriatic acid, in some cases to advantage.

3. That in many cases this acid may be substituted for oximuriatic acid, as an object of salubriety without possessing its inconveniencies.

The reporters repeat M. Badollier's process, with M. Debuc's modifications.

The reporters repeated the process of M. Badollier with the modifications advised by M. Debuc as follows.

They introduced a mixture of two pounds of sulphate of copper, and the same quantity of acetate of lead into a glass retort, placed it on a sand bath, and adjusted to it a tubulated receiver, which communicated with two bottles of a Wolf's apparatus; the first of which contained distilled water, and the second many pounds of lime water; from this last a tube was passed underneath a jar in an hydro-pneumatic apparatus: the retort was heated gradually to the end of the operation, which lasted more than 10 hours; and the following products were drawn from the receiver.

Products of their process from the receiver.

1. Eight ounces of a liquor similar to distilled vinegar, but with a less agreeable odour.

2. Ten ounces of a liquor with an unpleasing odour of acetic acid, more penetrating than the first, and not containing any trace of sulphurous or sulphuric acids.

3. Finally seven ounces of a liquor of great limpidity, with a very pungent odour of sulphurous acetic acid, and which did not precipitate muriate of barytes.

A considerable disengagement of an elastic fluid was observed, which became perceptible as soon as the retort began to run, and which lasted during the whole operation.

Carbonic acid gas evolved.

This gaseous fluid was absorbed almost totally by the lime water, forming with it a very abundant white precipitate, which, gathered on a filter, and dried, proved to be carbonate of lime: It weighed two hundred and fifty grains, which made the carbonic acid equal, according to the known proportions of this substance, to eighty-five grains; atmospheric acid alone passed under the jar mixed with some carbonic acid gas: no trace was perceived of hydrogen gas.

Many layers of different colours were found in the retort.

The first was of a beautiful green, surrounded with a circle of yellowish white towards the sides.

The second, much more thick, was of a red colour, greatly like copper in very small particles.

The third was a mixture of sulphate of lead and of copper apparently in the metallic state.

The

The last larger, which occupied the bottom, of a black colour, and shining, was a mixture of sulphate of lead and of charcoal.

The lowest layer of the residue is a mixture of sulphate of lead and charcoal.

The same experiment with the same quantities of the salts, was repeated a second time, with the precaution of reducing the sulphate of copper by disiccation to $\frac{11}{16}$ of its weight. The product from this was preferable to the other.

The same process again repeated; drying the sulphate of copper, the produce is better.

The second and third products were mixed and rectified on carbonate of potash and oxide of manganese, with the precautions indicated by M. Debuc: This rectification produced an acetic acid of nearly the same specific gravity as that afforded by simple distillation from crystals of copper, but of a less strong odour, less agreeable, and besides mingled with sulphurous acid.

Second and third products rectified.

A weaker and less agreeable acetic acid produced, mingled with sulphurous acid.

The reporters think that M. Debuc is deceived in his theory, "*that acetic acid is vinegar super-oxigenated by the oxigen of the sulphuric acid passing to the vegetable acid,*" for he has not considered,

1. That the acetic acid is almost all obtained, before the sulphurous acid becomes perceptible.

Reasons why M. Debuc's theory is erroneous.

2. That the metallic oxides, which are the basis of the salts employed, have less attraction than sulphur to oxigen.

3. That the disengagement of the carbonic acid is much more likely to explain the matter.

The considerable production of carbonic acid, and the presence of charcoal in the residue, surprised the reporters the more; as *MM. Boddolier and Darac* (the first in his notice of the preparation of acetic acid; the other in a memoir, in other respects very interesting, on the difference of acetous and acetic acids,) positively assert that in the operation related, *there was no other gaseous production but that of part of the air contained in the vessels, especially no carbonic acid, and not an atom of charcoal in the residue.*

The production of carbonic acid and charcoal from the process, is contrary to the assertions of MM. Bodolier and Darac.

The result found by the reporters so different from that of *M. Darac*, in an experiment on which he supports his theory of the identity of the acetous and acetic acids, was so favourable to the theory of *M. Chaptal*, that they would have been induced to decide in favour of the opinion of the latter, if the following comparative experiments had not confirmed them in a contrary notion, and appeared to them one of those, of which M. Darac might most avail himself.

To

An experiment made, favourable to M. Darac's opinion.

To four ounces of pure concentrated radical vinegar (extracted from crystals of copper by heat alone) were added by degrees four ounces of semi-vitreous oxide of lead (litharge) in powder; which compleatly dissolved in it by heat, there even remained an excess of acid, perceptible in the strong odour of the solution. Being laid by to cool, it produced a very irregular crystalline mass.

Four ounces of this mass of acetic lead, mixed with an equal quantity of sulphate of copper dried, were treated in a convenient apparatus. The acetic acid produced had an odour more penetrating and agreeable: but all the other phenomena were the same as with the acetate of lead; that is to say, there was an equal developement of carbonic and sulphurous acids, and charcoal was found in the residue.

Which determined the reporters to conclude,

The reporters conclude that this acetic acid is always mixed with sulphurous acid; of which it cannot be freed entirely by M. Debuc's process; and never has so pleasing an odour as the common kind.

1. That acetic acid formed by the distillation of a mixture of sulphate of copper and acetate of lead, is always mixed with sulphurous acid, which does not become perceptible till towards the end of the distillation.

2. That it cannot be compleatly deprived of this sulphurous acid by the rectification proposed by M. Debuc.

3. That the acid itself, totally deprived of the sulphurous acid, is never of so lively and agreeable an odour, as that drawn from the crystals of the acetate of copper.

4. That it is preferable to dry the sulphate of copper before it is used.

5. That MM. Boddolier and Darac, were mistaken in supposing, that no carbonic acid was obtained in this operation.

The difference of acetous and acetic acids probably does not depend on the state of their acidification.

6. Finally that the production of carbonic acid does not any more prove the decarbonisation of the acetous acid in becoming the acetic, than the sulphurous acid proves the superoxigenation of the vinegar; but on the contrary that it is allowable to conclude, that the difference of these two substances is not caused by their state of acidification.

Account

X.

*Account of the Imperial Botanic Garden of Schœnbrunn, in the Vicinity of Vienna.**

IN 1753 the emperor, Francis the first, caused a portion of ground behind the garden of the castle of Schœnbrunn to be prepared for the cultivation of exotics, and of plants remarkable for their rarity or beauty. By the advice of the celebrated Van Swieten, the famous florist Adrien Steckhoven was invited to Schœnbrunn from Leyden, who caused many greenhouses to be constructed there, with a very large and beautiful hot-house, and various other buildings. At the same time Richard Vander Schot, of Delft, was named first gardener, and employed to convey to Vienna a great number of rare and exotic plants, brought up in different parts of Holland, and thus at the end of one year the garden was already rich in valuable plants.

The garden established in 1753, by Francis I.

Put under the care of Adrien Steckhoven, —Vander Schot the first gardener —brings to it many exotics from Holland.

M. Jacquin, who was then at Vienna, went to visit the garden of Schœnbrunn, to class those plants which had not yet received a specific denomination; on which occasion he became known to the emperor, who proposed to him to travel at his expence on the continent of South America, and in the American islands, to enrich the garden with plants from the most distant countries. Accompanied by the gardener Van der Schot, he departed from Vienna in 1754; and in passing through Italy was joined by Jean Buonamici and Ferdinand Barculli, who were entrusted with the zoological part of the expedition, by which it was proposed to improve the royal menagerie, and the cabinet of natural history at the same time. After having visited the islands of Martinico, of Grenada, St. Vincent, St. Eustatia, St. Christopher, St. Martin, St. Bartholomew, Aruba, Cuba, Caracca, and Jamaica, he returned to Vienna in 1759. From August 1757 to the middle of 1759, M. Jacquin could do little for the advancement of Science, having been ill of a lientery for four months, of which he was at last cured at Jamaica. The war which then commenced between England and France, also deranged his travels.

M. Jacquin sent to America to collect plants.

Account of his proceedings in the West India islands.

* Magasin Encyclopedique, T. 6, p. 552.

vels. The vessel in which he made his voyage was taken, and he was thus obliged to pass a considerable time, against his will, at Montferrat and the desert island of Gonave.

The first cargo of plants shipped, —the second cargo brought by Vander Schot.

In the month of August 1757 the first cargo of plants for the garden of Schœbrunn was shipped from Martinico, which arrived at Marseilles. In the month of February, 1757, Vander Schot returned also from Martinico, and brought with him from the same island a great quantity of trees and shrubs. All this cargo arrived safe, except some specimens of *Aleliconia*, which were attacked on the voyage by mice. The trees were of the height of a man, and of the thickness of an arm, and sometimes more. The most of them had born fruit in their native soil; their tops had been cut off, and only some of the principal branches were permitted to remain about two feet in length; the shrubs remained in their natural state. To remove

Method of preparing the trees for carriage, —and of packing them.

those trees from their native earth, a circular trench was dug round each, at a convenient distance, in such manner that there might remain attached to their roots as great a mass of the earth in which they grew as was possible. This mass, which formed a sort of ball, was entirely wrapped up in leaves of the *Musa*, secured with cords made of the bark of the *hibiscus tiliaceus*, in such a manner that the earth could not fall out.

Weight of a tree when packed 100lb.

A single tree packed in this manner, weighed commonly an hundred and odd pounds. The balls of earth were moistened a little, with the necessary care, and suspended in the air, where the vegetation soon became apparent.

Method of transporting the packages.

To prevent the earth from being detached from the roots on the way, all the packages were transported in barks to the port of St. Pierre, in Martinico; from hence they were shipped to Marseilles, and from thence brought by sea also to Leghorn, and from this port were carried by mules to Schœnbrunn. This was without exception the richest cargo of living plants which had ever been brought from the hot countries to Europe.

The third cargo of plants shipped, —the fourth, and the fifth.

In the month of August, 1756, Buonamici set off with the third cargo from St. Eustatia to Leghorn. The fourth cargo departed towards the end of the same year. The fifth was shipped from Curacao for Amsterdam, and was accompanied by J. A. Vesuntin, who died in Germany of the dysentery. This cargo was extremely rich in corals and other products of the sea, which still form some of the most precious ornam

of the Imperial Cabinet. In the same year, M. Jacquin sent off the sixth cargo, from the same island to Amsterdam. And finally, in January 1759, MM. Jacquin and Barculli departed with the seventh cargo from the Havannah, for Ferrol in Spain, and arrived at Vienna in the month of July. This last cargo was particularly rich in animals of every species.

The sixth cargo shipped, —the seventh brought by MM. Jacquin and Barculli, 1759.

Thus in the space of a few years the number of plants in the garden of Schœnbrunn was considerably encreased; for, besides those which had arrived from America, means were found to make many important acquisitions in different other countries. In 1765, after the death of Francis the first, Maria Theresa ordered the garden to be conducted on the same footing that it was before. In 1780, a little while before the death of this princess, it suffered a small but irreparable loss; the gardener, Van der Schot, then very aged, had been confined to his chamber for many weeks by an attack of the gout. Those to whom the management of the plants was entrusted in that period acquitted themselves with great negligence; in one of the coldest nights of that winter, the person who should have taken care of the great hot-house forgot to keep up the fire. In the morning he thought to repair this neglect by heating it to an unusual degree; but the sudden transition from cold to heat killed a great number of fine plants, and among others all the cinnamon trees from Martinico, of which the trunks were as thick as a man's arm, the heads very large, and of the greatest beauty; and also destroyed the plants *Crescentia*, *Achras*, *Annona*, *Portlandia*, and a *Coccoloba Grandifolia*, which was 20 feet high, and whose leaves were of the size of two feet.

The garden receives plants from other countries.

Maria Theresa patronises the institution.

Many valuable plants destroyed by the negligence of the attendants with regard to the hot house.

This garden also suffered another loss. A considerable collection sent from the isle of France by M. Gere, arrived at Trieste entirely spoiled, the trees all dead, and the seeds improlific.

A cargo of plants from the Isle of France spoiled.

At this time the emperor, Joseph the second, directed M. Jacquin and M. Born to propose to some men of abilities to undertake a voyage into remote countries. Professor Marter was appointed the conductor of this expedition, and Doctor Stupics was associated with him, together with the gardeners Boor and Bredemeyer, and the painter Moll. This company of travellers quitted Vienna in the month of April, 1783, and arrived in September at Philadelphia. They travelled over

Prof. Marter and others sent to collect in America in 1783.

 Pennsylvania,

Penſylvania, Virginia and Carolina. M. Boor along with M. Schopf, made a journey into Florida, and from thence paſſed to the iſland of Providence. M. Bredemeyer returned from Carolina, and paſſing through England, arrived at Vienna in November, 1784, with ſeveral very beautiful plants. Boor, who during his ſtay at the Bahama iſlands had collected many rare plants, returned to Vienna in the month of September, 1785. But the painter Moll and Doctor Stupiez were ſeparated from their fellow travellers.

Several fine plants arrive at Vienna from thence, with M. Bredemeyer.

By the orders of the emperor, M. Bredemeyer, and the gardener Schucht, went towards the end of the year 1784 to rejoin the director of this expedition, M. Marter, who remained all this time in America; they paſſed over many of the great iſlands and a part of the continent as far as the mouth of the river Oronoco.

Bredemeyer ſent out again, —ſearches the iſlands and continent to the river Oronoko.

In 1788 they returned by way of Amſterdam to Vienna, and brought back many rare and new plants. M. Marter alſo arrived the ſame year, by the way of London and Bruſſels with a new collection of plants.

Many rare plants brought back by him and M. Marter in 1788.

The emperor had not forgotten the loſs of the plants from the iſle of France, and commiſſioned M. Boor and the gardener Scholl to go there, and touch on their way at the Cape of Good Hope. In the month of May 1786, they arrived at the Cape with the Dutch veſſels, M. Boor remained there till 1787, and then departed by himſelf for the iſle of France and that of Bourbon. In the month of January, 1788, he returned to the Cape with 250 caſes full of rare plants; and on the 20th of July in the ſame year arrived at Vienna with a great number of magnificent vegetables; but as all the caſes could not be brought in the veſſel, the gardener remained at the Cape with the remainder. There has not ſince been any poſſibility of getting them to Vienna, as well as many other plants; and the gardener Scholl remains at the Cape from that time, from whence he has ſent from time to time ſeeds and roots. Beſides this increaſe to the garden, the number of plants was augmented in different manners. Thus, at the ſale of the garden of Schwenk at the Hague, the emperor cauſed all the rare plants to be bought; and likewiſe M. Jacquin, the ſon, when he was on his travels over a great part of Europe, ſent to Schœnbrunn many exotic plants which he found in other gardens.

M. Boor and Scholl ſent to the Iſle of France.

M. Boor returns with many fine plants, —leaves ſome behind at the Cape with Scholl.

The plants of Schwenk bought.

M. Jacquin, the ſon, ſends many exotics from his travels.

The

The emperor Joseph alſo enlarged the hot-houſes, and cauſed others to be built. In order to bring back Scholl the gardener to Vienna, with the plants which remained in his care at the Cape, the emperor Leopold, in 1791, ordered the gardener Bredemeyer and young Van der Schot, (the ſon of him who had been with M. Jacquin in the Eaſt Indies) to ſail to the Iſle of France, where Cere had collected many plants for the imperial garden, and during their return they were to touch at the Cape to take up all thoſe which remained with Scholl. The captain of the veſſel, in which the two gardeners had taken their paſſage put into Malaga; where they diſcovered in time that he had bad intentions with reſpect to them; which obliged them to return to Vienna without performing their commiſſion. After the death of the emperor Leopold, his ſucceſſor Francis the ſecond had an hot-houſe conſtructed, 235 feet long, for the plants of the Cape. A new garden was alſo eſtabliſhed, of which Doctor Hoſt was appointed inſpector, and in which were carefully cultivated all the plants which grew in the ſtates belonging to the houſe of Auſtria.

Emperor Joſeph enlarges the hot houſes and builds others.

Emperor Leopold orders Bredemeyer to the Cape for Scholl,—he is diſappointed of his paſſage.

Francis II. builds an hot houſe 255 feet long for Cape plants.

A new garden added for plants of the Auſtrian ſtates.

By theſe details may be ſeen with what care this juſtly celebrated garden was augmented from the reign of Francis the firſt, and all aſtoniſhment will ceaſe at the riches it contains, and which have furniſhed materials for different magnificent works on Botany, ſuch as the *Icones plantarum rariorum*, publiſhed by M. Jacquin, and above all, that which appeared a few years ago, under the title of *Plantarum rariorum Horti Cæſarei Schœnbrunnenſis deſcriptiones et Icones*, in two volumes folio, containing 150 coloured engravings.

Valuable Botanic publications from the garden of Schœnbrunn.

XI.

Letter from a Correſpondent on the Means of increaſing the Action of Sound on the Organs of ſuch as are partially deaf.

To Mr. NICHOLSON.

SIR

ALTHOUGH I am ſo deaf as not to be able to hear the beating of a watch, unleſs it be put cloſe to the ear, yet, if I place one end of a ſtick, or of a metal rod between my teeth and

Sounds imperfectly heard rendered audible through a ſolid applied to the teeth.

and the other end upon the watch, at the diftance of feveral feet, I can hear it very diftinctly.

The hearing confiderably affifted by preffing the external ear forward;

I know only two methods of alleviating the difficulty of hearing articulated founds; one is by furrounding the ear, with the hand open, and preffing it forward, the fingers and thumb being fixed behind it; this expedient does more than might be fuppofed. Another method is, that of the application of a trumpet; which however, is of but little ufe, conftructed as it is at prefent. The difcovery of any inftrument to facilitate hearing, by being placed in the mouth (probably after the manner of a tobacco-pipe) would be of great importance to a numerous clafs of our fellow creatures, whofe faculty of hearing is nearly fufficient for common converfation. If an inftrument fhould be invented, which will do any thing at all in this way, our experience in regard to other inventions, encourages an expectation, that improvements will follow: means of affifting human fight have long been devifed; little indeed has been done to affift defective hearing; it is however an object deferving of more attention than has been beftowed upon it. If you fhould be fo good as to infert this in your Journal, I indulge a hope, that fome of your ingenious correfpondents, compaffionating the unfortunate fituation of thofe whofe hearing is imperfect, may be led to attempt difcoveries, the refult of which may be of extenfive utility. It is defirable to afcertain the beft form and fize of an ear trumpet, and what metal is to be preferred.

Or by a trumpet, though not confiderably ufeful.

Probability that an inftrument might be invented to act on the teeth or bones of the head fo as to magnify founds.

I am, Sir,

Your moft obedient Servant,

A. B.

Reference to fome difquifitions on founds in the quarto feries of this Journal.

P. S. On referring to my quarto edition of the Journal, vol. IV. page 383, I find fomething correfponding to my own obfervation. I fhall be extremely obliged if your humanity fhould determine you to infert the above, as it may be a means of exciting inveftigation on a fubject which is certainly of great confequence.

ANNOTATION—W. N.

Defultory remarks on the modification of found by means of folid bodies.

A CONSIDERABLE mafs of fpeculation concerning found, and the means of encreafing its action on the organs of fenfe is to be found in my annotations on the experiments of Perrole, at p. 416 of vol. I. of the Quarto Series of this Journal. The excellen

excellent papers of Mr. Gough, at page 66 and 160 of vol. X. of the present Octavo Series, concerning the augmentation of sounds, and the speaking trumpet have added considerably to our knowledge of this subject. The memoir of Hassenfratz on the same instrument in our Ninth Volume, p. 283, and another, by the same author, on the Propagation of Sound, at Vol. XI. p. 127, also deserve to be consulted. From the whole consideration of the facts it seems as if the sonorous vibration of the instrument were of much more consequence than has hitherto been suspected; and it seems not improbable, that a large surface exposed to the aerial pulses of sound, and having a tail of communication to be applied to the teeth, or inserted in the ear, might have considerable effect. The use of the external ear, which has excited so much discussion, may, perhaps, be of this kind. The experiment of Dr. Moyes (Philos. Journal, III. 57) of conveying sound to great distance through a string may be added to the other facts; and tends to shew that the sonorous undulation does not require to be transmitted through such bodies as are the most dense, uniform, and elastic. Leather, or felt, or pasteboard, or various other similar materials, are more frequently observed to tremble in the hand at certain particular sounds than many other dense bodies.

XII.

Easy and Correct Method of verifying the Portion of a Transit Instrument. By J. S. Butt, *Esq. Communicated by the Author.*

To Mr. NICHOLSON.

SIR, *Paragon, December* 8, 1805.

A SHORT note having appeared in Mr. Kelly's new edition of Spherics, describing my method of verifying the position of a transit instrument, and thinking an account more in detail may not be unacceptable to your astronomical readers, I am induced to trouble you with it, that you may, if you please, insert it in your valuable Journal. Introductory note.

Your's respectfully,

JAMES STRODE BUTT.

TO

The usual method of adjusting a transit instrument by a circumpolar star.

TO make the line of collimation move in the plane of the meridian, we are desired to observe the transits of circumpolar stars, and if the intervals between the times of their transits are equal, the transit instrument moves in the place of the meridian; for the axis and line of collimation being previously adjusted, it must pass through the zenith; and if it divides the circle described by any circumpolar star, into two equal parts, it must pass through the pole.

— requires the clock to keep time for at least 24 hours.

But here a difficulty arises which is a probable alteration, or a want of uniformity in the rates of the clock or watch for so long a period as twenty-four hours, or during that portion of time which the observer may require to repeat his observations, so as to be satisfied.

Method independant of this rate and of previous right ascension, &c.

A method independant of the rate of a clock or watch for so long a time, and also entirely of any other previous observations of right ascension, is a desideratum to practical astronomers, and also to those who occasionally amuse themselves by observing time, and the rates of their chronometers, in their present improved state; but who may be unacquainted with astronomical equations, of precession, nutation, &c.

Rule, observe the transits of two different stars, one above and the other below the pole; which differ only a short time, for example, only a few minutes: at any time afterwards repeat the observation upon the stars when their situations as to the pole are reversed. If the difference in time be the same the transit is duly placed, if not it must be altered, &c.

Rule, Observe the difference of transits of any two circumpolar stars, that are situated nearly in the same azimuth, or vertical circle, the one above and the other below the pole; and whose difference of right ascension is nearly 180°; (namely)

Observe, The transit of α cassiop. above the pole, and immediately after it the transit of ε ursæ majoris below the pole, whose difference of transit is not more than 15 minutes, and for so short a time the clock or watch may be safely depended on. Then invert the operation, and observe the transit of α cassiop. below, and ε ursæ above the pole. If their difference of transits is the same in both observations, the transit instrument is accurately in the meridian; if not the error may be corrected by altering the position of the instrument till their difference of transit is the same in both observations.

Should the error be great it may be corrected nearly by any of the theorems now in use; (*vide* Wales on Time-Keepers) or half the difference may be substituted for the error, and by repeated approximation the transit instrument may be accurately adjusted.

The

The advantage of this method is, that you rely upon the stars keeping $\frac{99}{100}$ of the time, which would otherwise be kept by the clock or watch; and it is of no consequence whether the observations follow one another on the same day or week, provided the instrument is adjusted to the same point of the horizon, previous to observation, for there is little or no difference in their precession, &c. during an interval of a month. Advantages. Most of the time is kept by the stars, and not by the clock.

Another advantage is, that the observations follow each other so soon, that you are not likely to be disappointed by a change of weather; for each pair of observations is complete as far as it goes, which is not the case in the other method, which requiring an interval of twelve hours between each observation, a change of weather is more likely to take place. The short interval insures against change of weather, &c.

A transit instrument is the basis of astronomy, and whoever has the fixing of it should consider himself independant of every previous observation, and acting entirely upon principle, which is not the case where the adjustment is by previously observed right ascension, and which require reducing to the day of observation; indeed nicely reduced right ascensions are not always in the hands of those who may be wish to be in possession of a simple and accurate method of placing a transit instrument precisely in the meridian. Other useful remarks.

This method was devised and used by me since 1794, but I have never read or heard of any one using the same.

J. S. B.

N. B. Proper stars in this Lat. are,

α Cass. and ε Ursæ Majoris.
β Cass. and δ Ursæ.
γ Cass. and ι Ursæ.

Also the stars of Draco and Auriga.
Cepheus and Ursa.
Perseus and Draco.

A large comet was discovered at the Royal Observatory Dec. 8, which passed the meridian at 6.^{h} 24.^{m} 7. mean time. A large comet.

Observed right ascension was - 353° 6′ 41″
Declination south - - - - - - 23° 41′ 8″

*** I have since heard that this comet was not again seen, but is supposed to have proceeded southward.—N.

A Com-

XIII.

*A Comparifon of fome Obfervations on the Diurnal Variations of the Barometer, made in Peyroufe's Voyage round the World, with thofe made at Calcutta by Dr. Balfour *.*

Barometrical obfervations between the Tropics by Lamanon and by Dr. Balfour.

THE firft of the obfervations here referred to were made by M. Lamanon, an ingenious naturalift who accompanied Peyroufe, and who has given an account of them, (fee fourth volume of the *Voyage*, octavo edit.), in a letter to M. de Condorcet, dated St. Catharine, 5th November 1785. Dr. Balfour's Obfervations are in the *Afiatic Refearches* for 1794, and a fhort account of them is alfo inferted in the fourth volume of the *Tranfactions, R. S. Edin.* Hift. p. 25.

M. Lamanon's obfervations were made in confequence of inftructions from the Academy of Sciences, directing him to keep an exact account of the heights of the barometer in the vicinity of the equator at different hours of the day, with a view to difcover, if poffible, the quantity of the variation of that inftrument, due to the action of the fun and moon, that quantity being there probably at its *maximum*, while the variations arifing from other caufes are at their *minimum*.

Lamanon ufed a marine Barometer of Nairne.

M. Lamanon was provided with one of Nairne's marine barometers, which, he fays, was fo little affected by the motion of the fhip, that it might be depended on to the $\frac{1}{100}$ of an inch. In this barometer, he tells us, that from about the 11th degree of north latitude, he began to perceive a certain regular motion, fo that the mercury ftood higheft about the middle of the day, from which time it defcended till the evening, and rofe again during the night. As they approached the equator, this became more diftinctly perceptible; and on the 28th of September, the fhip being then 1° 17′ north latitude, a feries of obfervations was begun, and continued for every hour till the 1ft of October, at 6 A. M. The following abftract fhews the refult of the obfervations on the 28th and 29th.

Regular diurnal change in lower Lats. than 11°. N.

Twenty eighth of September.

From 4 to 10 A. M.	Barometer rofe	1l.	$\frac{8}{10}$
From 10 A. M. to 4 P. M.	fell	1	$\frac{3}{10}$
From 4 to 10 P. M.	rofe	0	$\frac{9}{10}$

* From the Hiftory of the Royal Society of Edinburgh, 1805.

Twenty

Twenty ninth of September.

From 10 (28th) to 4 A. M.	fell	1*l.* $\frac{1}{16}$
From 4 to 10 A. M.	rose	1 $\frac{5}{16}$
From 10 A. M. to 4 P. M	fell	1 $\frac{1}{16}$
From 4 to 10 P. M.	rose	1

The observation on the 30th were to the same effect; and hence it is concluded that at the equator the flux and reflux of the atmosphere produces in the barometer a variation of about 1 line $\frac{2}{10}$ English, corresponding, as M. Lamanon remarks, to a height in the atmosphere of nearly 100 feet. According to Bernouilli, the action of the sun and moon should produce a tide of seven feet, and according to Mr. de la Place, a tide not nearly so great.

The effect is greater than might arise from the computed atmospheric tides.

It should be observed, that when these observations were made, the moon was in her last quarter, and the sun a few degrees to the south of the equator. The latitude on the 28th was 50′ north, and 11′ north on the 29th; in the night between that and the 30th, the ship crossed the line; and on the 30th at noon, the latitude was 42′ south: the longitude all this while between 17° 31′ and 18° 33′ west of Paris, by the time-keeper; so that the coast of Africa, which was the nearest land, was distant about 8° of a great circle, and the American continent about 19°.

Situation of the sun and moon, and place of the ship.

She was *far out at sea*.

The agreement between these, and Dr. Balfour's observations at Calcutta is very remarkable. Dr. Balfour found that during the whole lunation, in which he observed the barometer from half-hour to half-hour, the mercury constantly fell from 10 at night to 6 in the morning; from 6 to 10 in the morning it rose; from 10 in the morning to 6 at night it fell again; and lastly rose from 6 to 10 at night. The *maximum* height is therefore at 10 at night and 10 in the morning, and the *minimum* at 6 at night and 6 in the morning. The only difference is, that in Mr. Lamanon's observations, the *minimum* is stated to have happened about 4 instead of 6. This, however, will not seem a very material difference, when it is remembered, that the instant when any quantity attains either its greatest or its least state is not easily ascertained with precision. From the observations as detailed by M. Lamanon, the time of the *minimum* seems to answer fully as well to 5 as to 4; so that the difference of the results is in every

Argreement between these Observations, and those of Dr. Balfour *at Calcutta*.

every view inconsiderable, and their coincidence on the whole, not a little singular. The variations in Dr. Balfour's barometer between the nearest *maximum* and *maximum* is sometimes about $\frac{1}{10}$ of an inch, though in general considerably less.

Whether the cause which produces land and sea winds could produce the regular change.

In the abstract of Dr. Balfour's observations referred to above, it is remarked, that it seems not improbable that these variations of the barometer are connected with the reciprocations of the sea and land winds during the day and night. But whatever may have been formerly the probability of this supposition, it is entirely destroyed by the observations of the French navigators. These observations were made too far out at sea to leave room for supposing that the land winds had any influence on the phenomena to which they refer. It is at the same time doubtful, whether those phenomena can be ascribed to the atmospherical tides produced by the sun and moon, as the *ebbing* and *flowing* of the mercury in the barometer appears to have no dependence on the position of those luminaries relatively to one another, but happens, it would seem, constantly at the same hour, in all aspects of the moon and all seasons of the year. The subject is well deserving of a fuller investigation.' We should probably before now have had farther information respecting it, if happily the able navigator above-named, and his brave associates, had been destined to revisit their native shores. The cruel fate of an expedition so well planned, and so well appointed for the purposes of science, will never cease to be matter of sincere regret.

—most probably not.

—neither is it likely that it was caused by tides in the air, as it does not follow the moon.

Annotation.—W. N.

Probability that the equi-tropical change is caused by ascending and descending currents in the atmosphere.

I have inserted the foregoing with a view, in some measure, to afford a comparison with Mr. Horsburgh's paper on the same subject, at page 16. It is not without diffidence that I venture to propose a conjecture on this subject, which in fact requires more consideration than I can, at present, bestow on it. Its change seems to me to be governed by the ascent of the air which would take place immediately beneath the sun, if the earth were stationary, and the surrounding descent of the same fluid, of which the circumstances and modifications are so well explained

Dr. Herschel on the Sun's proper Motion &c.

Fig. 1.

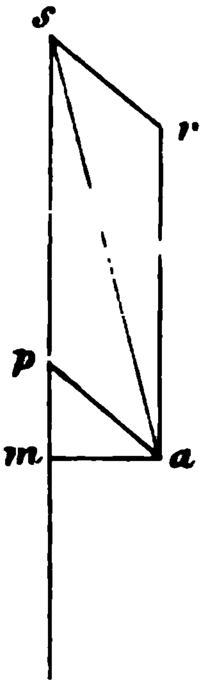


Fig. 2.

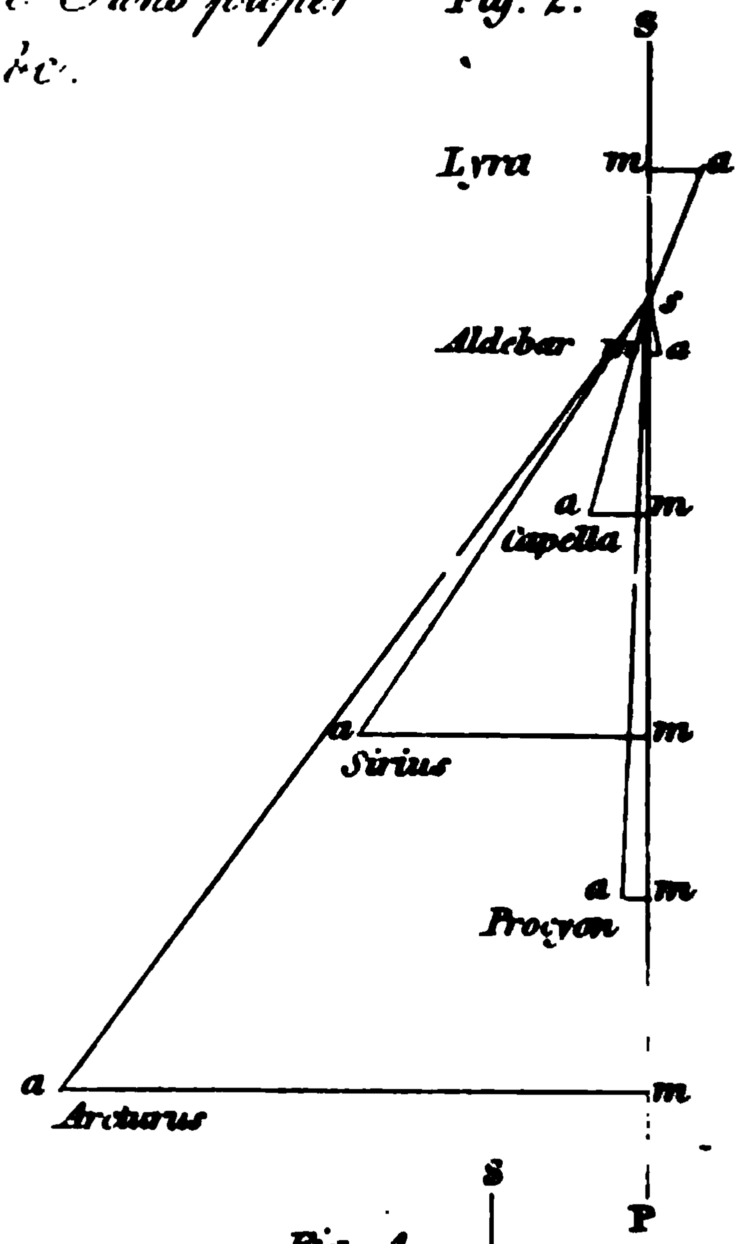


Fig. 3.

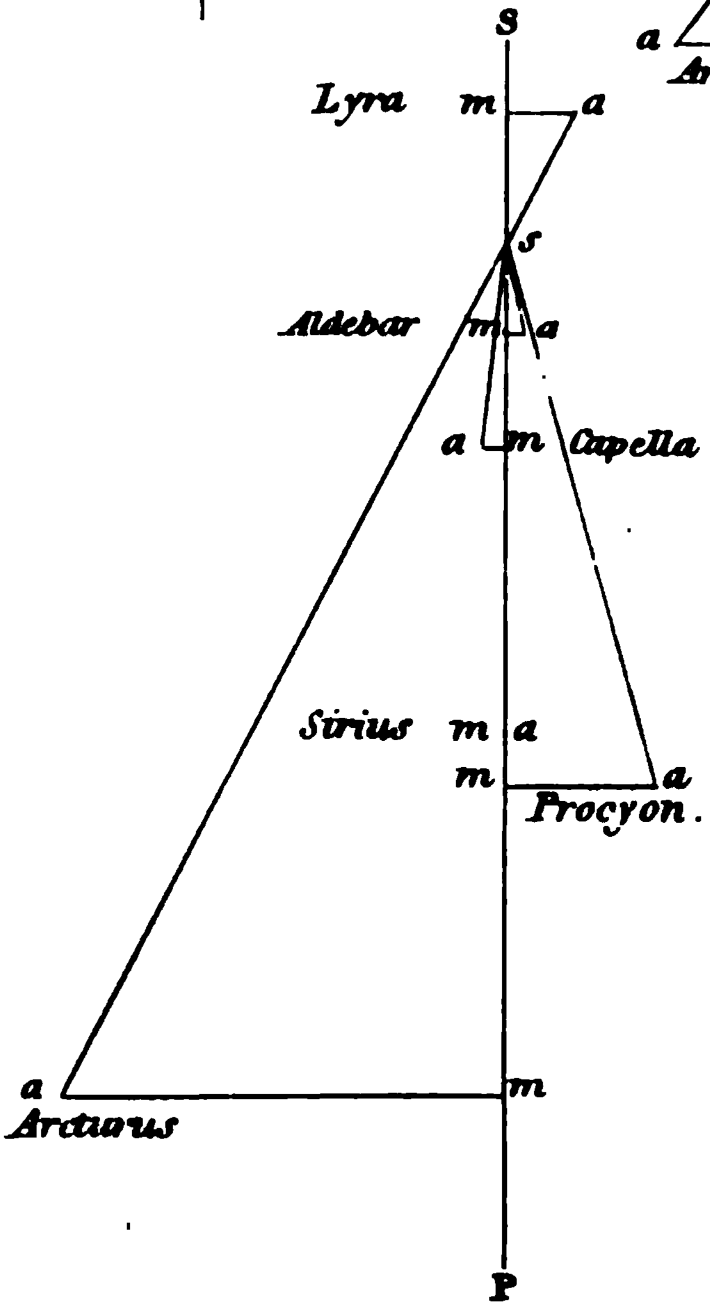
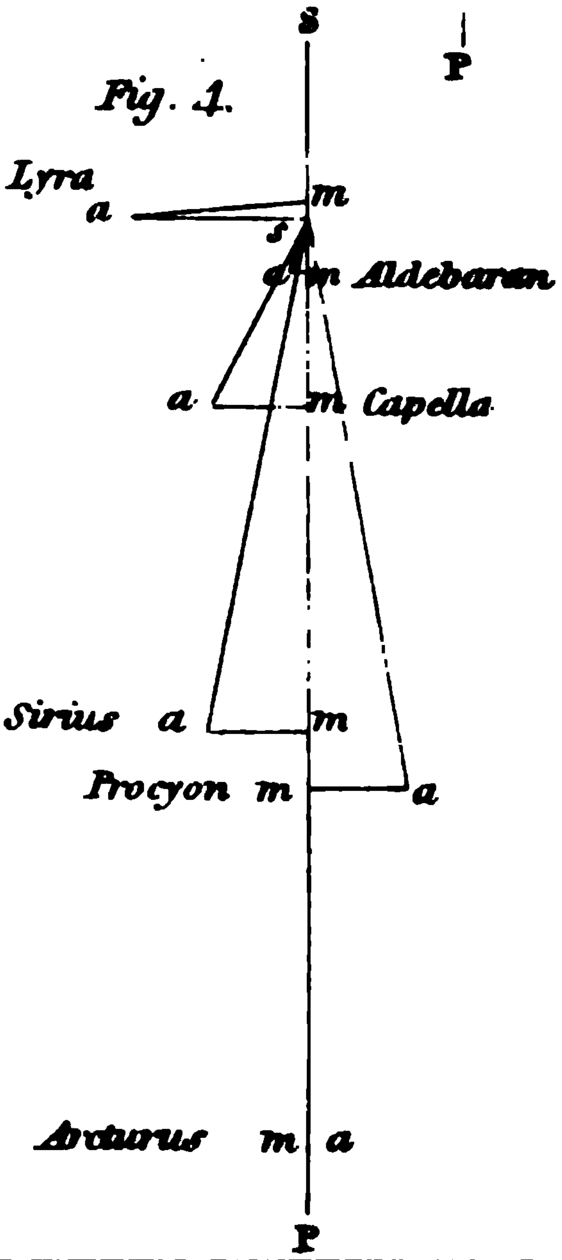


Fig. 4.



explained by Prieur in his memoir on the morning and evening dew (at p. 86, vol. IV. of our quarto feries.) The confiderations there detailed may be eafily extended to fhew alfo that the effects muft be greatly altered, and, in moft inftances, obliterated by the vicinity of land; which even changes the regular trade winds into land and fea breezes.

XIV.

Abftract of a Memoir on the Direction and Velocity of the Motion of the Sun and Solar Syftem. By Dr. HERSCHEL. *From the Philofophical Tranfactions,* 1805. (A.)

THE learned author begins his paper by noticing Dr. Mafkelyne's table of the proper motions of 36 ftars of the firft magnitude, and conceives that if this table affords proof of motion in ftars in our immediate neighbourhood, the changes of pofition in minute double ftars, many of which are only to be feen by means of the beft telefcopes, likewife prove that motions are equally carried on in the remoteft regions of fpace. Proper motions of the fixed ftars.

In 1783, the Doctor deduced from the proper motions of the ftars, a motion of the fun and folar fyftem towards λ hercules; and the opinion he then conceived has been much ftrengthened by the confiderations ftated in the following pages. Should this doctrine be eftablifhed, many phenomena may be accounted for, which without it muft remain inexplicable. Deduction of a proper motion of the fun and its direction.

Though it was propofed, by the admiffion of a folar motion, to take away many of the proper motions of ftars, by invefting the fun with a contrary one; our author admits that it will reveal a vaftly greater number of concealed real motions than would be neceffary to admit, were the fun at reft; and that the neceffity for admitting its motion ought therefore to be well eftablifhed. Confiderations in favour of fuch a motion.

The motion of fatellites round their primary planets, and of thefe round the fun, fuggefts the idea of a revolution of the latter round fome other unknown centre; nor are we without hypothefes built upon this conjecture.* Reafons from analogy of the planets, &c.

The poffibility of a folar motion has been fhown by the late

* See Syftème du Monde de Lambert, p. 152, 158. Alfo Phil. Tranf for the year 1783, p. 283.

Dr.

Dr. Wilfon, of Glafgow, upon theoretical principles; and in probability, from reafons of the fame nature, by M. de Laland.

Probability that the fun has progreffive as well as rotatory motion.

The rotatory motion of the fun, from which the latter concludes a difplacing of the folar centre, indicates a motion of tranflation in fpace; for it is not very probable that the mechanical impreffion which gave the former, fhould not alfo occafion the latter. This however can be admitted only as a plaufible hypothefis, until we attain a knowledge of the caufe of the rotatory motion.

—and the variable ftars alfo.

This argument might be ftrengthened by clofely obferving the ftars which change their magnitudes periodically; for if thefe changes arife from a rotatory motion,* a real motion in fpace may be expected to attend it; and the multitude of thefe ftars is fo great, that their concurrent teftimony is defirable.

Three forts of motions of ftars.

But fetting afide theoretical arguments, the Doctor notices that as all parallactic motions indicate the obferver not to be at reft, it may be neceffary to explain three forts of motions, which will frequently be alluded to in the following difcuffion.

Parallactic, real, and apparent.

Suppofe the folar fyftem to move towards a certain part of the heavens, the ftars, to an inhabitant of the earth, will appear to move in an oppofite direction. Let $s\,p$ (*Pl.* II. *Fig.* 1.) reprefent the parallactic motion of a ftar; which, if the ftar have no real motion, will alfo be its apparent motion; but if it fhould have a real motion, which in the fame time that it could have gone from s to p, would have carried it from s to r, it will be feen to move along the diagonal $s\,a$; and $p\,a$, being parallel and equal to $s\,r$ will reprefent its real motion. The triangle $s\,p\,a$ is fuppofed to be formed in the concave of the heavens by three arches of great circles, the obferver being in the centre, and $s\,p$ reprefents the parallactic, $p\,a$ the real, and $s\,a$ the apparent motion of the ftar. The fituation and length of thefe arches in feconds of a degree will reprefent the direction and quantity of each motion; and calling the folar motion S, the diftance of the ftar from the fun d, and the fine of the ftar's diftance from the point towards which the fun is moving ϕ; the parallactic motion will be expreffed thus: $\frac{\phi.S}{r.d} = s\,p$.

The largeft ftars are moft fit to fhew the fun's motion.

A motion of the fun will occafion parallactic motions of the ftars, and *vice verfa*; but to afcertain if parallactic motions exift, fuch ftars fhould be examined as are moft vifibly affected

* See Phil. Tranf. for the year 1795, p. 68, and our Journal, XI. 271.

by

by solar motion; which points out the brightest stars as most proper for the purpose; for any star may have great real motion, but to have great parallactic motion it must be in the neighbourhood of the sun.

The parallactic motions are directed to a point,

Parallactic may be distinguished from real motions by their directions: for, if a solar motion exist, all parallactic ones will tend to a point in opposition to the direction of that motion; but real motions will be indiscriminately dispersed.

and will combine with the real (angular).

Under these distinctions, the proper motions of the stars, if the sun be not at rest, will be parallactic, or composed of real and parallactic; the latter case constituting the apparent motion of the star.

Deduction of the parallactic centre from obs. on many stars.

Dr. H. next describes the meeting of the arches arising from a calculation of the proper motions of the 36 stars in Dr. Maskelyne's catalogue on a celestial globe, of which ten were made by stars of the first magnitude, about the constellation Hercules; beyond these there was no appearance of any other than a promiscuous situation of intersections.—Of the intersecting points, that towards which the sun moves is denominated the apex of its motion; and as the stars will then have a parallactic motion towards the opposite point, it has received the appellation of a parallactic centre.

Tabulated results.

Intersecting points.	Right Ascension.			Polar distance North.		
	°	′	″	°	′	″
1. Sirius and Arcturus, in the mouth of the Dragon - - -	255	39	50	36	41	34
2. Sirius and Capella, near the following hand of Hercules - -	275	9	32	64	21	48
3. Sirius and Lyra, between the hand and knee of Hercules - -	272	23	58	58	23	24
4. Sirius and Aldebaran, in the following leg of Hercules - -	263	25	38	44	39	47
5. Arcturus and Capella, N. of the preceding wing of the Swan -	290	0	58	32	7	23
6. Arcturus and Aldebaran, in the neck of the Dragon - - -	267	2	19	33	57	20
7. Arcturus and Procyon, in the preceding foot of Hercules -	235	3	13	46	21	34
8. Capella and Procyon, S. of the following hand of Hercules -	272	51	49	73	7	56
9. Lyra and Procyon, preceding the following shoulder of Hercules	266	46	49	66	48	11
10. Aldebaran and Procyon, in the breast of Hercules - - - -	260	1	29	60	59	34

As

Confirmation by other stars.

As a further confirmation that the parallactic motion may be perceived in the motion of the brightest stars, Dr. H. examined the intersections made by the proper motions of some large stars of the next order, with the arches in which the stars of the first magnitude move, and found 15 which gave similar results with the former 10, in pointing out the same part of the heavens as a parallactic centre.

This result confirmed by double stars.

Changes in the position of double stars indicate the same result, and may therefore be more eligibly ascribed to the effect of parallax, than to admit of separate motions in different stars: for, if the alterations of the angle of position were owing to a motion of the largest star in each set, such motions must, in contradiction to probability, tend nearly to one particular part of the heavens. This argument derives its validity from the same source with the former, viz. the parallactic motions of at least 28 more stars pointing out the same apex of a solar motion, by their direction to its opposite parallactic centre.

and by the harmony of the proper motions deduced from it.

The incongruous mixture of great velocity and extreme slowness in the proper motions of the stars of the same magnitude, is removed by the consideration of parallax from the solar motion; and it will be seen that there is a general consistency in their motions. The same observation is also applicable with respect to the sidereal occultation of a small star in the Swan.

Investigation of the direction of the sun's proper motion.

Dr. H. concludes from the foregoing premises, that the expediency of admitting a solar motion will not be questioned, and proceeds to investigate its direction. He begins by proving, that when the proper motions of two stars are given, an apex may be found, towards which if the sun be supposed to move with a certain velocity, the two given motions may be resolved into apparent changes arising from sidereal parallax, the stars remaining perfectly at rest. For we must not admit more motions than are sufficient to account for the observed changes in the situation of the stars; and it would be wrong to have recourse to the motions of two stars, when that of the sun alone may be sufficient to account for both; which consideration would be a sufficient inducement for fixing at once on the calculated apex as well as on the relative distances assigned to the two stars, could other proper motions be, with equal facility, resolved into similar parallactic appearances: but, when a third star does not direct towards the

An apex or parallactic centre is deduced from the apparent motions of two stars, supposed to have no real motion.

fame apex as the former two, its apparent motion cannot be refolved by the effect of parallax alone; and this difficulty is farther enhanced by the number of apices required to folve all proper motions into parallactic ones, increafing, not as the number of ftars admitted to have proper motions, but, when their fituation is favourable, as the fum of an arithmetical feries of numbers, beginning at 0, continued to as many terms as there are ftars admitted.

The author here propofes an illuftration of his fubject by confidering the three apices, or interfecting points, No. 1, 2, 5, in the foregoing table.

The diftance of Arcturus from the apex of the folar motion is found to be 47° 7′ 6″, and its parallactic motion, which is as the fine of that diftance 2.08718″, which is the apparent motion of Arcturus, as eftablifhed by obfervation. Namely Arcturus and Sirius.

Admitting Sirius to be a very large ftar, at the diftance of 1.6809 from us, and computing its elongation from the apex of the folar motion at 138° 50′ 14.5″, its parallactic motion will be $\frac{\varphi \,.\, S}{r \,.\, d} = s\,p = 1.11528''$, which alfo agrees with the apparent motion already afcertained by obfervation as the proper motion of Sirius.

The diftance of Capella from the apex of the folar motion is 80° 54′ 46″, and admitting the velocity of the fun towards the before given point, it will occafion a parallactic motion of Capella, in a direction 89° 54′ 48″ fouth-following its parallel, amounting to 2.8125″. Capella is here taken for a ftar of the firft magnitude, fuppofing its diftance from us to be equal to that of Arcturus. Hence the parallactic motion of another, viz. Capella, is deduced:

By conftructing a triangle, the fides of which reprefent the three motions of every ftar, not at reft; one of the fides, reprefenting the apparent motion, will be equal to 0.4637″; the other fide, being the parallactic motion, 2.8125″; and the included angle 18° 19′ 27″, from which will be obtained the third fide, or the real motion of the ftar, 2.3757″. By the given fituation of this triangle with refpect to the parallel of declination of Capella, the angle of the real motion will be had, which is 86° 34′ 11″ north-following the parallel of this ftar. A compofition of the parallactic and real motions in the directions, will produce the annual apparent motion, as eftablifhed by obfervation. and by refolving this into the apparent motion and another, this laft will be the real (angular) motion, (fuppofing the other ftars to have none.)

It is here obferved, that although the proper motion of a

third

But it is not fit that all the real motion should be ascribed to Capella; third star is accounted for by retaining the same apex of the solar motion which explained the apparent motions of the other two, yet a great degree of real motion has been assigned to Capella, of which Arcturus and Sirius have been altogether deprived; which shews that the apex of solar motion must be so fixed as to be equally favourable to every star proper for directing our choice. Hence a problem arises, for discovering a point whose situation among three given apices shall be such as if the sun's motion be directed towards it, there may be taken away the greatest possible quantity of proper motion from the three given stars. The intricacy of this problem is, that by a change of the distance of the apex from any one of the stars, its parallactic motion, which is as the sine of that distance, will be affected; so that it is not merely the alteration of the angle of direction which is concerned. From the solution of this problem, a much more complex one would arise, as three stars would certainly not be sufficient to direct the present endeavour to find the best situation of an apex for the solar motion.

but the apex must be taken so as to leave the real motions as small as possible.

Apparent motions of six bright stars It was before shewn that the brightest stars are the most proper for demonstrating the effect of parallax, and that in searching after the direction of the solar motion, the aim should be to reduce the proper motions of the stars to their lowest quantities. The six principal stars, whose intersecting arches have been given, when their proper motions in right ascension and polar distance are brought into one direction, will have the following apparent motions:

tabulated.

Names of the Stars.	Direction of the apparent Motions.				Quantities of the apparent Motions, per Year
Sirius,	63°	49′	40.7″	South-preceding.	1.11528″
Arcturus,	55	29	42.0	Ditto.	2.08718
Capella,	71	35	22.4	South-following.	0.46374
Lyra,	56	20	57.3	North-following.	0.32435
Aldebaran,	76	29	37.3	South-following.	0.12341
Procyon,	50	2	24.5	South-preceding.	1.23941
	Sum of the apparent Motions,				5.35337

In

In seeking a solar motion, which requires the least motion in the above six stars, let the line $p\,a$, *Fig.* 1, which represents the real motion, be brought into the situation $m\,a$, and the real motion required will then be at a minimum. If by the choice of an apex for the solar motion the angle at s, made by the lines $s\,p$ and $s\,a$, can be lessened, the quantity of real motion required to bring the star from the parallactic line $s\,p\,m$ to the observed position a, will also be diminished.

Deduction of a solar motion that shall leave the real motions of these the least possible.

It has already been shewn that when two stars only are given, the line $s\,p$ may be made to coincide with the line $s\,a$ of both stars, whereby their real motions are reduced to nothing; and that when three stars are concerned some real motion must be admitted in one of them. Now, since all parallactic motions are directed towards the same center, a single line may represent the direction of the effect of the parallax. Therefore, let s P or s S, *Fig.* 2, stand for the direction of the parallactic motion of the stars; and as in the foregoing table we have the angles of the apparent motion of six stars, with the parallel of each, the direction of the line s P or s S must be computed with the parallels of the same stars, which may be done as soon as an apex for the solar motion is fixed upon. The difference between these angles and the former will give the several parallactic angles P $s\,a$ or S $s\,a$, required for an investigation of the least quantity, $m\,a$, belonging to every star.

A single line may shew the direction of parallactic effect in two stars, &c.

The author exemplifies what he here lays down, by supposing the sun to move towards λ Herculis; and calculating the required angles of the direction in which the effect of parallax will be exerted with the six stars already selected, he obtains the angles of the parallactic motion with the parallel, the difference between which and the former apparent angles with the parallel of each star gives the angles of the apparent with the parallactic motion, as represented in *Fig.* 2. The lines $s\,a$ represent the annual quantity of the apparent motions.

Computation, supposing the sun to move towards λ Herculis.

When the situation of the last mentioned angles is regulated as in the figure alluded to, the several lines $m\,a$ may be drawn perpendicular to S P, and by computation their quantity will be found to be—

Sirius	- -	0.65437″
Arcturus	- -	1.28784
Capella	- -	0.10887
Lyra -	- -	0.11281
Aldebaran	- -	0.01104
Procyon	- -	0.04998
Sum	- -	2.22491

The result of this investigation is, that by admitting a motion of the sun towards λ Herculis, the annual proper motions of the fix stars alluded to, of which the sum is 5.3537″, may be reduced to real motions of no more than 2.2249″.

A more favourable apex.

The author here observes, that although the precise place of the best apex is difficult to ascertain, a more favourable one than that above proposed may be obtained: for, by inspection of the figure which represents the quantities of real motion required, when λ Herculis is fixed upon, it will appear that by a regular method of approximation, the line S P may be turned into a situation, wherein all the angles of the apparent motion of the fix stars will be much reduced; and it is evident that the parallactic line S P should be turned more towards the line *s a*, representing the apparent motion of Sirius. He accordingly tries a point near the following knee of Hercules, whose right ascension in 270°. 15′. and north polar distance 54°. 45′, see *Fig.* 3, the quantities required for constructing which are found by the same method as already described in *Fig.* 2. By a calculation of the angles and the least quantities of real motion, according to this apex, it appeared that the annual motion of the fix stars was reduced to 1.4594″, which is 0.7655″ less than when the apex was λ Herculis.

Its situation.

Supposition that Sirius may be most affected by parallax, as brightest;

In the approximation to this point, it appeared, that when the line of the parallactic motion of Sirius was made to coincide with its apparent motion, a certain minimum might be easily obtained of the other parallactic motions. But as Sirius has not the greatest proper motion, the author conceived that another minimum, obtained from the line wherein Arcturus appears to move might be more accurate; as this star from its great proper motion may be more affected by the parallax arising from the motion of the sun. He therefore chose a point not only in the line of the apparent motion of Arcturus, but equally favourable to Sirius and Procyon, the remaining two stars which have the greatest motions.

or Arcturus, as having the greatest apparent motion.

CH

"If the principle of determining the direction of the solar motion by the stars which have the greatest proper motion, be admitted", observes the author, "the following apex must be extremely near the truth; for, an alteration of a few minutes in right ascension or polar distance either way, will immediately increase the required real motions of our stars. Its place is, right ascension 245° 52′ 30″, and north polar distance 40° 22″. The calculation is delineated in *Fig.* 4. The sum of the least quantities of real motion in this experiment is 0.95595″, less than the former by 0.50343″.

Apex on this supposition.

In these calculations the author has proceeded upon the principle of obtaining the least possible quantity of real motion to ascertain the most favourable situation for a solar apex, and has proved that the sum of the observed proper motions of the six principal stars may be the result of a composition of two other motions; and that if the real motions were reduced to their smallest possible quantities, they would not exceed 0.9559.

The Doctor, however, seems to think that these real motions may not be brought down to the low quantities mentioned; and proceeds to shew that this circumstance will not affect the arguments he has used for establishing the method he has adopted; for, although the great proper motions of Arcturus, Procyon, and Sirius, are strong indications of their being affected by parallax, it is not probable that the apparent changes of their situations should be entirely owing to solar motion; but that their own real motions would have a great share in them; and it is evident that in parallactic motions the distance of a star from the sun is of material consequence; and as this cannot be assumed at pleasure, we are not at liberty to make the parallactic motion sp, *Fig.* 1, equal to the line sm; hence it follows that the real motion of the star cannot be from m to a, but will be from p to a. If, however, $m\,a$ be a minimum, $p\,a$ when sp is given, will also be a minimum, and if all the $m\,a$'s in *Fig.* 4 be minima, the sp's will give the $p\,a$'s as small as possible; which is the point desired to be established.

If the nearest stars be most affected by parallax, their proper motions may also be more evident.

In concluding Dr. H. observes, that as it is known that proper motions do exist, and as no solar motion can resolve them entirely into parallactic ones, we ought to prefer that direction of the motion of the sun which will take away most real motion, and this, as has been shewn, will be done when the right ascension of the Apex is 245° 52′ 30″, and its north polar distance 40° 22′.

Conclusion. Preferable situation of the apex.

New

XV.

New Experiments on the Solution of Sulphur in Alcohol, and the various Kinds of Ether. By M. FAVRE *.

Probability that ether would dissolve more sulphur than alcohol.

IN my first note on the solution of sulphur in alcohol, I announced an intention of examining the solvent power of the several ethers upon this combustible; which I had at that time been prevented from, by being obliged to leave Paris for Brussells, to take the office of apothecary to the military hospital. In the paper alluded to, I hinted that it was probable ethers would dissolve sulphur in greater quantity than alcohol; I had been led into this opinion by the results obtained from mixing this mineral with alcohol, at various degrees. I observed, as already stated, that the more alcohol was rectified, the more readily it dissolved sulphur; and *vice versa*, which difference I imagined to proceed from the greater quantum of hydrogen contained in highly rectified alcohol. Knowing ethers to contain less carbon and more hydrogen than alcohol, I had no doubt that they would dissolve a greater quantity of sulphur. The result of the several experiments, which I made under this impression, I am now about to detail: from which it will be perceived that I was not mistaken in my conjecture. I shall also subjoin the new experiments, which I made with alcohol, to ascertain the precise quanity of sulphur it is capable of dissolving, in order to compare the results with those obtained from ethers.

Preparation, &c. of the ethers.

The ethers I employed were prepared with much exactness, and according to the methods recommended by professor Fourcroy. I took care, in each experiment, to ascertain the specific gravity of the ether made use of, the quantity of sulphur dissolved by it, the various results obtained with or without the contact of the sun's rays, and the properties of sulphurated ether.

First and second Experiments.

Sulphuric ether by long digestion without heat took up nearly

In each of two six-ounce matrasses I put two drachms of the flowers of sulphur, prepared in the same manner as for the

* Van Mons's Journ. de Chimie, Vol. VI.

the experiments mentioned in my first note, viz. nicely washed, and one ounce of rectified sulphuric ether, whose weight was 0.7396. Having secured the mouths of the mattrasses with luting, I put one in a very light place, and the other in a dark place. I shook them every day, and at the end of a month, filtered their contents. On examination the two sulphurated ethers obtained by these operations, presented the following characteristics:

one-thirteenth part of sulphur in the light; and only one-seventeenth in the dark.

The colour of the ether exposed to the light was scarcely changed; it had a powerful hydro-sulphurous smell, and its taste was disagreeable, and likewise hydro-sulphurous. Mixed with distilled water, it precipitated nothing; but I remarked that the water dissolved a much less quantity of it than when pure. In proportion as the ether became volatilised, the sulphur formed a whitish scum on the surface of the liquid, which at length was precipitated to the bottom of the glass in which the experiment was made. (I shall hereafter mention the quantity of this precipitate.) Put in contact with white metals, it deeply blackened them. (Care must be taken in this latter experiment to close exactly the mouth of the vessel in which the metals are placed in contact with sulphuric ether, on account of the great tendency of ether to be converted into gas by its attraction of caloric from surrounding bodies.) When mixed with a solution of acetite of lead, it gave a pretty considerable black precipitate.

The sulphurated ether prepared without light, possessed all the properties of the other, but in a less degree. It also was less impregnated with sulphur: for, on a repetition of the experiment, and carefully weighing the products, I found that each ounce of the ether prepared in the light contained 38 grains of sulphur; whilst that prepared in the dark held only 29.

Third and fourth Experiments.

Having proceeded as above described, with nitric ether weighing 0.9088, I obtained an ether whose colour was in no degree changed; its smell and taste, though hydro-sulphurous, were not so powerful as those of sulphurated sulphuric ether; mixed with distilled water, it presented the same phenomena, but deposited a less quantity of sulphur. It discoloured white metals less forcibly than the preceding ether;

Nitric ether by the same treatment took up nearly one twenty-second part of sulphur in the light; and only twenty-fourth in the dark.

ether; and, in a word, it had all the qualities of fulphurated fulphuric ether, but in a lower degree. It likewife contained a lefs quantity of fulphur; the refult of the experiment made in the light being but 22 grains of precipitated fulphur; and 20 for that conducted in darknefs.

Fifth and fixth Experiments.

Muriatic ether took up one thirty-feventh in the light; and only one fifty-third in the dark.

With muriatic ether, weiging 0.7196, proceeding as already defcribed, and at the fame proportions, I obtained a fulphurated muriatic ether, poffeffing all the peculiarities above mentioned, but weaker. It contained only 13 grains of fulphur, when conducted in the light, and $9\frac{1}{2}$ grains when managed in the dark.

Seventh and eighth Experiments.

Acetic ether took up very little fulphur.

Acetic ether weighing 0.8664, diffolved but a very fmall portion of fulphur, and its qualities were but flightly marked. It contained but three grains of fulphur in an ounce of ether, in the experiment made in the light, and about $1\frac{1}{2}$ grains in that made in the dark.

Ninth Experiment.

Solution of fulphur in alcohol was lefs charged than that of fulphuric ether.

Having made the foregoing experiments, I wifhed to afcertain the difference exifting between the feveral ethers and alcohol, in refpect to their capacity for diffolving fulphur: I therefore retraced the experiments I had formerly made with alcohol. To avoid the repetition of what has been already communicated in my firft effay, I fhall here merely ftate the quantity of fulphur I have been able to diffolve, either by fubmitting the mixture to a heat lefs than fufficient to caufe the alcohol to boil, or by expofing it to the light, or by placing it in a dark place. For thefe experiments I ufed alcohol of 43 degrees.

After digefting for 12 hours over a gentle fire an ounce of alcohol with two drachms of the flowers of fulphur, I obtained 23 grains of precipitate.

Tenth and eleventh Experiments.

On leaving fimilar mixtures, one expofed to the rays of the fun, and the other in a place impervious to the light, during a month, and proceeding as already defcribed, I obtained

tained 16 grains from the firſt mixture, and 13 from the ſecond.

After what has here been laid down, it is evident that ſulphuric ether diſſolved the greateſt quantity of ſulphur; for, after frequently repeating the experiment, I found the average to be 25 grains in an ounce. Nitric ether and alcohol at 43 degrees, diſſolved nearly in the ſame proportions; and acetic ether the leaſt of any. Recapitulation.

It has been long a deſideratum in medicine to diſcover a method of adminiſtering ſulphur in a ſtate of extreme diviſion, eſpecially in complaints of the lungs and diſeaſes of the ſkin. With this intent, phyſicians have recommended it to be diſſolved in eſſential oils, and to form what is known in pharmacy under the title of *balſams of ſulphur, terebinthinated, aniſated, &c.* Theſe medicaments have the diſadvantage of giving to the mixtures into which they enter an almoſt inſupportable taſte and ſmell of ſulphurated hydrogen. Sulphurated ether is free from this inconvenience; it may be eaſily mixed with other potions, to which it gives very little ſmell; and as the ſeparation of the ſulphur is only occaſioned by the evaporation of the ether, it may be eaſily prevented by keeping the mixture to which it is added cloſely corked. I have already adopted its uſe with ſucceſs, adminiſtered either upon ſugar, or with any appropriate vehicle: ſeveral phyſicians of my acquaintance, for whom I have prepared it, have likewiſe employed it with advantage: and I hope, ere long, to be able to flatter myſelf as having added an efficacious medicament to the art of healing. Sulphurated ether is a good medicine.

The ſulphurated ether may be alſo ſucceſsfully employed to detect the adulteration of wine with preparations of lead: in addition to the facility with which this ether precipitates the lead, in the form of a black ſulphur, it poſſeſſes the advantage of introducing nothing into the wine that can deceive as to its quality, which ſometimes happens even to thoſe who are accuſtomed to uſe the ſolution of ſulphur of potaſh. It may be uſed as a teſt for lead in wine.

I am now occupied in the cryſtallization of ſulphur diſſolved in ether, the reſult of which I ſhall loſe no time in laying before the public.

On

XVI.

On the Utility of scientific periodical Publications. In a Letter from Mr. RICHARD WINTER. *To which are added, some Experiments of Heat produced by a Blast of Air from Bellows.*

To Mr. NICHOLSON.

DEAR SIR,

Periodical works are of modern invention.

THE advantages derived from scientific periodic publications, are an acquisition which former philosophers were not possessed of; and it was not until the last century they were first instituted. The rapid progress of science and information since that period, would be a sufficient argument in favour of their decided utility, without any reference to systematic treatises published, of undoubted merit, and sanctioned by universal approbation.

Advantages derived from scientific Journals.

To the active and ingenious mind in early life this mode of information is invaluable. Besides furnishing new ideas to the young student, they point out the precise state of the different branches of human knowledge; they teach him the necessary caution for conducting experiments with vigour and accuracy, instead of drawing conclusions from a few insulated analyses, or imagining that his data are sufficiently perfect for establishing new systems. By reading these publications it is that he will enlarge his general conceptions, and will learn to emulate the various illustrious characters of all the enlightened countries of the world. In these treatises his views will not be confined to one object, but he will contemplate a scene continually varying. The physiology and phenomena of the animal and vegetable kingdoms; the actions and re-actions of the different elementary substances in nature, and their combinations with each other, will pass in succession under his observation.

The great physical laws which constitute and maintain the equilibrium of the world, are inserted in respectable works of this nature as they are discovered and demonstrated, while the errors of former philosophers are detected and exposed; by which means he has an opportunity of ascertaining the value and correctness of those works he may be already in possession of.

To

To those who consult an Encyclopædia for scientific matter, these publications are of indispensable utility, by continually pointing out the numerous improvements as they become public, and by that means the general system of philosophical knowledge is kept to the level of the existing state of discovery.

To the mechanic a repository of this kind must be highly useful, as the receptacle in which he may record his labours and improvements, and secure to himself the well-earned fame of his discoveries, at the same time that he derives advantage from others following his example in their contributions to the general fund of science.

In short, there is no class of individuals but may profit from this method of extending useful knowledge. The small sum of seven-pence or eight-pence a week to any economical person is trifling, and there is no doubt but every enquirer will find something of which he may abridge himself, in order to become possessed of such an assemblage of facts and opinions. He is as it were making himself intimate with a class of men whose names will be read with admiration by a grateful posterity. It is only by familiarizing the mind with the sublime objects of science, and diffusing them over the face of the earth, that we can expect to establish that spirit of philanthropy and social order, which is so necessary to the happiness of the human race.

I will leave it to your judgment to abridge, or cancel the whole of this paper, as it would perhaps exclude more valuable subjects.

I am, Sir,

With the greatest respect,

Your very humble servant,

RICHARD WINTER.

21, *Bolsover Street,*
Dec. 14, 1805.

The thermometer raised by a blast from bellows.

The following experiments were made in order to ascertain whether a current of air projected upon a thermometer would increase or diminish the temperature. I made use of a pair of common bellows, the contents of which, when opened, were 95 cubic inches; the diameter of the end of the pipe was

was $\frac{3}{17}$ths of an inch. The thermometer was adapted to Fahrenheit's ſcale, and the reſults of three experiments are exhibited in the following table:

		Number of Blaſts.	Time of blowing.	Therm. roſe.
Exp. 1.	-	425	6 minutes.	4°
2.	-	222	3 ———	3.75
3.	-	217	3 ———	3.7

The current of air was directed againſt the bulb of the thermometer. The diſtance of the pipe out of which the air iſſued, was half an inch from the bulb. The experiments were repeated with every caution poſſible for twelve times, and always with the ſame reſults.

Mr. Dalton obſerved (Philoſ. Journal, III. 160), that the thermometer fell on exhauſting the veſſel in which it was placed, and roſe again on re-admitting the air. It is probable that the riſing of the thermometer in my experiments may be referred to the ſame cauſe, *viz.* the greater capacity of a vacuum for caloric than atmoſpheric air.

XVII.

An Account of two interſecting Rainbows, ſeen at Dunglaſs in Eaſt Lothian in July laſt, was communicated by Profeſſor PLAYFAIR *.

Large rainbow where the ſun was 2° high.

"AT Dunglaſs, where I happened to be in the beginning of July laſt, 1799, our attention was called one evening, a little before ſunſet, to a very large and beautiful rainbow, formed on a cloud which hung over the ſea, and from which a ſhower was falling at a conſiderable diſtance to the S. E. The ſun was about 2° high, ſo that the arch was not much leſs than a ſemicircle, with its higheſt point elevated about 40°. At the point where the northern extremity of this arch touched the horizon, another arch ſeemed alſo to ſpring from the ſea, diverging from the former at an angle of 3° or 4°, on the ſide toward the ſun.

Another interſecting bow, over the ſea.

* Edin. Tranſ. 1805. Hiſtory, p. 8.

This

This arch did not exceed 7° or 8° in length; it was of the same breadth with the principal bow; it had the colours in the same order, and nearly of the same brightness; or if any difference was discernible, it was, that the transition from one colour to another was not made with so much delicacy in the last-mentioned rainbow as in the former. It was a short portion,

We recollected that a phenomenon similar to this is described in the *Philosophical Transactions*, as having been seen at Spithead, and that it is ascribed by the gentleman who observed it to the reflection of the sun's rays from the surface of the sea, so as to fall on the cloud where the rainbow was formed. This hypothesis seemed to agree exactly with the phenomenon now before us. and was apparently formed by reflection of the sun's rays

The accidental rainbow, for so it may be called, was seen only at the extremity where the principal arch rose from the sea, and where of consequence, the sun's rays, reflected from the surface of the water, at that moment very smooth, might fall on the drops of rain. The other parts of the cloud could not receive rays so reflected, as the land intervened, and there, accordingly, no vestige of the accidental rainbow was observed. from the smooth water.

The accidental rainbow lay, as was already said, on the side toward the sun, and this is agreeable to the hypothesis; for the rays that after reflection from the surface of the water fell on the drops of rain, must have come as from a point as much depressed below the horizon, as the sun was at that instant elevated above it. The axis of the accidental rainbow must therefore have made with the axis of the principal, an angle equal to twice the sun's elevation, and its center must have been elevated by that same quantity above the centre of the other, so that if it had been complete, it would have been wholly between the principal rainbow and the sun. Its center was above the horizon;

The only cirumstance in which the appearances did not perfectly correspond with this hypothesis, was, that the two rainbows did not intersect one another in the horizon, but rather a little above it. This however, ought to have no great weight, as the reflected image of the sun cannot have presented to the cloud a disk so regular and well defined as the sun itself and the but the intersection was not quite as low as the horizon.

the accidental rainbow must have somewhat participated of this indistinctness *.

The inclination of the two arcs computed,

When phenomena of this kind occur, it would afford a sure means of trying the justness of the explanation, if the inclination of the two bows were observed, and also the sun's altitude at the same time. These two things are necessarily connected; for if we call I the angle of their intersection, E the elevation of the sun, and S the angle subtended at the eye by the semidiameter of the rainbow, if complete, an angle which is constantly the same, and nearly equal to 42°, it is easy to infer from spherical trigonometry, that $\sin \frac{1}{2} I = \frac{\sin E}{\sin S}$.

and was a little more than the estimate.

Computing from this formula, the inclination of the two bows in the present instance comes out nearly 5°; somewhat greater than I was inclined to estimate it by the eye.

Phenomena of this kind can but rarely occur, as the necessary conditions will not often come together. The principal rainbow must be over the sea; the sea itself must extend somewhat on the side toward the sun; it must be smooth and tranquil, and the sun so low that the light reflected from the water may be considerable. Were it ever to happen that the accidental bow was completely formed; the effect could not fail to be very striking.

* As the place of intersection will lie in a plane passing through the eye of the observer and parallel to the plane of reflection; does not this fact afford ground for a suspicion that the reflection, at this low altitude, was made, not from the surface of the sea, but from that of the stratum of vapour which occasions looming, and has been so well treated of by Dr. Wollaston and others, (see our Journal, VI. 46, and elsewhere), and that this stratum was higher farther out at sea than near the coast?—N.

Notice

XVIII.

Notice of a Collection of Memoirs which have lately appeared at Paris, being Part of a Work on which the celebrated Lavoisier was employed till the lamented Close of his Life; with a Translation of that Memoir, in which he claims the modern Theory of Chemistry as his own exclusive Discovery. Received from Mr. W. A. Cadell, at Paris.

To Mr. NICHOLSON,

SIR, *Paris, Oct.* 27, 1805.

I HAVE translated the two following passages (pages 4 and 5) from a work which has lately appeared in two volumes octavo, entitled *Memoires de Chimie*. They will prove interesting to the readers of your Journal. The first is the notice prefixed to the work by Madame Lavoisier, (now countess of Rumford) it is written with the eloquence of real feeling, and I refer to it for an account of the nature of the work; the second proves completely that the new theory of chemistry is due to M. Lavosier alone. I also send you the titles of the papers of which the work is composed. I am, See introductory letter.

Your very humble servant,

W. A. CADELL.

CONTENTS OF THE TWO VOLUMES.

PART. I.—*General Views on Caloric: its Effects; the Manner of measuring it, and the Formation of Liquids and Fluids.*

1st mem. on caloric, by *Lavoisier*. Mem. Ac. des Sci. 1777. Contents of the memoirs arranged by Lavoisier.

2. On caloric, and the means of measuring its effects. *ib.* 1780. *Lavois. et Laplace.*

3. Supplement to the preceding. *Lavois. and Laplace.*

4. On some of the principal phenomena of chemistry.—*Seguin.* Soc. Philom. 1790.

5. On the natural zero. *Seguin.* Annal. de Chim. 1790.

6. On the effects of heat in dilating metals and glass, &c. *Laplace and Lavoisier.*

7. On the passage of solids to a state of liquidity by means of heat. *Lavoisier.*

8. On the action of heat on liquids from their freezing point to that of their vaporization. *Lavoisier.*

On

Contents of the memoirs arranged by Lavoisier.

9. On the combination of heat with different evaporable substances and the formation of several fluids. *Lavoisier*. Mem. Ac. Sci. 1777.

10. On the electricity that is absorbed by bodies that pass to the state of vapour. *Lavoisier and Laplace*. M. A. S. 1781.

11. On the action of heat on some aerial fluids from the freezing to the boiling point. *Guyton and Duvernois*.

12. On some substances which are constantly in the state of aerial fluid at the ordinary temperature and pressure of the atmosphere. *Lavoisier*.

13. Memoir on some liqnids which can be obtained in an aerial form at a degree of heat a little above the mean temperature of the earth. M. A. S. 1777.

14. General views concerning the formation and constitution of the atmosphere.

15. On the cause of some of the principal phenomena of meteorology.

PART II. *On the Decomposition of atmospheric Air, its Analysis and the Conversion of its Principles into the solid or liquid State.*

SECTION I. *On the Decomposition of Air by metalic Substances and the Formation of Oxids.*

1. Memoir on the action of mercury upon atmospheric air. *Lavoisier*. In pars. in M. A. S. 1777, p. 186.

2. On the decomposition of atmospheric air by the oxidation of lead and tin performed by means of a burning glass under a glass receiver. *Lavoisier*. Opusc. Chim. chap. 6. pub. in 1773.

3. On the oxidation of tin in close vessels, &c. *Lavoisier*. Read in 1774.

4. On the decomposition of atmospheric air by iron. *Lavois.*

5. Historical details on the cause of the augmentation of weight that metals acquire when heated with contact of air. *Lavoisier*. It is the paper of which I send you the translation.

SECITON II. *on the Decomposition of Air by simple inflammable Substances which form Acids by their Combustion.*

1. Memoir on the decomposition of air by phosphorus, and the formation of phosphoric acid. *Lavoisier*. Opusc. Chim.

2. Sup-

2. Supplement to the preceding paper. *Lavosier* M. A. S. 1777. Contents of the memoirs, arranged by Lavoisier.

3. Proving that caloric disengaged from vital air during combustion is not possessed of weight susceptible of being estimated.

4. Process usually employed for obtaining phosphorus, phosphoric acid, and phosphoreous acid. *Seguin.*

5. Memoir on the combustion of phosphorus employed as an eudiometer. *Seguin.*

9. On the decomposition of air by sulphur, the formation of sulphureous and sulphuric acid, and the use of sulphurets in eudiometry. *Lavoisier.*

7. On the process employed in commerce to obtain the sulphureous and sulphuric acids. *Seguin.*

8. On the decomposition of air by charcoal and the formation of carbonic acid. *Lavoisier.* M. A. S. 1781.

2. On the formation of nitric acid by the immediate combination of azotic gas and vital air. *Seguin.*

10. On the eudiometer composed of nitrous gas. *Seguin.*

SECTION III. *On the Decomposition of Air by those simple inflammable Substances which do not form Acids by their Combustion.*

1. Mem. Account of the last experiments on the decomposition and recomposition of water. *Lavoisier* Journal Polytip. February, 1786.

2. Shewing that water is not a simple substance, but a binary combination of hydrogen and oxigen. *Lavoisier.* read in 1783.

3. Shewing by the decomposition of water that it is not a simple substance, and that there are several means of obtaining in abundance, the hydrogen gas, which is one of its elements. *Lavosier* M. A. S. 1781.

4. Report on the paper of Seguin, which treats of the combustion of hydrogen gas with vital air. *Lavoisier, Laplace, &c.*

5. On the combustion of hydrogen gas in close vessels. *Fourcroy, Vauquelin,* and *Seguin,* read in 1790.

PART IV. *On the principal Phenomena of the Animal Œconomy.*

1. Mem. Experiments on the respiration of animals, and the change which takes place in air in the lungs. M. A. S. 1777. p. 185.

2 2. The

2. The alterations that the air undergoes during respiration. *Lavoisier*, read in 1785.

3. Memoir, report on a paper of Seguin's concerning respiration and animal heat. *Maquer and Fourcroy*.

4. On respiration and animal heat. *Seguin*.

5. On the respiration of animals. *Seguin and Lavosier*, read in 1790.

Notice prefixed to the Work (by (Mad. Lavoisier) countess of Rumford.

Intention of Lavoisier to republish his memoirs,

In the year 1792 M. Lavosier had formed the design of making a collection of all his memoirs which had been read at the academy during the twenty years preceding. This would have formed in some degree the history of modern chemistry.

In order to render this history more interesting, and more complete he had proposed to insert the memoirs of those, who having adopted his theory, had published experiments in support of it.

in eight volumes.

This collection was to have been comprised in about eight volumes.

All Europe is acquainted with the cause which prevented their completion.

Parts recovered.

The portion that have been recovered are, the first volume almost entire, the whole of the second, and some sheets of the fourth.

Several men of science expressed a desire for their publication: this was received with hesitation—it is difficult not to be under apprehensions when we are intrusted with the power of publishing the unfinished work of a man justly celebrated. When we have lost the object of our affections and veneration, we should employ an impartial criticism, in order to offer to the public those of his works only which may augment his fame.

Madame Lavoisier has printed them.

We should have persisted, and these fragments would not have appeared, had they not contained a memoir of M. Lavoisier (inserted below page 5) in which he reclaims the modern theory of chemistry as belonging to himself, and states the facts in support of his claim.

It is consequently a duty towards him to fix the opinion of men of science concerning this point.

Lavoisier was employed on this

Their indulgence is requested for the errors that may exist in some other parts of the collection. It will be granted whe

they are informed that the greatest part of the proof sheets were revised in the last days of the author's life; and that whilst he knew that his assassins were premeditating his death, M. Lavosier, calm and intrepid, employed his last moments in a work which he considered as useful to science, and gave a great example of that serenity which a virtuous and enlightened man can preserve in the midst of the most severe calamities.

work in the last moments of his life.

PART II.—SECT. I. *Fifth Memoir*, (Tom. II. p. 78.)

Historical Details concerning the Augmentation of Weight which the Metals acquire when heated with Contact of Air. (By Lavoisier.)

IT is not my object in this paper to give a compleat history of the different opinions that have been successively adopted by the chemists and natural philosophers on the cause of the augmentation of weight in metals exposed to the action of heat; such a history would only serve to shew how much the minds of men are susceptible of being led astray when they give themselves up to the spirit of theory, and how easily we are deceived by reasoning, if it is not perpetually rectified by experiment. John Rey, a physician *(medecin)* little known is one of the first authors who has written on this subject; he lived in the beginning of the 17th century at Bugue in Perigord, and kept a correspondence with the small number of persons who cultivated the sciences at that time.

Limit of this historical memoir.

John Rey an early writer on combustion.

Neither Descartes nor Pascal had yet appeared; the vacuum of Boyle, and that of Toricelli, the cause of the ascent of liquids in tubes void of air, were unknown; experimental philosophy did not exist; a profound darkness reigned in chemistry. Nevertheless, J. Rey, in a work published in 1630, with a view of determining the cause of the augmentation of weight which takes place in lead and tin during their oxidation, displayed views so profound, so analogous to the facts which have been since confirmed by experiment, so conformable to the doctrines of saturation and affinity, that for a long time I could not help suspecting that the essays of J. Rey had been composed at a much later period than that announced on the title page of the book.

His philosophy far exceeded that of his cotemporaries.

J. Rey, after having refuted successfully, not by facts (for at that time the art of making experiments was in its infancy) but by conclusive reasoning, the different causes to which the aug-

He contends that metals gain weight from the air in oxidation.

augmentation of weight of metallic oxides might be attributed, expresses himself as follows in his 16th essay: "to this question then, supported on the grounds already mentioned, I answer and maintain with confidence, that the increase of weight arises from the air of the vessel, which is condensed, rendered heavy, and adhesive, by the violent and long continued heat of the furnace; this air mixes itself with the calx (frequent agitation conducing) and attaches itself to the minutest molecules, in the same manner as water renders heavy sand which is agitated with it, and moistens and adheres to the smallest grains.

He opposes other current opinions.

J. Rey combats in this work the opinion of Cardan (*lib.* 5 *de subtilitate*) on the augmentation of weight of metallic oxides; that of Scaliger, that of Cœsalpinus, who ascribed this augmentation to a soot condensed and reflected by the furnace, which soot, according to their opinion, fell down upon the metal. He shews likewise that the augmentation of weight proceeds neither from the vessel, nor from any emanation of the charcoal, nor from the humidity of the air. It is difficult to conceive how J. Rey could attain to these conclusions by the force of reasoning alone, without experiment, and ignorant as he was of many of the preliminary date.

His doctrines were not received by Boyle,

It appears that towards the end of the last century, when Boyle and some of his cotemporaries created the new science of natural philosophy, of which the ancients had not the slightest notion, the work of J. Rey was entirely forgotten.— Boyle, in his treatise on the weight of flame and of fire, published in 1670, that is 40 years after the publication of Rey's work, makes no mention of it; proceeding upon some illusory experiments, he still maintained at that time that the augmentation of weight which the metals acquire by their oxidation arises from the fixation of fire.

—nor by Lemery.

Lemery, who was an exact and scrupulous observer, embraced the same opinion: he attributes the oxidation of metals and their augmentation in weight which accompanies that operation, to the combination of igneous particles with the metal.

Opinion of Charras,

Charras, cotemporary of Lemery, ascribed that augmentation to the acids of the wood and charcoal, which as he supposed penetrated the vessels and entered into combination with the metals. Since that time the same acid of wood and charcoal

coal has re-appeared under the name of *acidum pingue*, igneous acid, and under ſeveral other denominations which it would be ſuperfluous to enumerate.

—and of Staahl

Staahl could not be ignorant of the fact that metals expoſed to heat acquire an increaſe of weight; and yet he not only did not attempt to explain it, but alſo the ſyſtem under which he claſſed the whole of the chemical phenomena, and which after him has been ſo much extended, is abſolutely in contradiction with this capital fact.

Staahl ſuppoſed that the metals are compoſed of a metallic earth, and an inflammable principle, which he named phlogiſton; he pretended that they loſt this principle by their oxidation, and that they could not return to the metallic ſtate unleſs the phlogiſton they had loſt was reſtored to them.

Difficulties of the phlogiſtic ſyſtem.

It was difficult to imagine how the metals acquired weight, whilſt, according to Staahl's doctrine, they loſt a part of their ſubſtance; and on the other hand, how they diminiſhed in weight, when they recovered one of the principles which they had loſt; it was one of the chief difficulties that could be propoſed againſt the theory of Staahl, this difficulty however, has not hindered the theory from having a ſucceſs of limited duration.

Morveau's endeavours to remove them.

Guyton Morveau has made ſome unſucceſsful efforts to palliate this contradiction, in his diſſertation on this ſubject, under the title of *Degreſſions Academiques*; he ſuppoſes that phlogiſton is lighter than atmoſpheric air; and he concludes that all bodies that acquire phlogiſton ſhould loſe a part of their weight; that, on the contrary, thoſe which loſe phlogiſton ſhould augment in weight. This explanation would have been tenable, had the augmentation of weight acquired by the metallic oxides been equal only to the weight of the air diſplaced; or, which is the ſame thing, if it had diſappeared on weighing in vacuo; but the augmentation is much too conſiderable to admit of being attributed to that cauſe, ſince in ſome metals it exceeds one third of their weight. It is neceſſary then, either to give up the explanation of Guyton Morveau, or to ſuppoſe that phlogiſton has a negative gravity, a tendency to recede from the centre of the earth, a ſuppoſition incompatible with all the facts admitted by the diſciples of Staahl.

Hiſtory of the authors experiments.

Such was the ſtate of the ſcience when a ſet of experiments undertaken in 1772, upon the different kinds of air or gas which

which are difengaged in effervefcence, and a great number of other chemical operations, difcovered to me demonftratively the caufe of the augmentation of weight that the metals acquire when expofed to heat. At that time I was not acquainted with J. Rey's work upon the fubject, publifhed in 1630; and had I known it, I fhould have confidered his opinion in the light of a vague conjecture, which did honour to the genius of the author, but required the attention of chemifts in order to afcertain the truth of the opinion by experiment. I was young, I had newly entered the lifts of fcience, I was defirous of fame, and I thought it neceffary to take fome fteps to fecure to myfelf the property of my difcovery. At that time there exifted an habitual correfpondence between the men of fcience of France and thofe of England; there was a kind of rivality between the two nations, which gave importance to new experiments, and which fometimes was the caufe that the writers of the one or the other of the nations difputed the difcovery with the real author; confequently I thought it proper to depofit on the 1ft of November 1772, the following note in the hands of the fecretary of the Academy. This note was opened at the meeting of the 5th of May following, and mention of thefe circumftances marked at the top of the note. It was in the following terms:

He finds that fulphur and phofphorus gain weight by combuftion, and that the gain is from air abforbed. He infers that the phenomenon is general and difengages air from litharge on reducing it in clofed veffels.

"About eight days ago I difcovered that fulphur in burning, far from lofing, augments in weight; that is to fay, that from one pound of fulphur much more than one pound of vitriolic acid is obtained, without reckoning the humidity of the air; phofphorus prefents the fame phenomenon; this augmentation of weight arifes from a great quantity of air, which becomes fixed during the combuftion, and which combines with the vapours."

"This difcovery, which I confirmed by experiments which I regard as decifive, led me to think that what is obferved in the combuftion of fulphur and phofphorus might likewife take place with refpect to all the bodies which augment in weight by combuftion and by calcination, and I was perfuaded that the augmentation of weight in the calces of metals proceeded from the fame caufe. The experiment fully confirmed my conjectures; I operated the reduction of litharge in clofe veffels with Hales's apparatus, and I obferved that at the moment of the paffage of the calx into the metallic ftate, there was a difengage-

disengagement of air in considerable quantity, and that this air formed a volume at least 1000 times greater than that of the litharge employed. As this discovery appears to me one of the most interesting which has been made since Staahl, I thought it expedient to secure to myself the property, by depositing the present note in the hands of the secretary of the academy, to remain secret till the period when I shall publish my experiments."

(Signed) LAVOISIER.

Paris, 1st November, 1772.

Comparing this first note with that which I had deposited at the academy the 20th of October preceding on the combustion of phosphorus, with the paper which I read at the academy of the public meeting of Easter, 1773, and lastly with those that I have published successively, it is easy to perceive that I had conceived so early as the year 1772 the general idea of the theory of combustion which I have since published.

Whence he vindicates his right to the modern theory of combustion in 1772.

This theory which I have considerably developed in 1777, and which almost at that period I brought to the degree of perfection in which it is at present, was not begun to be taught by Fourcroy till the winter 1786-1787; it was not adopted by Guyton Morveau till a later period; and Berthollet wrote still in the language of the phlogistic doctrine in 1785. This theory then is not, as I hear it called, the theory of the French chemists; it is *mine*, and it is a property which I reclaim before the tribunal of my cotemporaries and of posterity. Others undoubtedly have contributed to its perfection, but I hope that no one will dispute with me, all the theory of oxidation and combustion; the analysis and decomposition of air by metals and inflammable bodies; the theory of acidification; more accurate knowledge on the nature of a great many acids, and particularly the vegetable acids; the first notions on the composition of vegetable and animal substances; the theory of respiration, in which Seguin co-operated with me; the present collection will present all the papers on which I found my claims; the reader will judge.

—which was not adopted by other chemists till many years afterwards.

The claim specifically stated.

ANNOTATION.—W. N.

It was my intention to have pointed out how far the earlier chemists, as well as some of the contemporaries of this deservedly

Notice of the early invento

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of the theory of combustion.

edly celebrated philosopher, are intitled to rights which will greatly modify the unqualified claim he has made. I cannot now say, whether Rey did, or did not *make experiments*, but whether he did or not, he certainly must have founded his introductions upon facts; and between the observation of well established facts, and the making of direct experiments there seems to be no essential difference. How it has happened that the great Robert Hooke, who had investigated the modern theory of combustion in 1664 and published it in an ample detail on his micrographia in 1675*, and John Mayow, who soon afterwards, or about the same time established the same doctrine, and extended it to physiological results, are overlooked by our author, appears to require some discussion. I shall take an early opportunity of resuming this subject.

XIX.

On a Method of analyzing Stones containing fixed Alkali, by Means of the Boracic Acid. By Humphry Davy, Esq. F. R. S. Professor of Chemistry in the Royal Institution †.

Acid of borax very useful in analysis.

I HAVE found the boracic acid a very useful substance for bringing the constituent parts of stones containing a fixed alkali into solution.

It combines with earths by ignition and quits them to mineral acids.

Its attraction for the different simple earths is considerable at the heat of ignition, but the compounds that it forms with them are easily decomposed by the mineral acids dissolved in water, and it is on this circumstance that the method of analysis is founded.

Processes.

The processes are very simple.

Pulverize the stone and fuse with two parts boracic acid.

100 grains of the stone to be examined in very fine powder, must be fused for about half an hour, at a strong red heat, in a crucible of platina or silver, with 200 grains of boracic acid.

Digest with weak nitric acid.

An ounce and half of nitric acid, diluted with seven or eight times its quantity of water, must be digested upon the fused mass till the whole is decomposed.

Evaporate.

The fluid must be evaporated till its quantity is reduced to an ounce and half or two ounces.

* Copied in our Journal quarto series III. 479.

† Phil. Trans. Part II. for 1805.

If

If the stone contain silex, this earth will be separated in the process of solution and evaporation; and it must be collected upon a filter, and washed with distilled water till the boracic acid and all the saline matter is separated from it. Silex if present will separate.

The fluid, mixed with the water that has passed through the filter, must be evaporated, till it is reduced to a convenient quantity, such as that of half a pint; when it must be saturated with carbonate of ammonia, and boiled with an excess of this salt, till all the materials that it contains, capable of being precipitated, have fallen to the bottom of the vessel. Precipitate the rest with carbonate of ammonia.

The solution must then be separated by the filter, and the earths and metallic oxides retained.

It must be mixed with nitric acid till it tastes strongly sour, and evaporated till the boracic acid appears free. Add nitric acid to the clear liquid.

The fluid must be passed through the filter, and subjected to evaporation till it becomes dry; when, by exposure to a heat equal to 450° Farenheit, the nitrate of ammonia will be decomposed, and the nitrate of potash or soda will remain in the vessel. Separate the boracic acid by evaporation. Decompose the nitrate of ammonia by heat.

It will be unnecessary for me to describe minutely the method of obtaining the remaining earths and metallic oxides free from each other, as I have used the common processes. I have separated the alumine by solution of potash, the lime by sulphuric acid, the oxide of iron by succinate of ammonia, the manganese by hydrosulphuret of potash, and the magnesia by pure soda.

XX.

Some Facts and Speculations on the luminous Phenomena of Electricity. W. N.

ABOUT eighteen years ago, I was considerably occupied in experiments upon electricity, many of which were communicated in 1789, to the Royal Society, and were published in the transactions. In the twenty-third section of that communication, some account is given of certain changes which take place in the luminous appearance of metallic balls when electrified; but the phenomena were not delineated, because I reserved them for another opportunity. After so long an interval Communication to the Royal Society on electricity.

terval of time, I now present them to the reader from my notes, and the sketch then made.

Three appearances of an electrified ball. It gives flashes; is then luminous, and then gives flashes of another kind.

Sept. 19, 1787. A small ball in the state of electricity called positive, threw out flashes or ramified sparks; and when the intensity was encreased, the ball itself became luminous, at the same time emitting the flashes. When the electricity was still more strongly excited the flashes ceased, and a circle of light, extending about 45 degrees round the point farthest from the stem, was seen on the ball, and a strong wind proceeded from it.

Experiment with a ball 1½ inch diameter.

A ball of one inch and a half diameter was used; and electricity communicated by means of a cylinder nine inches diameter, having its cushion eight inches long. The excitation was strong enough, by slow turning with a single winch, to throw out large brushes of light. When the rotation was quicker, the flashes disappeared, and the circle of light was seen, having a bright speck moving irregularly round in its periphery. Quicker turning threw out brushes of light very different from the others: These were less luminous in the branches; many started out at once with a hoarse sound. They were greenish at the point or surface of the ball, reddish in the stem, and ramified sooner. Half a dozen were sometimes seen flashing out at once.

Description.

Experiment with a much smaller ball. It became luminous, and acted like a point.

A ball of four tenths of an inch in diameter was used. Moderate excitation produced a dense brush of light about two inches in length. With stronger electricity the brush disappeared, and the upper half of the ball became luminous. When the excitation was still stronger, more than half of the ball was luminous, as represented *Fig.* 3, *Plate I.* and sometimes a ramified flash struck out from the top. Other flashes were sometimes seen sideways when the electricity was strongest of all; but this happened seldom.

The light was faint, and seemed to be about twice the diameter of the ball. It extended more than half way down, and spread most sideways.

A large ball 2½ inch. diam.

When a larger ball of two and a half inch diameter was used, the brushes of light flew out from three or four stems together to the length of about six or seven inches, making a hoarse noise; but they could not be made to disappear, though they seemed now and then to cease for a moment when the turning was most vigorous.

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The next day, when the excitation was very nearly, but not quite, as ſtrong, it was obſerved that the order of theſe appearances could be effected by the affiſtance of a metallic point. Plentiful bruſhes were thrown out from a three inch ball, but they could not be made to diſappear. When a pointed wire or a ſmall metallic ball was preſented, the effects were as follow: Modification of the phenomena,

The point being at a great diſtance, the root of the bruſh had a luminous circle of lambent light round it on the ſurface of the ball. When the point was nearer, the bruſh diſappeared, and nothing was ſeen but an exceedingly bright ſpeck on the ſurface of the ball, which was ſometimes ſtationary and ſometimes moved about. When the point was ſtill nearer, the ſpeck threw out ramified ſparks of the ſecond kind, at the ſame time that a lambent luminous circle appeared. The ſpeck was never in the center of the circle, but moved at a diſtance round the circle, irregularly, ſometimes the one way and ſometimes the contrary, and was ſometimes ſtationary. by the vicinity of a point or ſmall ball.

Theſe two orders of bruſhes were entirely the ſame as thoſe of the day before. The luminous bruſh which firſt appeared had a ſtraight ſtem, then a broken or leſs luminous part, beyond which looſe cotton-looking fibres flew off in radial directions, as at *Fig.* 1, *Pl. I.* The latter ramified ſparks had a ſtraight central ſtem; out of which well defined branches iſſued nearly at right angles. They much more cloſely reſembled a tree bare of leaves. More particular deſcription of the luminous appearances.

The ſecond bruſh was not larger, but rather leſs in its dimenſions than the firſt.

When the ball of four-tenths of an inch was held at a certain diſtance from the two and a half inch ball, when electrified, the firſt kind of bruſh was ſeen on the ſide fartheſt from the ſmall ball, at the ſame time that the ſecond kind of ſpark or bruſh flew out towards the ſmall ball, and the lambent luminous appearance was ſeen on the ſurface. All the phenomena viſible at once.

Theſe are the general facts; but I have no doubt but they would preſent many modifications upon being repeated.

Theſe facts may ſerve to aſſiſt our meditations with regard to the nature of the electric ſpark. In a late paper by Mr. Biot, given at page 214 of our Vol. XII. the author makes an ingenious conjecture, that the light and heat in this phenomenon may have been produced by mechanical compreſſion of Remarks on the electric ſpark.

Whether Biot's theory of luminous condensed air can be supported.

of the atmospheric air. Whether this supposition can be reconciled to the appearance of the spark in oil, and to some atmospheric phenomena, in which we are told of luminous balls moving apparently with little velocity through the air, and particularly that slowly-moving artificial fire-ball, produced once, and only once, by Warltire, as narrated in Priestley's work on air, may admit of question. When we consider that a particle of iron, cut off and set on fire in the common action of striking a light, appears, from the vivacity of its combustion, to be a body of considerable magnitude, though the usual quantity of metal would not form a ball of one thousandth of an inch in diameter; when we consider the prodigious elevation of temperature indicated by the explosion of wires of all metals by the electric shock, particularly in those beautiful and striking experiments which Van Marum has published; and lastly, when we call to mind that a metallic chain loses part of its weight every time a shock is passed through it, and that the spark is never seen to pass between incombustible bodies—considerable reasons will present themselves in favour of a modified supposition, that the electric spark may consist of, or be accompanied by, a portion of the body from which it proceeds.

Warltire's fire-ball.

Combustion of flint and steel, requires a very minute portion of metal:

Electric temperature is extremely elevated:

All metals lose by electricity, and the spark passes only between combustible bodies.

Hence probably it was part of the body set on fire.

Fire-Balls, &c. may be electric sparks;

Are not the atmospheric fire-balls or luminous meteors, the shooting stars and the stones which have fallen from the atmosphere, electric sparks upon a scale of immense magnitude?

and the spark a fire-ball.

If any luminous ball were to pass with a swift angular motion over the field of view, it would have the appearance of a line or streak of light. If it were to break in pieces many divergent streaks would be seen. May not the electric brush be a phenomenon of this description on a small scale?

Facts are more wanted than conjectures.

It would not be difficult to apply this speculation to the figures 1 and 2 before us; but as we are more in want of facts than of conjectures, and as it may be hoped that some of my readers who have the means and the time will pursue this investigation, I shall for the present conclude.

SCIENTIFIC

SCIENTIFIC NEWS.

Anatomical Cabinet.

THERE has appeared at Berlin, a complete defcription of the anatomical cabinet of M. Walter, which the king has purchafed, almoft a year ago, for the fum of 400,000 francs. This catalogue is compofed of fixty-two printed fheets. Anatomical cabinet.

Shower of Peas.

Dr. Hiem, of Berlin, has publifhed a note, in which he explains that the peas, which were faid to have fallen from the atmofphere in a fhower at Landfchut, in Silefia, were merely tubercles which are feparated from the roots of feveral plants. Thofe in queftion, according to the Doctor, were afforded by the roots of the aquatic plant *Ranunculus Ficaria.* He pretends that an enormous mafs of thefe tubercules may have been formed in certain cavities, whence they might be carried to a diftance by the whirl or eddy of ftrong wind. He fupports his opinion by the accounts of fhowers of this nature given by the celebrated Klaproth in his Journal of Chemiftry.— The Doctor concludes by remarking, that thefe tubercles contain a farinaceous fubftance equal in goodnefs to that of potatoes, and recommends an at tention to the ficaria for this purpofe. Shower of peas.

Univerfal Language.

THE Celtic academy, in a fitting of laft April, made proof of a new difcovery by one of its members; which gives the power of correfponding, and difcourfing, with men, whofe language is unknown, with expedition, without previous ftudy, any expence, the leaft trouble, or the fmalleft labour of the mind. The proof made at that fitting by twenty-five academicians, on the languages of Europe, afcertained, that by the aid of this invention, a man may travel any where without an interpreter, demand what he wants, difcourfe on whatever fubjects can intereft any fort of travellers, and even exprefs metaphyfical thoughts. It is intended to make this difcovery public at the return of the Emperor. Univerfal language.

The above account has appeared in ſeveral publications of credit, but it is probable the account is exaggerated in ſeveral reſpects.

Turkiſh Edict in Favour of Science.

Turkiſh edict in favour of ſcience.

THE Grand Seignor has conſtituted Prince Morouſi, by a diploma written with his own hand, director general of the hoſpitals of his empire, and inſpector of the ſchools of medicine, mathematics, and *belles lettres*, which his highneſs is engaged in founding with all poſſible diſpatch. This diploma is remarkable for the great praiſes of the ſciences made in it by the Grand Seignor; as they hitherto have been in no great favour with the Mahometans. In rendering juſtice to the ſkill of the Chriſtian phyſicians, who have ſtudied at the univerſities of Halle, of Padua, and of Montpelier, the Grand Seignour remarks with much truth, that theſe phyſicians, when brought into foreign countries, often commit great errors on account of the difference of the temperature of the climates; from whence he concludes that, in order to practiſe medicine well, it is neceſſary to ſtudy in the country where the profeſſion is to be exerciſed.

Coptic Manuſcripts.

Coptic manuſcript.

THE celebrated Daniſh antiquarian, M. Zoega, is daily occupied at Rome in completing his catalogue of Cophtic manuſcripts in the Borgheſe muſeum. He intends afterwards to publiſh a new topography of ancient Rome. It is probable this work will be printed in Germany, becauſe it will require numerous engravings, which no Italian bookſeller would chooſe to go to the expence of. It is, however, not believed that M. Zoega will occupy the profeſſor's chair, which has been granted him at the Univerſity of Kiel, as he is too much accuſtomed to the fine climate of Italy to leave it willingly.

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A
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AND
THE ARTS.

FEBRUARY, 1806.

ARTICLE I.

On the Cause of Fairy Rings. In a Letter from Mr. FLORIAN-JOLLY.

To Mr. NICHOLSON.

Assembly-house, Laytonstone, Essex,
January 13.

SIR,

SEEING by the letter of Mr. Gough inserted in the last Number of your Journal, that the cause of fairy-rings is not yet agreed upon among naturalists, I beg leave to submit to their consideration a few facts which I had occasion to remark some years ago, during a summer residence in Hampshire. The phenomenon of fairy-rings

The park of Broadlands, Lord Palmerston's seat, near Rumsey, was divided into three principal inclosures, formed by hurdles only. One of these had been lately mowed; there were cattle grazing in the next; and the other, which had afforded winter and spring fodder to some horses kept at grass, was then left to grow for an autumn crop. *This* last exhibited an immense number of fairy rings, some perfectly circular, some forming irregular curves, and others nothing but small round produced in great numbers in one division of Broadlands park and none in other parts.

round patches: In all of these the grass grew more luxuriant and of a deeper hue: No other fungus was to be found in any of them but the esculent mushroom. In the part lately mowed, and in that where the cattle were grazing, there was not the least appearance of fairy-rings.

Another field abounding with them.

In the course of subsequent perambulations, I observed in a grass field situated on the top of the first high ground upon the road from Rumsey to Salisbury, appearances nearly similar to those exhibited in the growing grass of the park. There had been all summer, and there were still horses grazing in this field: The fairy-rings were numerous, but the grass in the rings and patches, instead of being more luxuriant, was completely dry and blasted, and there grew two or three different fungi, all of them of those sorts which are reckoned noxious.

They were not produced by electricity,

That the fairy-rings at Broadlands were not the effect of electricity, appears to me beyond all doubt, since one part only of the park exhibited them, while the rest of the contiguous grounds, divided from that part by nothing more than a row of hurdles, did not shew any such appearance: otherwise it must be contended, that the electrical phenomena might take place on one side of the hurdles and never on the other, a predilection truly singular, and, I should think, difficult to be accounted for.

but by the excrement of the horses.

Another fact which I have repeatedly observed since that time, has led me to suspect that the fairy-rings, their different appearances, and the various species of fungi found in them, might be produced by no more uncommon cause than the excrements of the horses.

Argument from the appearances in hot-beds.

The hot-beds made of horse-dung, which I have had several times in my garden, have generally produced in succession the same fungi which are to be found in the different states of the fairy-rings. Whilst the beds are yet new, the fungi are of the same noxious species as I saw in the dry blasted fairy-rings, but when they grow cooler and more matured, esculent mushrooms begin to grow naturally, and although no spawn was ever put in the bed.

I have also remarked, that horse-dung produced in some seasons an immense quantity of mushrooms, and hardly any in others: This might perhaps be attributed to the different quality of the hay on which the horses had fed; and this might explain why fairy-rings are to be found in some pastures rather than in others.

That

That fairy-rings ſhould be produced by the excrements of horſes, may be illuſtrated by a very ſimple fact, which it is in the power of every perſon to obſerve. If you let fall ſome oil upon a marble ſlab, or ſome other liquid upon ſome ſubſtance that will imbibe it, you will ſee it gradually ſpread round in a more or leſs regular form; ſometimes aſſuming the appearance of a patch, and frequently continuing to flow from the center to the circumference, where it accumulates in a much greater proportion than in the inner part of the circle, taking thus the form of a ring. Experiment with oil, to illuſtrate the deductions.

This accumulation of the fluid at the circumference may be eaſily explained. As the fluid expands, the preſſure from the center becomes gradually leſs, till at laſt there is no ſufficient force to overcome the reſiſtance oppoſed by the dry parts of the ſolid ſubſtance which has imbibed it: yet, in conſequence of the firſt impulſe, the fluid will continue to flow from the center through the ſmall channels already opened, and will thus accumulate in greater quantities at the boundaries where its expanſive motion is ſtopped.

The excrements of horſes, diluted by the rains and imbibed in the ſoil, muſt have an effect ſimilar to that juſt deſcribed. This effect muſt, beſides, greatly depend upon the nature of the ſoil and the facility with which it is pervaded by the fluid; hence the conſtant appearance of fairy-rings in ſome paſture-grounds, while none are ever to be found in others. Application.

Should you, Sir, conſider theſe remarks, and the deductions which they have ſuggeſted to me, as likely to throw ſome light upon the cauſe of fairy-rings, you are welcome to make any uſe of them you may think proper.

I am, Sir,

Your obedient humble ſervant,

J. FLORIAN-JOLLY.

II.

Experiments on the Magnetifm of flender Iron Wires. *By* JOHN GOUGH, *Efq.*

To Mr. NICHOLSON.

SIR, *Middlefhaw, January* 9, 1806.

A general maxim in magnetifm ftated.

THE general phenomena of magnetifm have given rife to a maxim which fhall be here ftated in the words of a judicious writer on the fubject. "The magnetifm acquired by being placed within the influence or the fphere of activity of a magnet in foft iron, lafts only while the iron continues in that fituation; and when removed from the vicinity of the magnet, its magnetifm vanifhes immediately; but with hard iron, and efpecially with fteel, the cafe is quite different; for the harder the iron or fteel is, the more permanent is the magnetifm, which it acquires from the influence of a magnet." Cavallo on Magnetifm, London, 1787, p. 30.

Remarks on this maxim.

This propofition is of great utility in the fcience, for it explains a variety of relations betwixt the magnet and ferruginous bodies, but I have obferved one phenomenon that appears inexplicable on the principle, and confequently may be faid to offer one exception to the general propofition. As my experiments on the fubject are very eafy, it feems adviseable to deliver the leading circumftances in the form of fo many precepts because this method will affift any one defirous of purfuing the enquiry, to repeat them with eafe.

An experiment confiftent with the maxim.

Experiment 1. Apply either pole of a ftrong magnet to one end of a fhort horizontal bar of clean foft iron, and a particle of iron equally foft to the other end. This particle will remain fufpended at the extremity of the bar until the magnet is withdrawn; but the removal of this power will diffolve the connection fubfifting betwixt the two pieces of iron, and the particle will drop off immediately.

An experiment contradicting the maxim.

Exp. 2. The preceding experiment confirms the maxim ftated above, when conducted according to the foregoing directions; but let it be repeated with the following alteration, and it will contradict the general propofition. In place of the particle of foft iron, fubftitute a piece of iron wire of number 32 in the wire drawers fcale, the weight of which may amount to two or three grains. The removal of the magnet will

will not break the connection formed by its presence between the bar of soft iron and the wire; for the latter will remain attached to the end of the former, by the extremity which was first brought into contact with the iron; if the piece of wire be removed from the end of the bar, the magnetic connection may be revived by replacing it immediately. The same thing will happen if the wire be expeditiously transferred from the first bar to another rod of soft iron; but it loses its magnetism in the space of two or three seconds when kept at a distance from all ferruginous bodies which are capable of attracting it, and of being attracted by it. These facts prove wire of number 32 to be a magnet, the virtue of which is conditional, because its permanency depends on the presence of soft iron, and perhaps on no other circumstance; for the experiment may be repeated with success upon rusty wire of the same size, or on pieces which have been made red hot in the flame of a candle, or surrounded by sand in a crucible, in which situation they will cool much more gradually than when drawn singly through a flame.

Exp. 3. This capacity of iron wire to preserve the magnetism imparted to it, as long as it remains in contact with a bar of the same metal, is a property confined to certain sizes; for let the first experiment be repeated with a small piece of numbers 18 or 17, not equal to half a grain in weight, and just as it comes from the hand of the workman, this piece will perform the part of a particle of soft hammered iron, that is it will drop from the end of the bar, to which it has been attached by the application of a magnet, to the opposite extremity, as soon as the magnetic influence ceases to act upon it: consequently the mere operation of drawing soft iron into wire, by forcing it through a conical hole too narrow for its present diameter, will not convert it into a conditional magnet.

Wires of low sizes not conditional magnet.

Amongst other experiments relating to the subject, I took the trouble to examine the quality of every size from 32 to 21, both inclusive; the 11 smallest wires, the extreams of which were 32 and 22, were all conditional magnets; that is, they all adhered to the bar of soft iron, to which they had been previously attached, after the removal of the magnet. Number 23 supported seven grains of lead including its own weight, without the assistance of the magnet; No. 24, 6¼ nearly; No. 32, 4¼; No. 22, no more than two grains.

The lowest size capable of conditional magnetism ascertained.

As

As for number 21, it poffeffed the fimple properties of foft iron: for the fhorteft cylinder which could be taken from a rod of this fize by means of a cutting file, dropped from the end of the horizontal bar as foon as the magnet was withdrawn.

Remarks on Exp. 3d.

It is difficult to fay, which of the 11 wires mentioned above, had the magnetic virtue in the moft perfection, becaufe each piece differed in diameter from the reft; befides which, it is very well known, that a mafs of iron, of a weight and figure determinable by experiment only, is attracted by any particular magnet, more powerfully than any other mafs of the fame metal. But the preceding trials have difcovered one circumftance apparently of fome importance, for they fhew that wire is converted into a conditional magnet by its paffage through the 22 wordle, or wire drawers inftrument; and that the 23d operation brings this quality in it to perfection as far as we can judge from experiment.

I here only fpeak of wire drawn in Kendal, for I have been told, that the fame article manufactured in fome parts of Yorkfhire, has a much greater propenfity to become magnetical. This information was communicated to me by Mr. Morrice, a very intelligent fuperintendant of a manufacture of cards in this town; who moreover obferved, that wire of this defcription acquires a degree of magnetifm under the fhears, which induced him, when employed in working it, to fubftitute a brafs gauge for the common inftrument made of iron.

Conjectures relative to the caufe.

The magnetic property which commences with number 22, feems to be common to all the finer fizes, for I found it in the fmalleft wire I could procure, and which apparently did not exceed a ftrong human hair in thicknefs.

The foregoing experiments, befides proving that flender wires acquire a magnetifm which is permanent as long as they remain in contact with iron, alfo affords an exception to a fecond general maxim of the fcience, which afferts, that the permanency of communicated magnetifm depends on the hardnefs of the ferruginous body that receives it. This does not appear to be the cafe in experiment 2, in which wire of No. 32 did not lofe the faculty of being convertible into a conditional magnet after undergoing a red heat, a procefs that is well known to render wire very foft. I even repeated the experiment with the fame refult on all fizes betwixt 22 and

and 33, except 26; pieces of each fort were heated both in the flame of a candle, and in fand; all of which retained the faculty under confideration after being treated in both ways. In reality, wires that had been thus foftened, feemed to be in the fame condition with fmall nails of caft iron, confidered as retainers of magnetifm, though the latter are of a much harder quality; for a nail of the fort called fparrow-bills by fhoe-makers exhibited the appearances defcribed in the fecond experiment, after being filed down to the thicknefs of a fmall wire.

If then that kind of magnetifm which I have ventured to call conditional do not depend on comparative hardnefs, to what caufe is the phenomenon to be defcribed? little can be offered on my part, befides probable conjecture, in anfwer to this queftion. The temperature of wire is confiderably raifed during its paffage through the wordle; and may not we imagine with fome fhew of reafon, that this encreafe of temperature, joined to the fubfequent contact of cold air, produces a new arrangement of the molecules conftituting the wire which enables it to retain a portion of magnetifm as long as it remains in contact with a ferruginous body? if this fuppofition be true, experiment proves the new arrangement to take place in the 22 wordle; when the flendernefs of the wire will occafion it to cool fuddenly after paffing through the inftrument. The reality of fuch changes in the texture of bodies which are not in a ftate of fufion, is admitted at prefent by experimental philofophers. I may alfo quote in favour of this hypothefis fome valuable obfervations made by Gregory Watt, Efq, on the various degrees of magnetifm exhibited by the fame bazaltic ftone under different forms of cryftallization; which obfervations may be feen in your Journal for February, 1805.

Any attempt to explain the permanent magnetifm of fmall wires during their connection with foft iron, and the lofs of this property which enfues when the connection is broken, appears to be fuperfluous, becaufe the fact is evidently analogous to the well known method of adding ftrength to a magnet by a gradual encreafe of its load; for this operation, when judicioufly conducted, gives a magnetic charge to a bar of fteel already touched, which it cannot retain after the weight is removed.

I remain, &c.

JOHN GOUGH.

P. S. I neg-

P. S. I neglected to mention the following circumstance in the body of the letter. The drawing instrument, or wordle, is made of steel; and is it not probable that this tool, possessing a slight degree of magnetism given to it by friction or otherwise, assists in producing the necessary arrangement, by acting upon heated and slender wires, while their molecules are in a violent motion from the pressure of the instrument itself? This supposition has some claim to plausibility; because a weak magnet will impart a portion of the same virtue to a bar of tempered steel, the particles of which are in a state of vibration; for a rod of this metal will acquire a degree of polarity, provided it be struck on the end with a hammer when its axis lies parallel to the dipping-needle.

III.

Concerning the Differences in the magnetic Needle, on Board the Investigator, arising from an Alteration in the Direction of the Ship's Head. By MATTHEW FLINDERS, *Esq. Commander of his Majesty's Ship Investigator. From the Philosophical Transactions,* 1805.

The magnetic needle is affected at sea by the position of the ship's head:

WHILST surveying along the south coast of New Holland, in 1801 and 1802, I observed a considerable difference in the direction of the magnetic needle, when there was no other apparent cause for it than that of the ship's head being in a different direction. This occasioned much perplexity in laying down the bearings, and in allowing a proper variation upon them, and put me under the necessity of endeavouring to find out some method of correcting or allowing for these differences; for unless this could be done, many errors must unavoidably get admission into the chart. I first removed two guns into the hold, which had stood near the compasses, and afterwards fixed the surveying compass exactly a-midships upon the binnacle, for at first it was occasionally shifted to the weather side as the ship went about; but neither of these two arrangements produced any material effect in remedying the disagreements.

scarcely from the iron on deck.

The following table contains the observations for the variation of the compass in which the differences are most remarkable, and from which I shall beg to point out such inferences as I think may be drawn from them.

TABLE,

TABLE, shewing the Errors produced in the Magnetic Compass by the proper Magnetism of the Ship.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1801.									
Dec. AM	35° 5′ S	116° 28′ E	two	4 azimuths	binnacle	7° 0′ W	5° 59′ W	NW b. N	Commander
	Princess Royal	Harbour	three	6 —	on shore	6 15	6 23	—	
	—	—	theodolite	1 —	—	—	6 8	—	
1802.									
Jan. 9, PM	34 1	121 20	one	2 —	binnacle	5 0	9 22	ESE	
16, AM	Goose Island	Bay	one	2 —	—	4 0	0 54	W	
1803.									
May 20, AM	—	—	—	2 —	—	—	6 8	E	
1802.									
Jan. 18, PM	33 37	124 10	—	2 —	—	4 30	5 44	NNE	
20, PM	32 38	125 35	—	2 —	—	—	7 15	E b. N	
21, PM	32 30	125 48	—	2 —	—	—	4 45	S	
22, PM	32 30	126 7	—	2 —	—	4 15	6 13	NE b. E	
23, PM	32 21	126 33	—	1 amplitude	—	4 0	4 18	S b. E	
24, AM	32 5	128 15	—	2 azimuths	—	3 0	6 4	E b. N	
26, PM	32 15	128 15	three	6 —	—	—	3 7	S b. E	Lt. Flinders
30, AM	32 18	132 29	one	2 —	—	0 30	1 41	SSE	Commander
Feb. 4, AM	No. 4, bay	in island e	—	2 —	—	0 15	2 23	Easterly	
5, AM	32 39	133 55	—	1 amplitude	—	—	1 56	E b. S	
6, AM	32 36	133 58	three	6 azimuths	—	—	1 0 E	NW	Lt. Flinders
16, PM	34 3	135 20	one	2 —	—	1 5 E	1 33 W	SE b. E	Commander
—, PM	34 5	135 24	—	1 amplitude	—	1 5	3 56 E	SW	

TABLE, shewing the Errors produced in the Magnetic Compass by the proper Magnetism of the Ship.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1802.									
Feb. 18, AM	34° 50′ S	135° 32′ E	three	6 azimuths	binnacle	1° 12′ E	1° 12′ E	S	Lt. Flinders
Mar. 1, PM	In No. 10, bay		theodolite	1 —	on shore	1 39	1 39	—	Commander
5, PM	—	—	one	2 —	binnacle	1 39	0 53	S b. E	
17, PM	34 12	137 20	—	1 amplitude	—	2 15	4 38	SW b. S	Commander
18, PM	34 23	137 36	one	2 azimuths	binnacle	2 15	0 35	SE	
21, AM	35 33	137 15	three	6 —	—	2 40 E	1 10	SE b. S	
23, AM	Kanguroo Island		two	4 —	—	2 58	6 31	SSW	
26, AM	35 10	137 41	one	1 amplitude	—	2 45	1 49	NE b. N	
27, AM	35 21	137 52	one	1 —	—	2 50	1 49	SSE	
April 6, AM	Kanguroo Island		—	2 azimuths	on shore	2 58	2 58	—	
10, AM	35 47	139 15	—	1 amplitude	binnacle	3 0	5 11	W b. S	
11, PM	35 53	139 26	—	2 azimuths	—	3 0	0 50	SE	
13, PM	36 45	140 5	—	2 —	—	3 30	1 25	SE b. S	
16, AM	37 55	139 55	—	{2 — 1 amplitude}	—	4 15	2 20	—	
17, PM	37 57	139 56	—	2 azimuths	—	4 15	2 2	NE	
22, AM	39 38	144 50	—	2 —	—	7 45	11 52	WSW	
26, PM	38 35	144 25	—	2 —	—	7 30	3 41	NE b. E	
—, AM	38 38	144 35	—	1 amplitude	—	7 30	6 48	NE b. N	

Note. All the compasses made use of on board the Investigator were of Walker's construction, one excepted, which was made by Adams, and used only on July 22, 1801.

It

It is apparent that ſome of the obſerved variations in the above table are 4° leſs and others 4° greater than the truth; and it may be remarked, that when this error is weſtward, the ſhip's head was eaſt, or nearly ſo, and when it was eaſtward the head was in the oppoſite direction. When the obſervations agree neareſt with what was taken on ſhore, or with what may be deemed the true variations the ſhip's head was nearly north or ſouth; and a minute inſpection of the table will favour the opinion, that the exceſs or diminution of the variation was generally in proportion as the ſhip's head inclined on either ſide from the magnetic meridian.

The errors were about 4° each way, and the north end of the needle deviated as if repelled by the ſhip's head, &c.

After I had well aſcertained the certainty of a difference in the compaſſes, ariſing from an alteration in the point ſteered, I judged it neceſſary, when I wanted a ſet of bearings from a point where we tacked the ſhip, to take one ſet juſt before and another immediately after that operation: ſome ſpecimens of theſe here follow.

Other obſervations.

1802.		*Head* ESE.		*Head* SW b. W.
April 13th,	Le Geographe Rocks -	N 55° to 71° E		
11ʰ 32′ AM	Ξ point -	N 4 W	after tacking	N 9° W
—	Π point -	S 32 E	-	S 40 E.
		Head SE b. E.		*Head* W.
April 14th,	Π point rocky, inner part	N 39° E.	after tacking	N 30° E
9ʰ 29′ AM	——— projecting part	N 67 E	-	N 59 E
—	Furtheſt viſible extreme from deck -	S 51 E	-	S 55 E.
		Head ENE.		*Head* SW b. S.
April 15th,	Π, the weſtern part -	N 15° W.	after tacking	N 21° W
11ʰ 50′ AM	A peaked hummock -	N 19 E	-	N 15 E
—	Furtheſt extreme from deck	S 53 E	-	S 61 E
—	Centre of a naked ſandy patch -	E	-	E 5 N.

Variation per amplitude April 15, AM, taken with the ſurveying compaſs } 4° 8′ E, ſhip's head being S.

April

		Head E.		*Head* SW b. S.
April 15th, 5h PM	The peaked hummock	N 12° W.	after tacking	N 18° W
	Former extreme, a projection	S 59 E	-	S 64 E
—	Naked sandy patch, distant $3\frac{1}{2}$ -	N 33 E	-	N 31 E.

Limits of error in observing bearings on ship-board.

From some little change of place after tacking the ship, and from the part whose bearing was set not being perhaps the individual spot in both instances, the difference between the separate bearings in any set will not be always the same: to these causes for error also may be added inacurracies in taking the angles arising from the motion of the ship and compass, from the view of the object being obstructed by the rigging, masts, or ship's upper works, and from too much haste to get the bearings before the ship's place was materially altered. Even in the Table of azimuths and amplitudes greater accuracy than one degree must not be looked for; and in ship-bearings two or even three degrees is not, I believe, too great an allowance for error, unless in very favourable circumstances.

They may amount to two or even three degrees.

Results similar to those first stated.

Without attending to small differences, it is evident that the bearings correspond with the observation in requiring a less east variation to be applied when the ship's head was easterly, and a greater when it was to the westward, in order to get at the true direction of the object *. When examining the north and

* As a specimen of the plan I followed in protracting such bearings as the above, take the set of April 15, A M, when the true variation appears to have been 4° E. On the first bearing the ship's head was six points on one side of the meridian, and on the second it was three points on the other side, the mean is one point and an half on the east side; now for this one point and an half I allow 1° of error, which, as it is on the east side of the meridian, and the variation is easterly, must be subtracted: the variation then to be allowed upon the mean between the bearings before and after tacking will be 3° E, from which the true bearings will stand as follows:

April

and eaſt coaſts of New Holland, I always endeavoured to take the angles on ſhore with a Troughton's portable theodolite, and to obſerve for the variation in the ſame places, that all the errors might be done away or corrected; and as I was frequently fortunate enough to carry on my ſurveys in this manner for weeks together, inſtances that might corroborate or contradict the preceding remarks are neither very numerous or pointed; the following are the moſt remarkable:

April 15th, AM }	Π weſtern part - -	N 15° E
11h 50′ }	A peaked hummock - -	N 20 E
—	Furtheſt extreme from deck -	S 54 E
—	Centre of a naked ſandy patch -	E 0¼ S.

In the ſame manner upon ſingle ſets of bearings I was obliged to allow a variation different from what I ſuppoſed the true to be, unleſs the ſhip's head was nearly north or ſouth: but, that I might proceed as little upon conjecture as poſſible, I always endeavoured to get obſervations for the variation when the ſhip's head was in the ſame direction as when I had taken or wiſhed to take a particular ſet of bearings, and I then allowed that variation exactly, whatever it was. The perplexity ariſing from diſagreements in bearings was by theſe means much alleviated, and happy agreements were frequently produced, when, without ſuch corrections, there was nothing but diſcord.

TABLE

TABLE of observed Variations of the Compass, and of the Influence of the Ship's Position upon them.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1802.									
Aug. 5, PM	23° 51′ S	151° 42′ E	one	1 amplitude	binnacle	8° 0′ E	12° 7′ E	WSW	Commander
— AM	23 51	151 40	—	—	—	—	10 15	WNW	
12, PM	23 30	151 11	three	6 azimuths	—	—	6 50	SSE	Lt. Flinders
18, PM	23 23	151 16	one	2 —	—	7 45	7 52	W	Commander
31	22 23	150 38	two	4 —	—	7 30	4 49	E	
Sept. 6, AM	Upon Pier Head		theodolite	1 —	on shore	8 0	8 2	—	
Oct. 14, PM	20 44	150 42	one	1 amplitude	binnacle	7 0	6 40	SSE	Lt. Flinders
20, PM	19 22	148 40	—	1 —	—	6 0	5 39	S	Commander
21, AM	18 15	148 38	three	6 azimuths	—	—	5 42	N b. E	Lt. Flinders
Nov. 2, PM	10 50	142 32	one	2 —	—	4 0	3 32	E	Commander
7, AM	12 11	142 0	—	2 —	—	—	4 4	S	Lt. Flinders
9, PM	12 37	142 2	—	1 amplitude	—	—	5 24	W	Commander
1803.									
Jan. 3, PM	14 20	136 16	—	1 —	—	2 30	0 58	E	
7, PM	14 20	136 37	—	1 —	—	—	1 9	SE	
13, {PM AM}	13 38	137 20	—	2 —	—	3 0	3 47	Westerly	Lt. Flinders
14, AM	13 35	136 58	—	1 —	—	—	5 51	WSW	Commander
16, PM	In NW Bay (Gr. Eyl.)		theodolite	1 azimuth	on shore	—	3 6	—	
Feb. 3, AM	Arnhem S Bay		three	6 —	binnacle	2 20	2 26	NW b. W	
9, AM	—	—	theodolite	1 —	on shore	—	2 20	—	
Mar. 10, AM	11 5	134 15	one	2 —	binnacle	1 0	1 55	WNW	

In the latter of these observations, the differences arising from a change in the direction of the ship's head is less considerable than in the higher latitudes; indeed, on approaching the line of no variation upon the south coast, the differences in the variation were smaller than before and afterwards; but that these differences shall be greater in a large variation and smaller in a less, both places being equally distant from the magnetic pole, I will not venture to assert. The inferences that I think may be safely drawn from the above observations are as follows: 1st. That there was a difference in the direction of the magnetic needle on board the Investigator when the ship's head pointed to the east, and when it was directed westward. 2d. That this difference was easterly when the ship's head was pointed to the west, and westerly when it was east. 3d. That when the ship's head was north or south the needle took the same direction or nearly so that it would on shore; and shewed a variation from the true meridian, which was nearly the medium between what it showed when east and when west. 4th. That the error in variation was nearly proportionate to the number of points which the ship's head was from the north or south. Constant employment upon practice has not allowed me to become much acquainted with theories, but the little information I have upon the subject of magnetism has led me to form some notion concerning the cause of these differences, and although most probably vague and unscientific, I trust for the candour of the learned in submitting it, as well as the inferences above drawn, to their judgment.

General inferences. 1. The compass was found to deviate accordingly as the ship's head was easterly or westerly; the north end being farther from the ship's head than the truth. The deviation was proportional to the distance of the head from the N. or S.

1st. I suppose the attractive power of the different bodies in a ship, which are capable of affecting the compass, to be collected into something like a focal point or center of gravity, and that this point is nearly in the center of the ship where the shot are deposited, for here the greatest quantity of iron is collected together.

Theory proposed. That all the iron in the ship acts like one magnet,

2d. I suppose this point to be endued with the same kind of attraction as the pole of the hemisphere where the ship is; consequently, in New Holland the south end of the needle would be attracted by it and the north end repelled.

having a different polarity accordingly as the ship is near the N. or S. pole of the terrestrial magnetism.

3d. That the attractive power of this point is sufficiently strong in a ship of war to interfere with the action of the

 magnetic

magnetic poles upon a compaſs placed upon or in the binnacle.

If theſe ſuppoſitions are conſiſtent with the laws of magnetiſm, eſtabliſhed by experiments, I judge that they will account for all the differences above noticed; for the interference will neceſſarily be moſt perceptible upon a compaſs when the attractive point is at right angles to the magnetic meridian, that is, when the ſhip's head is eaſt or weſt, and will altogether vaniſh or become imperceptible when the attractive point and meridian coincide, or when the ſhip's head is north or ſouth. That the power of this point ſhould become leſs as the ſhip increaſes her diſtance from the magnetic pole has not indeed entered into my ſuppoſitions; but it may probably be true, and is indeed almoſt a neceſſary conſequence of the ſecond ſuppoſition. If the above hypotheſis, ſo to call it, be true, it muſt follow, that the differences in the variation of the magnetic needle, ariſing from a change in the ſhip's head, ought to be directly contrary to thoſe before recited, when the ſhip is on the north ſide of the magnetic equator, for the north point of the needle ſhould then be attracted, and the ſouth end repelled. I have no obſervations which are very deciſive upon this head, but thoſe that were taken on board the Inveſtigator ſeem to beſpeak that as it is ſo; they are as follow.

Inferences from the laſt ſuppoſition:

that the effects ſhould have a contrary direction in north latitude.

TABLE

TABLE of Observations to illustrate the foregoing Inferences.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1801.									
July 21, PM	Start Point	in sight to the NE	two	5 azimuths	binnacle	—	29° 34′ W	W	Mr. Thistle
—			one	1 amplitude	—	—	29 30	—	——
22, PM	49° 10′ N	5° 25′ W	two	4 azimuths	upon the booms in the middle of the ship	—	24 12	WNW	——
—, AM	18 15	6 45	one	1 amplitude		—	24 49	WSW	——
28, PM	38 1	14 20	five	10 azimuths		—	20 57	SW	——
—	—	—	—	11 —	binnacle	—	25 34	—	Commander
31, PM	Porto Santo	in sight to the NW	two	4 —	—	—	22 45	—	Mr. Thistle
—			—	4 —	booms	—	19 51	—	——
Aug. 24, AM	10 20	22 15	one	2 —	binnacle	—	12 45	SE b. S	Commander
29, AM	5 40	16 30	two	4 —	—	—	12 18	—	Lt. Flinders
Sept. 5, AM	2 15	14 00	—	3 —	—	—	14 54	WSW	Mr. Thistle

These

These obſervations, particularly thoſe of July 28, ſeem to be deciſive in ſhowing that the variation is more weſterly when taken upon the binnacle of a ſhip whoſe head is weſtward in north latitude, than when obſerved in the center of the ſhip, which is a ſtrong confirmation of the ſuppoſitions before given; but the obſervations on the change of the ſhip's head are too few to be ſatisfactory. Almoſt every ſea officer can tell whether he has obſerved the variation of the compaſs to be greater when going down the Engliſh Channel than when coming up it: and indeed it would be very eaſy for a ſhip lying in harbour to aſcertain the point beyond controverſy.

Remarks and obſervations on the ſame law.

Should this point be well eſtabliſhed, I think it would follow, that from a high ſouth latitude where the differences are great on one ſide, they are moſt likely to decreaſe gradually to the equator, and to increaſe in the ſame way to a high north latitude, where they are great on the other ſide; thus the ſmaller differences on the north coaſt of New Holland will be accounted for. I ſhall leave it to the learned on the ſubject of magnetiſm to compare the obſervations here given with thoſe made by others in different parts of the earth, and to form from them an hypotheſis that may embrace the whole of the phenomena: the opinion I have ventured to offer is merely the vague conjecture of one who does not profeſs to underſtand the ſubject.

Inſtances of the compaſs being affected by local magnetiſm.

Some account of the magnetiſm of Pier Head, upon the eaſt coaſt of New Holland, may not perhaps be thought an unappropriate concluſion to this Paper. I was induced to attend to this from the following paſſage in Hawkesworth, Vol. III. p. 126. "At ſun-riſe I went aſhore," ſays Captain Cook,

Pier Head.

"and climbing a conſiderable hill," Pier Head, "I took a view of the coaſt and the iſlands that lie off it, with their bearings having an azimuth compaſs with me for that purpoſe; but I obſerved that the needle differed very conſiderably in its poſition, even to thirty degrees, in ſome places more, in others leſs; and once I found it differ from itſelf no leſs than two points in the diſtance of fourteen feet*. I took up ſome

Local deviation of the compaſs as far as 50°.

* In a ſet of angles taken near the head of Arnhem north bay, on the weſt ſide of the gulph of Carpentaria, I found the needle of the theodolite had been drawn 50° from its proper direction. The ſhore conſiſted of grains of iron ore caked into a ſtony maſs; and a piece of it, when applied to the needle, drew it ſix or eight degrees

IV.

Letter from Mr. ROBERT HARRUP, *ſhewing that the Smut in Wheat exiſts in the Seed, and is greatly remedied by Lime ſteeping.*

To. Mr. NICHOLSON,

SIR, *Chobham, January* 7, 1806.

LITTLE converſant in agricultural affairs, I am yet to learn what enquiries have been made into the nature and cauſes of the diſeaſes of grain.

If the following communication on the diſeaſe of wheat, known by the name of *ſmut,* contains any thing new, or may lead to farther inveſtigation; an early inſertion of it will greatly oblige,

Sir,

Your obedient humble Servant,

R. HARRUP.

Different cauſes have been aſſigned for the production of ſmut; ſome ſuppoſing it to ariſe from too great an abundance of water ſhoots, others from intemperate ſeaſons.

Cauſes uſually aſſigned for the ſmut.

A writer in a reſpectable publication ſtrenuouſly contends in favour of the latter opinion.

He informs us, that brine, pickling, liming, change of ſeed, and ſeed of one year old, and upwards, avail nothing. In cold wet ſummers, ſays he, the ſmut prevails notwithſtanding the uſe of every means which invention hath urged or ingenuity practiced. After a number of obſervations, he continues, "to ſum up the whole of this matter, it ſeems as certain as demonſtration can render it, that the ſmut is not owing to any defect or imperfection in the ſeed, but entirely to ſome corrupt-creating principle in the atmoſphere, in the blowing ſeaſon, which blights and deſtroys the grain in ſome ſhape or other, according to the time it has been blowing, when it is ſtruck with the blight."

Intemperate weather; and that attention to the ſeed avails nothing.

On the contrary, it would appear from the accounts of thoſe who have the moſt frequent opportunities of making obſervations, that the primary cauſe of ſmut is in the ſeed. All the farmers I have converſed with on the ſubject, are decidedly

Practical men uſually aſcribe it to the ſeed,

decidedly of opinion, that ſmut in the ſeed will produce ſmut in the crop, unleſs certain means are uſed to prevent it. With this intention I have ſomewhere ſeen a variety of preparations recommended, in ſome of which arſenic was one of the ingredients. The farmers in this neighbourhood prepare their ſeed wheat in one or other of the following methods.

and uſe preparations.

Steeping in brine:

Formerly the wheat was immerſed about twelve hours in a ſtrong ſolution of common ſalt in water, and afterwards dried by mixing it with a ſufficient quantity of lime newly ſlaked.

wetting,

Of late years, in place of immerſing it, they pour a quantity of the ſaline ſolution over it on the floor, and after mixing the whole well together, dry it with lime as before.

or treatment with lime water only.

Another method which is now pretty generally adopted, is that in which no ſalt is uſed.

A quantity of boiling water is poured upon quick lime, and kept conſtantly ſtirred till the lime is reduced to powder, when it is immediately mixed with the grain. No great accuracy is uſed in aſcertaining the proportions; five or ſix pounds of lime, and three gallons of boiling water are about ſufficient to prepare five buſhels of wheat. In reaſoning *a priori*, one would be apt to ſuppoſe, that the vegetative powers of the grain would be materially injured by this boiling compoſition, but experience proves the contrary.

Experiment. Equal meaſures of ſound and ſmutty wheat were mixed. Half of this was ſteeped in brine for twelve hours, and half left unprepared.

Amidſt this diverſity of opinion on the cauſe of ſmut, I wiſhed to aſcertain the truth, if poſſible, by experiment. Accordingly, ſo early as December 1798, I mixed intimately together equal meaſures of ſound wheat and grains conſiſting entirely of ſmut. The heap was then divided into two equal parts; one of them was put into a ſaturated ſolution of ſalt in water for twelve hours, and then mixed with quick lime. The other part was ſubjected to no preparation whatever. Five or ſix days afterwards they were both planted in drills on a ſouth border, about nine or ten yards apart. Both parcels came up about the ſame time, and while in blade, no difference could be perceived. While the ear was yet inveloped by the blade, I cautiouſly opened ſeveral of both crops, and in ſome of that which had undergone no preparation, a conſiderable difference was obſervable. Some of the embrio grains were opened, and in place of a milky juice, they contained only a ſmall quantity of a whitiſh ſubſtance, in which, by

Both parcels were planted.

by the help of a common magnifier, I could readily diftinguifh from one, to three or four black fpecks in each. When rubbed between the fingers, a faint fmell of fmut was emitted. The ears which were examined, and had this appearance were marked, and afterwards proved to be fmut. When the crops came out in ear, it was eafy to diftinguifh the fmut from the wheat. At the time of blowing no bloffom whatever appeared on the fmut ears, and the weather proving tempeftuous at that time, the bloffom was frequently wafhed off the wheat ears by heavy fhowers, and as often renewed.

The unprepared feed gave an unhealthy product.

Smut eafily diftinguifhed.

Both pieces were cut at the ufual time, and upon a careful examination, *that which had been fubjected to no preparation confifted of nearly two-thirds of fmut ears, the remainder being tolerably good wheat. In that which had been prepared, not a fingle fmut-ball could be found.*

The prepared feed produced found grain, and the unprepared feed fmutty grain.

An accidental occurrence may be mentioned in corroboration of this experiment. Happening to pafs through a fmall field of wheat juft before the commencement of the harveft, I was ftruck with the unufual quantity of fmut in one part of it. On clofe examination, I found that this extraordinary crop of fmut ended abruptly in a line along one of the furrows. The other parts of the field had much the fame appearance of others in the neighbourhood; a few fmut ears fcattered through it. Upon enquiry I found, that the feed with which this field had been fown, running fhort, the piece fo abundant in fmut had been fown in feed which contained a confiderable quantity of fmut, and had undergone no preparation, only fprinkling it with a little flaked lime immediately before fowing.

An accidental occurrence of much fmut confined to a part of a field, which had been fown with unprepared feed.

The difeafe of fmut is entirely confined to the grain. The ftraw and every other part of the plant is found, and arrives at the natural fize. Smut ears are *ftaring*, and of a dirty whitifh colour, inclining to blue, at the time when healthy ears are of a bright yellow. Their odour is fœtid, and not inaptly compared to that of ftale lobfters. Part of an ear is not unfrequently found to contain fmut, while the other parts are filled with found wheat.

Smut affects the grain only.

Defcription of the fmut.

Difeafed grains have more the globular form than thofe of found wheat, which is perhaps the reafon why they are called fmut-balls. The fkin is fhrivelled and of a dirty brownifh

hue

hue, without any perforations which can be difcovered by a high magnifying power. The whole of their contents, in a recent ftate, are a blackifh foft fubftance with a few fhining specks, which difappear when dried.

The duft of fmut confifts of globules,

When kept fome time in a dry place, this foft fubftance is in the form of a fine duft or powder, of a dark brown colour when fpread out on glafs or talc. The microfcope fhews each of thefe minute particles to be well formed globules, fomewhat larger than the fanguineous.

heavier than water:

They are fpecifically heavier than water with which they readily mix but foon fubfides, fuffering no change by being kept in that fluid. In the beginning of September laft, I infufed fome of the powder in water in a watch-glafs. A few hours after I difcovered by the microfcope, in a drop of the fluid a few animalculæ. Upon examination next day every drop of the liquor contained innumerable animalculæ, generally very minute but fome a fize larger. After ftanding expofed fome days, the water evaporated, and an hour or two after the addition of frefh water every part fwarmed with animalculæ, moving nimbly in all directions. While viewing them in the microfcope they fuddenly became motionlefs owing to the evaporation of the drop of liquid; on adding a drop of frefh water, they inftantly revived and began the fame lively motion. A quantity of falt fufficient to faturate the water was then added to the mixture. Upon examination about twenty hours afterwards, I was much furprized to find the animalculæ as numerous and lively as before the addition of the falt.

in which they produce animalcules,

which are not killed by falt:

The watch glafs with its contents, after ftanding neglected, on a fhelf expofed to the effluvia of a variety of drugs, till the latter end of November, was again filled with water, and placed near a fire, placing at the fame time by it a fimilar glafs, containing fmut powder and frefh water. They were both frequently examined for fome days, but without difcovering any animalculæ. My attention being called off by other avocations they remained unnoticed about eight days. The glafs which contained the infufion with fimple water was quite dry, and only a fmall quantity of fluid remained in the other. A drop being examined in the microfcope by a fingle lens of a high magnifying power, was found to

to swarm with animalculæ. Both glasses were now filled with fresh water, and placed under inverted jars. Being examined two days after, each of them swarmed with lively animalculæ. While viewing them, *a small particle of lime water was added to the drop, which proved instantly fatal,* at least all motion ceased instantaneously, and was not renewed.

Lime water killed the animalcules from smut.

Among other inferences which may be drawn from the preceding facts and observations, are first, that the cause of smut is in the seed, and that smut produces smut in the crop. At the same time it is readily admitted, that certain seasons are more favourable to smut than others, which can only be considered as a secondary cause. 2. That lime used in the manner above mentioned, prevents smut, if not entirely, at least so far as not to prove injurious.

Inference. Smut is caused by bad seed, and lime water prevents it.

Is smut occasioned by animalculæ? Some of the foregoing facts seem strongly to favour the idea *.

V.

On the Discovery of Palladium; with Observations on other Substances found with Platina. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S* †.

HAVING some time since purified a large quantity of platina by precipitation, I have had an opportunity of observing various circumstances in the solution of this singular mineral, that have not been noticed by others, and which. I think, cannot fail to be interesting to this Society.

The principal subject of the present memoir is palladium.

* Mr. Nicholson will readily perceive that the subject is not near exhausted. If future investigation should present any thing worthy of communication on the subject, should Mr. N. deem such deserving a place in the Philosophical Journal, he has only to mention it in a marginal line.

Answer. The diseases of corn form a subject of such high importance, whether considered in an economical or scientific point of view, that I must consider it a duty to pay the most marked attention to whatever may tend to elucidate it.—N.

† Philos. Trans. 1805, p. 316.

As

As I have already given an account * of one product obtained from that ore, which I considered as a new metallic substance, and denominated Rhodium, I shall on the present occasion confine myself principally to those processes by which I originally detected, and subsequently obtained another metal, to which I gave the name of *Palladium*, from the planet that had been discovered nearly at the same time by Dr. Olbers.

In the course of my inquiries I have also examined the many impurities that are usually mixed with the grains of platina, but I shall not think it necessary to describe minutely substances which have already been fully examined by others.

§ I. *Ore of Iridium.*

Ore of iridium, resembles that of platina, but is insoluble in nitro-muriatic acid; grains, harder to the file, not malleable and peculiar in their fracture:

I must however notice one ore, that I find accompanies the ore of platina, but has passed unobserved from its great resemblance to the grains of platina, and on that account is scarcely to be distinguished or separated from them, excepting by solution of the platina; for the grains of which I speak are wholly insoluble in nitro-muriatic acid. When tried by the file, they are harder than the grains of platina; under the hammer they are not in the least degree malleable; and in the fracture they appear to consist of laminæ possessing a peculiar lustre; so that although the greater number of them cannot, as I have before observed, be distinguished from the grains of platina, the laminated structure sometimes occasions an external form by which they may be detected. With a view to be absolutely certain that there exist grains in a natural state, which have not been detached by solution from the substance of the grains of platina, I have separated from the mixed ore as many as enabled me to ascertain their general composition.

much heavier than the grains of platina; of which metal they contain none.

Their most remarkable quality is their great specific gravity, which I have found to be as much as 19,5, while that of the crude grains of platina has not, in any experiment that I have made, exceeded 17,7. From this circumstance it might naturally be conjectured that they contain a greater quantity of platina than the grains in general; by analysis, however, they do not appear to me to contain the smallest quantity of that metal, but

* See our Journal, IV. 107.

to

to be an ore conſiſting entirely of the metals that were found by Mr. Tennant in the black powder which is extricated by ſolution from the grains of platina, and which he has called Iridium and Oſmium. But, ſince the ſpecific gravity of theſe grains ſo much exceeds that of the powder, which by my experiments has appeared to be, at the utmoſt, 14,2. I have thought it might deſerve inquiry whether their chemical compoſition is in any reſpect different. For this purpoſe I have ſelected a portion of them, and have requeſted Mr. Tennant to undertake a comparative examination, from whoſe well known ſkill in chemical inquiries, as well as peculiar knowledge of the ſubject, we have every reaſon to expect a complete analyſis of this ore.

§ II. *Hyacinths.*

Very ſmall hyacinths found among the platina grains;

Among thoſe bodies which may be ſeparated from the ore of platina, in conſequence of their leſs ſpecific gravity, by a current of water or of air, there may be diſcerned a ſmall proportion of red cryſtals ſo minute, that 100 of the largeſt I could collect weighed ſcarcely $\frac{8}{10}$ of a grain. The quantity which I poſſeſs is conſequently too ſmall for chemical analyſis; but their phyſical properties are ſuch as correſpond in every reſpect with thoſe of the hyacinth. I was firſt led to compare them with that ſtone by their ſpecific gravity, which I conjectured to be conſiderable from their accompanying other ſubſtances, that appear to have been collected together ſolely by reaſon of their ſuperior weight.

Like the hyacinth, theſe cryſtals loſe their colour immediately and entirely when heated; they alſo agree with it in their hardneſs, which is barely ſufficient to ſcratch quartz, but is decidedly inferior to that of the topaz.

The principal varieties of their form may be very well underſtood by deſcription.

Varieties of their forms;

1ſt. In its moſt ſimple ſtate the cryſtal may be conſidered as a rectangular priſm terminated by a quadrilateral obtuſe pyramid, the ſides of which ſometimes ariſe direct from the ſides of the priſm; but,

2dly. The poſition of the pyramid is generally ſuch that its ſides ariſe from the angles of the priſm. In this caſe the ſides of the priſm are hexagons.

3dly. It

3dly. It is more usual for the prism to have eight sides by truncation of each of its angles, and at each extremity eight additional surfaces occupying the place of the eight linear angles between the prism and terminating pyramid of the second variety. The complete crystal has then thirty-two sides.

4thly. The eight surfaces last mentioned, as interposed between the prism and pyramid, are sometimes elongated into a complete acute pyramid having eight sides arising from the angles of an octahedral prism.

which prove the nature of the same.

The third form above described, corresponds so entirely with that given by the Abbé Haüy * as one of the forms of the hyacinth or jargon, that I have little reason to regret my inability to obtain chemical evidence of the composition of these crystals.

Those, and other impurities, I usually separated, as far as was practicable, by mechanical means, previously to forming the solution of platina, which has been the principal object of my attention.

§ III. *Precipitation of Platina.*

Account of the treatment of platina. After the first precipitation of it by sal ammoniac: another portion was thrown down by iron.

When a considerable quantity of the ore has been dissolved, and I had obtained, in the form of a yellow triple salt, as much of the platina as could be precipitated by sal ammoniac, clean bars of iron were next immersed in the solution for the purpose of precipitating the remainder of the platina.

This is called the first metallic precipitate, and was again dissolved as the first ore had been, and again precipitated by sal ammoniac.

For distinction it will be convenient to call this, which in fact consists of various metals, the first metallic precipitate. The treatment of this precipitate differed in no respect from that of the original ore. It was dissolved as before, and a portion of platina precipitated by sal ammoniac; but it was observable that the precipitate now obtained was not of so pale a yellow as the preceding. Nevertheless the impurity was in so small quantity, that the platina reduced from it by heat did not differ discernibly from that obtained from the purest yellow precipitate.

* *Traité de Mineralogie*, Pl. XLI. fig. 17. *Journ. des Mines*, No. 26, fig. 9.

At

At this time I found it advantageous to neutralize the solution with soda, and to employ a solution of green sulphate of iron for the precipitation of the gold, of which, I believe, a portion may always be obtained from the mixed ore; but I have observed in experiments upon any quantities of mere grains of crude platina carefully selected, that the smallest portion of gold could not be detected as a constituent part of the ore itself.

The solution was neutralized with soda; and gold, if present, precipitated by solution of green sulphate of iron.

Bars of iron were subsequently employed as before for recovering the platina that remained dissolved, together with those substances which I have since found to accompany it.

A second metallic precipitate was thrown down by iron.

The precipitate thus obtained, which I distinguish by the name of the second metallic precipitate, was to appearance of a blacker colour than the former, and was a finer powder.

As I was not at first prepared to expect any new bodies, I proceeded to treat the second precipitate, as the former, by solution and precipitation. But I soon observed appearances which I could not explain by supposition of the presence of any known bodies, and was led to form conjectures of future discoveries, which subsequent inquiry has fully confirmed.

When I attempted to dissolve this second metallic precipitate in nitro-muriatic acid, I was surprised to find that a part of it resisted the action of that solvent, notwithstanding any variations in the relative proportions or strength of the acids employed to form the compound, and although the whole of this powder had certainly been twice completely dissolved.

This was not all soluble in nitro-muriatic acid.

The solution formed in this case was of a peculiarly dark colour, and when I endeavoured to precipitate the platina from it by sal ammoniac, the precipitate obtained was small in quantity, and, instead of being yellow, was of a deep red colour, arising from an impurity which I did not at that time understand, but which we since know, from the experiments of Mr. Descotils, is occasioned by the metal now called iridium.

This solution was very dark and its precipitation by sal ammoniac was deep red, occasioned by iridium.

The solution, instead of being rendered pale by the precipitation of the platina, retained its dark colour in consequence of the other metals that remained in solution; but, as I had not then learned the means of separating them from each other, and as the quantity of fluid which accumulated occasioned me some inconvenience, I decomposed it by iron, as in

Precipitation of a third metallic precipitate by iron.

in the former inftances, and formed a third metallic precipitate, which could more commodioufly be referved for fubfequent examination.

Much of this being rhodium, was infoluble.

In this laft ftep I committed an error which afterwards occafioned me confiderable difficulty, for I found that a great part of this precipitate confifting of rhodium was unexpectedly rendered infoluble by this treatment, and refembled the refiduum of the fecond metallic precipitate abovementioned.

As I have already communicated to this fociety, in my Paper upon rhodium, the procefs by which I fubfequently avoided this difficulty, I fhall at prefent return to a previous ftage of my progrefs, and relate the means by which I firft obtained palladium in my attempts to analyze the fecond metallic precipitate.

§ IV. *Separation of Palladium.*

Separation of palladium. The fecond metallic precipitate contained lead, copper, and another metal precipitable by copper.

There was no difficulty in afcertaining the prefence of lead as one of the ingredients of this precipitate, by means of muriatic acid, which diffolved lead and iron and a fmall quantity of copper. It was equally eafy to obtain a larger portion of copper by dilute nitrous acid, with which it formed as ufual a blue folution. But when I endeavoured to extract the whole of the copper by a ftronger acid, it was evident, from the dark brown colour of the folution, that fome other metallic ingredient had alfo been diffolved. I at firft afcribed this colour to iron; but, when I confidered that this fubftance had been more flowly acted upon than copper, I relinquifhed that hypothefis, and endeavouring to precipitate a portion of it by a clean plate of copper, I obtained a black powder adhering to a furface of platina on which I had placed the folution. As this precipitate was foluble in nitric acid, it evidently confifted neither of gold nor platina; as the folution in that acid was of a red colour, the metal could not be either filver or mercury; and as the precipitation of it by copper excluded the fuppofition of all other known metals, I had reafon to fufpect the prefence of fome new body, but was not fully fatisfied of its exiftence until I attempted the precipitation of it by mercury.

It was feparated by agitating mercury with the folution with

For this purpofe I agitated a fmall quantity of mercury in the nitrous folution previoufly warmed, and obferved the mercury to acquire the confiftence of an amalgam. After this

5 amalgam

amalgam had been exposed to a red heat, there remained a white metal, which could not be fused before the blowpipe. It gave a red solution as before in nitrous acid; it was not precipitated by sal ammoniac, or by nitre; but by prussiate of potash it gave a yellow or orange precipitate; and in the order of its affinities it was precipitated by mercury but not by silver.

which it formed an amalgam; and the mercury was driven of by heat. It was palladium.

These are the properties by which I originally distinguished palladium; and by the assistance of these properties I obtained a sufficient quantity for investigating its nature more fully.

There were, however, various reasons which induced me to relinquish the original process of solution in nitrous acid and precipitation by mercury; for although I found the metal thus obtained to be nearly pure, the necessity of agitating the solution with the mercury was very tedious, and the waste was also considerable; for in the first place it seemed that nitrous acid would not extract all the palladium from any quantity of the second metallic precipitate, neither would mercury reduce the whole of what was so dissolved. I therefore substituted a process dependent on another of its properties, I had observed that this metal differed from platina in not being precipitated from nitro-muriatic acid by nitre or by other salts containing potash; for although a triple salt is thus formed, this salt is extremely soluble, while that of platina on the contrary requires a large quantity of water for its solution. On that account a compound *menstruum* consisting of nitrate of potash dissolved in muriatic acid is unfit for the solution of platina, but dissolves palladium nearly as well as common nitro-muriatic acid in which there is no potash present *.

The process with mercury was abandoned;

In five ounces of muriatic acid diluted with an equal quantity of water, I dissolved one ounce of nitre, and formed a solvent for palladium that possesses little power of acting on platina, so that by digesting any quantity of the second metallic precipitate till there appeared to be no farther action, I procured a solution from which by due evaporation were formed crystals of a triple salt, consisting of palladium combined with muriatic acid and potash. These are the crystals which I have on a former

and a solvent consisting of muriatic acid, with nitre was used to the second precipitation from which it takes palladium but not platina.

The solution gave crystals of triple salt of palladium potash and muriatic acid.

* I have found that gold may also be dissolved with equal facility by the same solvent, and nearly in the same proportion. Ten grains of nitre added to a proper quantity of muriatic acid are sufficient for sixteen grains of either gold or palladium.

former occasion * mentioned as exhibiting a very singular contrast of colours, being bright green when seen tranversely, but red in the direction of their axis; the general aspect, however, of large crystals is dark brown.

From the salt thus formed and purified by a second crystallization, the metal may be precipitated nearly pure by iron or by zinc, or it may be rendered so by subsequent digestion in muriatic acid.

§ V. *Reasons for thinking Palladium a simple Metal.*

That palladium is a simple metal appears from, its forming a distinctly crystallized salt with bases and an acid, its combinations with metals and separation without change. and its precipitation is reducible by mere heat.

From the consideration of this salt alone I thought it highly probable that the substance combined in it with muriate of potash was a simple metal, for I know of *no instance in chemistry of a distinctly crystallized salt containing more than two bases combined with one acid.* I nevertheless endeavoured by a suitable course of experiments to obviate all probable objections. After examining by what acids it might be dissolved and by what reagents it might be precipitated, I combined it with various metals, with platina, with gold, with silver, with copper, and with lead; and when I had recovered it from its alloys so formed, I ascertained that, after every mode of trial it still retained its characteristic properties, being soluble in nitrous acid, and precipitable from thence by mercury, by green sulphate of iron, by muriate of tin, by prussiate of potash, by each of the pure alkalis, and hydrosulphurets.

The precipitate obtained in each case was also found to be reducible by mere heat to a white metal, that, except in very small quantities, could not be fused alone by the blowpipe, but could very readily be fused with sulphur, with arsenic, or with phosphorus, and in all other respects resembled the original metal.

Qu. Whether it might consist of metal and a fixed acid?

The only hypothesis, on which I thought it possible that I could be deceived, arose from the recollection of the error, which subsisted for a few years, respecting the compound formerly called siderite. It was possible that some metallic or other fixed acid might unite too intimately with either a known or an unknown metal to be separated by the more common simple affinities. I consequently made such attempts as appeared best calculated to disunite a compound so constituted.

* Phil. Trans. 1804, p. 428.

Having

Having boiled the oxide with pure alkalis, and found it to be unaltered, I thought the affinities of lime or lead might be more likely to detect the presence of the phosphoric or of any known metallic acid; and accordingly I made various attempts by muriate and nitrate of lime, as well as by nitrate of lead, to effect a decomposition of the supposed compound. In the experiment on which I placed the greatest reliance, I poured liquid muriate of lime into a solution of palladium in nitro-muriatic acid, and evaporated the mixture to dryness, intending thereby to expel any excess of acid that might have been left in the solution, and to render either phosphate of lime, or any compound of lime with a metallic acid, insoluble in water. The residuum however was very readily dissolved by water, and consisted merely of muriate of lime and muriate of palladium, without any appearance of decomposition.

The oxide is not affected by boiling with alkalis; nor by pouring muriate of lime into its solution.

When I found all my endeavours directed to that end wholly unsuccessful, I no longer entertained any doubt of this substance being a new simple metal, and accordingly published a concise delineation of its character; but by not directing the attention of chemists to the substance from which it had been extracted, I reserved to myself an opportunity of examining more at leisure many anomalous phenomena, that had occurred to me in the analysis of platina, which I was at a loss to explain, until I had learned to distinguish those peculiarities, that I afterwards found to arise from the presence of rhodium.

Hence the discoverer was warranted in publishing it as a new metal.

§ VI. *Additional Properties of Palladium.*

In my former Paper on that subject I also added some observations upon the properties and origin of palladium, describing only such a mode of obtaining it from platina as should avoid the introduction of any unnecessary ingredient which might possibly be misinterpreted, and omitted one of the most distinguishing properties of palladium, by means of which it may be obtained with the utmost facility by any one who possesses a sufficient quantity of the ore of platina.

Method of easily separating palladium.

To a solution of crude platina, whether rendered neutral by evaporation of redundant acid, or saturated by addition of potash, of soda, or ammonia, by lime or magnesia, by mercury, by copper, or by iron, and also whether the platina has or has not been precipitated from the solution by sal ammoniac, it is merely necessary to add a solution of prussiate of mercury, for the

Prussiate of mercury added to the solution of crude platina, throws down the pure prussiate of palladium;

the precipitation of the palladium. Generally for a few seconds, and sometimes for a few minutes, there will be no appearance of any precipitate; but in a short time the whole solution becomes slightly turbid, and a flocculent precipitate is gradually formed, of a pale yellowish-white colour. This precipitate consists wholly of prussiate of palladium, and when heated will be found to yield that metal in a pure state, amounting to about four or five tenths *per cent.* upon the quantity of ore dissolved.

Heat disengages the pure metal which is not more than one two hund. part of the original ore.

The prussiate of mercury is peculiarly adapted to the precipitation of palladium, exclusive of all other metals, on account of the great affinity of mercury for the prussic acid, which in this case prevents the precipitation of iron or copper; but the proportion of mercury does not by any means influence the quantity of palladium, for I have in vain endeavoured, in the above experiment on crude platina, to obtain a larger quantity of palladium that I have stated by using more of the prussiate of mercury, or to procure any precipitate by the same means from a solution of pure platina.

More mercury does not augment the product.

The decomposition of muriate of palladium by prussiate of mercury is not effected solely by the superior affinity of mercury for the muriatic acid, but is assisted also by the greater affinity of prussic acid for palladium; for I have found that prussiate of palladium may be formed by boiling a precipitated oxide of palladium in a solution of prussiate of mercury.

The decomposition is by double affinity.

The prussiate of mercury is consequently a test by which the presence of palladium may be detected in any of its solutions; but it may be worth observing, that the precipitate obtained has not in all cases the same properties. In general, this compound is affected by heat similarly to other prussiates, but when the palladium has been dissolved in nitrous acid and precipitated from a neutral solution by prussiate of mercury, the precipitate thus formed has the property of detonating when heated. The noise is similar to that occasioned by firing an equal quantity of gunpowder, and accordingly the explosion is attended with no marks of violence unless occasioned by close confinement. The heat requisite for this purpose is barely sufficient to melt bismuth, consequently is about 500° of Fahrenheit. The light produced is proportionally feeble, and can only be seen in the absence of all other light.

Prussiate of mercury is the test of palladium.

The precipitation from a nitrous solution detonates by low heat.

In

In endeavouring to diſſolve a piece of palladium in ſtrong colourleſs nitric acid for the purpoſe of forming the detonating pruſſiate, I found that, although the acid ſhortly acquired a red colour ſurrounding the metal, the action of the acid was extremely ſlow, and I was ſurpriſed to obſerve a fact that appears to me wholly ſingular: the metal was taken up without any extrication of nitrous gas; and this ſeemed to be the cauſe of the ſlow ſolution of this metal, as there was not that circulation of this fluid, which takes place in the ſolution of other metals until the acid is nearly ſaturated.

Palladium is very ſlowly acted on by nitric acid and *extricates no gas.*

As the want of production of gas appeared to retard the ſolution of palladium, I tried the effect of impregnating a quantity of the ſame acid previouſly with nitrous gas, and obſerved its action to be very conſiderably augmented, although the experiment was neceſſarily tried in the cold, becauſe the gas would have been expelled by the application of heat.

Nitrous acid acts more ſtrongly.

Beſide thoſe properties which are peculiar to palladium there are others, not leſs remarkable, which it poſſeſſes in common with platina. I have on a former occaſion mentioned that theſe metals reſemble each other in deſtroying the colour of a large quantity of gold. Their reſemblance, however, in other properties is not leſs remarkable, more eſpecially in the little power they poſſeſs of conducting heat, and in the ſmall degree of expanſion to which they are liable when heated.

For the purpoſe of making a compariſon of the conducting power of different metals, I endeavoured to employ them in ſuch a manner, that the ſame weight of each metal might expoſe the ſame extent of ſurface. With that view I ſelected pieces of ſilver, of copper, of palladium, and platina, which had been laminated ſo thin as to weigh each 10 grains to the ſquare inch. Of theſe I cut ſlips $\frac{4}{10}$ of an inch in breadth, and four inches long; and having covered their ſurfaces with wax, I heated one extremity ſo as to be viſibly red, and, obſerving the diſtance to which the wax was melted, I found that upon the ſilver it had melted as far as $3\frac{1}{4}$ inches: upon the copper $2\frac{1}{2}$ inches: but upon the palladium and upon the platina only one inch each: a difference ſufficient to eſtabliſh the peculiarity of theſe metals, although the conducting power cannot be ſaid to be ſimply in proportion to thoſe diſtances.

Conducting powers of palladium and platina as to heat, tried by the melting of wax upon them. The meaſure is not half thoſe of ſilver and copper.

In order to form ſome eſtimate of the comparative rate of expanſion of theſe metals, I rivetted together two thin plates of platina

Rate of expanſion by heat, tried by rivetting bars together.

While steel expands through 12, platina will expand 9, and palladium 10.

platina and of palladium; and observing that the compound plate, when heated, became concave on the side of the platina, I ascertained that the expansion of palladium is in some degree the greater of the two. By a similar mode of comparison I found that palladium expands considerably less than steel by heat; so that if the expansion of platina between the temperatures of freezing and boiling water be estimated at 9 parts in 10,000, while that of steel is known to be about 12, the expansion of palladium will probably not be much more or less than 10, or one part in 1000 by the same difference of temperature.

It must, however, be acknowledged, that the method I have pursued is by no means sufficient for determining the precise quantity of expansion of any substance; but I have not been induced to bestow much time on such an inquiry, since the extreme scarcity of palladium precludes all chance of any practical utility to be derived from a more accurate investigation.

VI.

Report made to the Athénée des Arts of Paris, by MM. RONDELET, BEAUVALLET, *and* DUCHESNE; *on the founding the Statue of* JOAN OF ARC *in Bronze, by a Way never before used for large Works, by MM.* ROUSSEAU *and* GENOT; *under the Direction of M.* GOIS, *Statuary.**

Casting in sand used hitherto only for small figures.

THE method of casting in sand hitherto has only been used for figures from 65 to 70 centimetres (about 2¼ feet) in heighth; while the statue, which was to be formed, being of much larger dimensions, should of course be managed according to the method called the *great foundery*, on account of its being used for colossal statues.

The great foundery known to the ancients, but was lost.

At its revival large statues were cast in separate parts.

This method of casting was known to the ancients, who were even superior to us in it; but this art was lost with many others, and in the time of the Medicis large statues were not formed at a single casting. The figures of Henry the Fourth and of Lewis the Thirteenth, which are seen at Paris, were

* Magazin Encyclopédique, T. I. p. 350.

placed

placed on horſes made previouſly, one for the ſtatue of Ferdinand, Grand Duke of Tuſcany, and the other for that of Henry the Second, King of France.

That of Lewis XIV. the firſt caſt after this in a ſingle piece.

The ſtatue of Lewis the Fourteenth, in the Place de Vendome, is the firſt that was formed at a ſingle caſting ſince the revival of the art. It was ſuitable to ſo great a prince to permit his image to be made ſolely by a grand method; but Girardon and Keller, to whom the work was entruſted, then made their firſt attempts, which occaſioned many faults, ſuch as the caſting it too thick, which in uſeleſsly employing more metal, increaſed the difficulty of ſupporting the coloſſal figure; and ſuch alſo as uſing unneceſſary labour; but notwithſtanding all their precautions, the caſting did not ſucceed perfectly, and conſiderable repairs were obliged to be made in it.

Various other ſtatues made in the ſame way.

About the ſame time were erected the equeſtrian ſtatues of this prince, at Boufflers and at Lyons, by the ſame Girarden, at Rennes by Coizevox, at Montpelier by Mazeline and Utrels, and at Dijon by Le Hongre.

The art not very perfect before the ſtatue of Lewis XV. was caſt.

After this, Le Moine had to found a ſtatue of Lewis the Fifteenth at Bourdeaux, which met with great accidents; but he had more ſucceſs with one at Rennes, which was a pedeſtrian ſtatue: Guibal alſo made one for Nancy. But this art did not attain to a great perfection till Boucharden was employed to conſtruct an equeſtrian ſtatue of Lewis the Fifteenth at Paris: The great care of M. Goor prevented any accident from happening to it, or to that alſo which was caſt at Reims by the ſame artiſt, from the model of Pigale. This founder had not the ſame ſucceſs when he formed the ſtatue of Frederic the Fifth at Copenhagen, from the model of Saly, which required great repairs. Finally, great improvements had been made in the art when the ſtatue of Peter the Great was founded at Peterſburg by Falconet, and nevertheleſs he was obliged to refound a ſecond time the upper half of the ſtatue.

Great improvements made in the art when the ſtatue of Peter the Great was caſt.

Diſadvantages of the great foundery.

The great diſadvantage of the method hitherto uſed, is its enormous expence and the great time it requires. It is true, that for works which are intended for duration economy is not the chief object; but if they can be performed equally well by M. Gois's method, at one-half the expence and in a fourth

M. Gois's method ſuperior to it in various reſpects.

Of

Reafons for believing that it would anfwer for the largeft works.

of the time, it certainly ought to be preferred. There is good reafon alfo to decide, that this method will do equally well for the largeft works; for, according to calculation, the largeft ftatue of this kind in France exceeds that of Joan of Arc by a much fmaller proportion than the latter exceeds that of the largeft ftatues ever before caft in the fame manner, which never weighed more than from 8 to 10 kilogrammes (from about 17 to 22 lb.) The ftatue of Joan of Arc weighed 600 kilogrammes, which is 60 times more; but that of Lewis the Fifteenth, which weighed 17,000 kilogrammes, was only twenty-eight times heavier than that of Joan of Arc.

But in order to judge better of the advantages of M. Gois's method, it fhall be defcribed at large, and an account given alfo of the method of molding by wax, or of the *grand foundery*, in order to compare them together.

M. Gois's ftatue of Joan of Arc exhibited and admired.

M. Gois having made a ftatue of Joan of Arc for a prize, exhibited it in public in the year 10, (1802.) The prefect of the department of the Loire faw it, and propofed to the city of Orleans to re-erect that monument to the glory of this heroine, which had been deftroyed in the anarchy of the revolution. It was accordingly ordered to be done. M. Gois being informed of this, went to Orleans and offered to make a caft in bronze from his ftatue, without precifely knowing whether it was that which the city required.

He is employed to make a caft in bronze from it for the city of Orleans.

An agreement was then made with M. Gois to complete the ftatue at a fixed price, in the courfe of about one year from the 5th Germinal, *An XI.* or before May 4, 1804.

Is induced to have the caft made in fand to fave time and expence.

M. Gois began to be alarmed at the enormous expence of the ufual method of cafting fuch ftatues, and at the great time it required, which he feared would prevent his performing the agreement. He knew that M. Rouffeau had made a caft from the groupe of Graces by Germain Pilon, with great fuccefs, by a different method; and though thefe figures were but 1,38 metres high, and his ftatue was more than two metres, ($6\frac{1}{2}$ feet) he went notwithftanding to confult this founder, who engaged for the fuccefs of the method, and promifed to employ in the bufinefs the fame workman who had caft the above groupe without having met with any accident: This laft confideration determined M. Gois to entruft the work to the *founders in fand.*

M. Rouffeau undertakes the work for him, who had before made a fine caft of the Graces; and employs the fame workman who caft them.

The

The first of Fructidor, *An XI.* the businefs was began; but as they commenced with the bas-reliefs, it was not till three months after that they undertook the work of the ftatue.

They made ufe of the common fand of the founders, which is argillaceous, and always kept a little moift. After having well raked it, feparated all the ftones, and broken all the lumps that could be met with, they filled with it a cafe of 2,20 metres long, and one metre broad at the infide, and 16 centimetres high; the thicknefs of the wood of the cafe was eight centimetres. The fand was ftrongly beaten with a rammer 10 centimetres broad and 60 long, and by this operation acquired fufficient confiftence to be raifed along with the cafe without any danger of running out. The procefs defcribed for cafting Joan of Arc in fand.

After this the ftatue was placed upon the firft cafe, which is called the *falfe mold,* becaufe it was to be afterwards replaced by another: the fand was ftirred up a little, to permit the moft prominent parts to enter it; another cafe of the fame fize was then put over the firft, and attached to it by four points of iron.

The true *concave mold* was then began, by modelling each part of the figure with the fame kind of fand. A workman of much addrefs and intelligence is requifite for the divifion and diftribution of the different pieces which form the mold: he fhould explain the motives which induced him to prefer one diftribution to another: each piece fhould have different fections: care fhould be taken to mold the parts which have a large and uniform furface in a fingle piece, while the pieces muft be multiplied for thofe portions of the ftatue which have many finuofities and deep indentations. Method of forming the mold.

This part of the operation requires the moft care; for if it be performed with negligence, the extraction of the model would be attended with great difficulties; and if the workman employed is aukward, numerous faults will need reparation after the cafting, and probably great accidents may happen. It is but juftice to fay, that Genon, the workman on this ftatue, fhewed in his performance equal dexterity and knowledge.

To prevent the pieces of the mold from adhering to each other, care is taken to powder the parts of each which is finifhed, with charcoal duft inclofed in a bag, before a new piece is began. The workman having finifhed the mold-

ings

iage of the contours of the figure, filled up the empty spaces between them and the case with sand, which he first pressed and forced together with his hands, afterwards beat it with the bat, and finally with a mallet; this compression gives it such a solidity that it appears like stone, or at least like baked earth.

The same care was taken with each case as they were successively added, to the number of seven; which compleating the top of the statue, the whole was then reversed in order to replace the lowest case, which, as mentioned, was only a false mold; and then each part at the lower extremity was also modelled in same manner as the preceding.

The hollow mold being finished, the cases were taken asunder, and each piece removed separately to take out the statue; then they were all placed in their proper order in the exterior mold, which may be compared to the cover used by those who make plaster-of-Paris casts. Each piece would be well retained in its place by its irregular form; but it was still farther fastened by a little thin paste made of flour, which was applied by a brush both to the pieces themselves and the parts that adhered to the cases. It was thought necessary to take a precaution more than what was usual, through the apprehension that the paste would not hold together those large pieces as well as it did the small pieces in lesser works; they were therefore traversed by long wires of iron, which entered into the cover or exterior mold.

The pieces of the mold cemented together with paste, and secured by wires passing into the cases.

This mold being thus entirely compleated, had only to be dried till the time of the casting. A new mold was necessary to be made to cast the core: the same pains were not taken with this as with the first, as it would be useless to do so. When this second mold was finished, a coat of modelling-clay was applied to its inside, of the same thickness which was intended to be given to the bronze; and without waiting for its drying, it was closed and the core cast in it, which was composed, as is usual, of equal parts of plaster of Paris and of brick dust.

Method of forming the core.

Eight rods of iron having been placed at the inside of the mold, afterwards projected from the core about 10 or 12 centimetres, which served to place it with precision in the hollow mold.

Eight iron rods laid so as to project a few inches from the core, in order to place it in the mold.

But in the mean time the cases had been placed one over the other, and the iron pins which connected them fitted to their places, taking care to divide them into two portions, which answered to the two cases usually employed by the founders; and which, instead of the usual thickness of five centimetres at most, were, the one 48 and the other 64 centimetres thick.

In this state they were dried, by placing them round a brasier of kindled charcoal, the fire of which had the more power from the mold being divided into two portions, and empty. The pieces of the mold dried.

The core was likewise dried by placing it over a brasier of charcoal; the same was also placed round it; and in eight hours the moisture was entirely evaporated. It was left to cool, and it was placed in one half of the mold; the second half of the mold was afterwards fitted on, and the whole compressed together by iron presses in the usual manner. The core also dried. The mold and core put together ready for casting, and secured by presses.

After this there only remained to construct the bason, *(l'echino)*, to fuse the metal, and make the cast. These operations being the same for both methods of founding, shall be related, after first as briefly as possible describing the method of casting by the great foundery in which wax is used.

The first operation for the great foundery is to dig a trench proportionate to the size of the figure to be cast, and to surround it with a wall to prevent the earth from tumbling in. After the model is finished it is oiled, and a mold formed from it with plaster of Paris in the usual way, and with the precautions before directed for molding in sand: In each piece of this mold rods of iron are inserted, by which they may be easily lifted when the mold is taken asunder or put up; each of these pieces is numbered, that its proper place may be known. Description of casting by the great foundery. A mold formed from the statue in separate pieces,

After this several layers are applied with a brush to the inside of the pieces, of a composition made of 7-tenths of yellow wax, 1-tenth of turpentine, 1-tenth of white pitch, and 1-tenth of hog's-lard, which is melted slowly to prevent its forming bubbles. A composition of wax laid on to the inside of the mold, of the thickness intended for the bronze.

When the different layers form a thickness of three or four millimetres, (0.15 inch) cakes of wax are placed inside in those parts where the bronze should be of a greater thickness, and fastenings of sheet-brass are inserted, which may take hold of the core and prevent the wax from falling off.

The

The core caſt in this mold, and the modelling of the wax finiſhed by the ſtatuary after it is taken out.

The mold is then fitted together, and the core caſt with quickneſs, that it may form an entire maſs, and not lie in layers: As ſoon as it is ſolid the model is taken aſunder, and the ſtatuary repairs the wax, takes off all the futures of the molds, rectifies the errors which may have occurred, and gives to the work all the perfection of which it is ſuſceptible.

The true mold formed over the wax; its compoſition.

After this the true mold is formed of materials capable of ſupporting the heat and the impulſe of the metal; to compoſe which three-ſixths of earth are mixed with one-ſixth of horſe-dung, and left to rot in a ditch for one winter: when this mixture is taken out, two-ſixths of broken crucibles, well pounded and paſſed through a ſieve, are added: the whole is tempered with urine and beat up on a ſtone: it is then what is called *potee*.

Formation of the true mold continued.

When it is to be uſed, a ſufficient quantity is taken and water enough added to it, to make it capable of being laid on with a bruſh; forty coats of it are then laid over the wax ſucceſſively, (care being taken that one coat is dry before another is laid on), which altogether form a thickneſs of five centimetres (2 inches.) The mold is then ſurrounded with flat bands of iron, which croſs each other like net-work; then, after rendering the *potee* thicker, by adding earth to it, and hair that has been well beaten, it is laid on over the former work with the fingers, until the mold has attained the thickneſs of twenty centimetres below and ſixteen above (6 and 8 inches); after which it is ſurrounded a ſecond time with bands of flat iron.

Preparations for, and method of melting out the wax,

After this a wall is built round it, the paſſages neceſſary for the fire conſtructed, and the intervening ſpaces are filled up with broken bricks: Then the fire is kindled in the paſſages moſt diſtant from the figure, and is gradually increaſed for nine days, and again diminiſhed for the ſame ſpace of time. On the ſecond day the wax begins to flow, and continues to do ſo for ten or twelve days; about half of it is loſt.

When the fire is extinguiſhed, the work is left ſome days to cool; then the broken bricks are removed, and before the mold is buried, a coat of plaſter, about half an inch thick, is put over it, which is called the *chemiſe*. Then they proceed to bury the mold, or incloſe it with earth, firſt taking care to ſtop all the ways through which the wax flowed, and to raiſe the

The mold buried, and the pipes and vents placed ready for caſting.

the pipes for the vents and for the entrance of the fused metal. The earth used for enclosing the mold should be first skreened, and then laid on equally in the excavation. After each course is raised to a thickness of thirty centimetres, (1 foot) it is beaten down till it is reduced to ten.

After this there only remains to build the bason for the reception of the metal, called the *echino*.

Enumeration of the various advantages of casting in sand, and disadvantages of the great foundery.

In enumerating the operations necessary for the method of casting in sand first mentioned, the authors of the report state them to amount to ten; while those used for casting in the large way last recited, in which wax is used, amount to no less than twenty-eight, each of which they particularize; but as these operations may readily be counted from the relation already given, this catalogue is not inserted here. They also remark, that the laying on the wax on the pieces of the mold takes up much time, as does likewise the preparation of the *potee*: that in the first method the circling with bands of iron is entirely avoided, and the building of the passages for the fire, which are very expensive: that likewise the molding and setting up of the vast number of pipes and vents is saved in it; and in the drying of the work the economising of fuel is greatly in favour of the first method, for in it these operations are performed in a short time with a very small fire, which in the other method require at least three weeks and a powerful heat: that in the repairing of the wax the statuary must work with his own hands: and that in taking out the statue when cast, there is vastly less trouble in the first method.

The authors here describe the method of erecting the furnace for fusing the metal for the statue of Joan of Arc; but as it was constructed to burn wood, which fuel is not used in our founderies, and as the description would be on other accounts of but little benefit to our artists, it is omitted.

It is only necessary to state, that the place which contained the fused metal was at such an elevation, that, when the stopper which retained it was driven in, it might flow freely into the *echino* through the passage prepared for it.

The mold of the statue of Joan of Arc laid in the earth ready for casting.

The mold for the statue was partly buried in the earth, so as to allow a fall for the metal of eight centimetres (3 in.) from the hearth to the entrance of the pipes; and the authors observe, that the trouble of burying the mold might be avoided by lay-

ing

ing it on its side, for which position they think it was sufficiently well prepared.

Parts of the statue cast separate from the rest.

The statue was all formed in one mold, except the skirts, *(pleinte)*, one arm, and the plumes of the helmet, which were placed in a separate case: This might have been dispensed with, but it was apprehended that, if they remained with the statue, they would have much encreased the difficulty of the work, by adding to the elevation of the figure.

1000 kilograms of metal fused.

Every thing being prepared for the casting, about 1000 kilograms (about 32 C.) of the metal was placed in the furnace, one half of which metal consisted of old cannon, a third of copper, and the rest of brass; and on the 8th of Germinal, *An XII.* (29th March, 1804), at eight o'clock in the afternoon, the metal, after five hours heating, being in compleat fusion, and the *echino* and the stoppers which closed the two passages for the metal being previously heated, M. Rousseau forced in the plug that retained the metal in the furnace; it flowed immediately into the *echino*; the stoppers were removed from the passages for the metal from thence, and in less than two minutes a little of the metal appeared at the vents, and shewed that the cast was completed.

The cast made by M. Rousseau: It succeeded compleatly.

The statue requires no repairs or additions.

On removing the sand it was found that no accident had happened but a slight flaw on the stomach of the figure; that the head was quite perfect; and that there had been no partial casting, or any part of the figure necessary to replace, which often happens in the other method.

The reporters recommend the *Athénée des Arts* to give medals to MM. Rousseau and Genon, and to make honourable mention of M. Gois.

The reporters conclude with high encomiums on the advantages of this method of casting, and recommend that medals be given to MM. Rousseau and Genon by the Athénée des Arts, in testimony of their merit; and that as the rules of the Society prevented this recompence from being granted to any of its members, honourable mention should be made of M. Gois.

Experiments

VII.

Experiments made at the Galvanic Society of Paris, by M. RIFFANT, *Director of the Nitre and Gunpowder Works, tending to prove that Muriatic Acid is not composed as announced by M. Pachiani* *.

As soon as the Galvanic Society knew that M. Pachiani, of Pisa, had announced, that he had obtained muriatic acid by depriving water of a portion of its oxigen, their first care was to engage in a course of experiments both by galvanism and electricity, to obtain, if it was possible, a confirmation of a discovery so important to the progress of science. The society had a letter communicated to them, which was addressed on the 9th of May, 1805, by M. Pachiani, to M. Pignotti; in which he recited the results which he had obtained, but without entering into any detail relative to the nature or order of his experiments; they only knew that he used the galvanic pile. They therefore determined to make their experiments with the same apparatus, in the manner which appeared to them the least likely to produce results liable to objections. Two of these experiments. which appeared principally worthy of attention, were conducted as follows: **The galvanic Society engage in experiments to determine the truth of Pachiani's position.**

Experiment I.

A portion of a new glass tube was taken, three inches long, and 0.35 inch in diameter inside, one of the ends of this tube was closed at the lamp; to the other end was united a capillary tube bent in such a manner as to pass under a jar, and at equal distances from the junction of the capillary tube, two points were drawn out at the lamp in the thickness of the glass, by means of which two bits of gold wire of about 0.02 inch in diameter, and of the standard 0.976 of purity, were inserted in the tube, at a small distance from its lower extremity, and disposed so as not to touch each other, or bear against the sides of the tube; these points of the glass were then closed at the lamp. The tube and its capillary prolongation were filled with distilled water, whose **Description of the apparatus used in Exp. 1.**

* *Journal de Physique*, Tom. LXI. p. 281.

purity

purity had been proved by nitrate of silver; the whole was fastened by wax on a small piece of glass, placed in the midst of an horizontal galvanic pile of fifty-two double square plates of 4.25 inches at each side. These plates were separated by pieces of leather, which formed between each other divisions, which were filled with very pure sand, moistened by a solution of muriate of soda. The capillary tube was passed beneath the water of a cistern, with its extremity under the mouth of a jar filled with water. The two wires of gold being made to communicate with the two poles of the pile, its activity was immediately exhibited by the disengagement of gas in chains of bubbles very apparent, parting from the extremity of each of the gold wires, but in a more considerable quantity from that corresponding with the copper pole. This activity of disengagement of gas continued without interruption from the eighth of Thermidor, to the 23d of the same month, on which day, after the pile being moistened with solution of muriate of soda, it stopped for some time: it soon however recommenced, and always did so after any suspension. Its activity was immediately renewed by agitating the wires which communicated with the poles of the pile. The activity of the pile was constantly greatest at four o'clock in the afternoon; and immediately afterwards it began to diminish. On the 11th of Fructidor the apparatus was taken asunder, after continuing for thirty-four successive days in continual action. The water was then diminished to half its original volume, but had not lost any of its limpidity.

The sand of the pile moistened with solution of muriate of soda: gas immediately disengaged on its completion,

the activity of the disengagement of gas renewed by agitating the wires. This activity always greatest at four in the afternoon. The apparatus separated after continuing in action thirty-four days.

The extremities of the gold wires, from whence the gas had proceeded, were oxided, but that most perceptibly which communicated with the zinc pole of the pile. The whole gas obtained and collected during the experiment, was 793 cubic centimetres (1200 inches). The liquor remaining in the tube was carefully examined; it produced no sensation of taste on the tongue, had no action whatsoever on the tincture of turnsol and of fernambuc, and did not produce the least cloudiness in the solution of nitrate of silver.

The water in the tube diminished one half, the gold wires oxided, that next the zinc most, 793 cubic centimeters of gas collected.

The gas produced by the action of the pile was then examined. After having introduced one measure of it into the eudiometer of Fontana, an equal measure of nitrous gas, made purposely for the proof, was added; an absorption of seventy-

The remaining liquor in the tube has no taste, has no action on turnsole, and is not affected by solution of nitrate of silver.

ſeventy-ſeven two hundredth parts took place in the volume of the two meaſures.

The gas produced is tried by Fontana's eudiometer, an abſorption of ſeventy-ſeven two-hundredths takes place.

In order to aſcertain whether all the oxigen which the gas contained had entered into combination in this abſorption, a ſecond meaſure of nitrous gas was introduced into the eudiometer; but the gas did not experience any diminution of its volume. The quantity of oxigen which the abſorption produced by the introduction of the firſt meaſure of nitrous gas might indicate, was attempted to be valued by a compariſon with atmoſpheric air eſſayed in the ſame manner; for which purpoſe one meaſure of atmoſpheric air was introduced, and an equal quantity of nitrous gas added; an abſorption of fifty-five two hundredth parts took place in the volume of the two meaſures. In conſidering this abſorption as the effect of the combination of nitrous gas with the volume of oxigen gas, correſponding to the 0,22 parts, which atmoſpheric air contains, it may be concluded, that the abſorption of ſeventy-ſeven two hundredth parts, produced with the gas of the pile, repreſented proportionally the combination of the ſame quantity of nitrous gas with a little leſs than 0,31 parts of the oxigen. It was then obſerved that the meaſures of gas having been introduced ſeparately and ſucceſſively into the eudiometer, it might happen that they were not ſufficiently well mixed, and that conſequently the abſorption was not as compleat as it might be. It was thought that it might be better to paſs the gaſes in ſeparate meaſures under a jar, and then to introduce the whole volume together into the eudiometer. The former experiments having been repeated in this manner, an abſorption took place between the gas of the pile and the nitrous gas, of ninety-two two hundredth parts in the volume of the two gaſes, in place of ſeventy-ſeven reſulting from the ſame proof, by the firſt method; and with the atmoſpheric air and the nitrous gas the abſorption was ſixty-eight two hundredth parts inſtead of fifty-five. There reſults then from this, according to the ſame ratio of the 0,22 parts of oxigen contained in the atmoſpheric air, a proportional indication of about 0,30 parts of this gas contained in that of the pile. It was again proved with the eudiometer of Volta, by introducing a ſingle meaſure into it, through which the electric ſpark was made to paſs; the proof was afterwards repeated ſucceſſively.

The quantity of oxigen contained in the gas valued at 0,31 parts.

The proportion of the oxigen after more exact trials is more accurately valued at 0,30 of the gas.

The gas farther proved by Volta's eudiometer gives the same result of 0,30 oxigen.

successively, on two, three, and four measures, and always the absorption resulting from the inflammation by the electric spark, gave the same indication of about 0,30 parts of oxigen.

Experiment II.

Apparatus used in second experiment described.

The sand is moistened with river water containing 1-sixtieth nitric acid.

The gold wire is dissolved and precipitated,

Two grammes (31 gr.) of distilled water were put into a glass tube bent into the form of a syphon; two wires of gold of commerce of about 0,008 inch in diameter, passing into the water at about 0,024 inch distance from each other, were inclosed in this tube; the tube was then placed upon an horizontal pile of fifty double plates, of about 3¼ inches in each side. The intervals between them were filled up with dry sand, and then moistened with river water acidulated with about $\frac{1}{60}$ of nitric acid. The wires of gold having been placed in communication with the two poles of the pile, the water in the tube assumed in the first day a reddish brown tint at the side of the copper pole, and the wire which passed to that part was covered with a coat of oxide of gold of a deep brown colour. The wire which communicated with the zinc pole did not assume the same tint; the gold of the wire was dissolved by degrees, and was precipitated together with a part of the silver. This precipitate exhibited with a magnifying lens, over almost the whole length of the tube, crystals in needles. The wire corresponding to the zinc pole was entirely deprived of the gold which covered it, and then only consisted of a thread of silver of extreme tenuity. But very little gas was discharged from either extremities of the wires. The water was not diminished a fiftieth part of its volume.

very little gas produced, the water in the tube is not diminished 1-fiftieth.

The pile continued in action forty days.

Indicates by the electrometer an intensity of 840°.

The remaining liquor in the tube shews no trace of acidity by any of the reagents, and has a metallic taste.

The pile continued in activity 40 days from the 28th of Messidor to the 8th of Fructidor. It indicated then on the last day by the electrometer (simplified by one of the Members of the Galvanic Society, from that constructed in Germany, described in the *Journal de Physique* for the month Messidor, an. 13,) an intensity of 840 degrees. The liquor remaining did not afford, by any of the different reagents, the least trace of oxidity; a metallic taste was alone perceptible in it.

The galvanic society, in examining chiefly the results of the first experiment, as corresponding more particularly with the fact announced by M. Pachiani, have considered, that in allowing

allowing for the small quantity of oxigen which had caused the oxidation of the extremities of the gold wires, the whole quantity of the oxigen contained by the gas of the pile may be valued in a very near approximation, at 0,31 of its volume; and it is very nearly in this same proportion that oxigen gas enters into the formation of water; it was thought that it might thence be concluded, that the whole effect of the galvanic pile, during the entire continuation of the experiment, had been the decomposition of a part of the water employed, and the separation, in their state of purity, of the oxigen and hidrogen gases which composed it. The Society then are of opinion, that M. Pachiani was deceived as to the nature of the acid which he announced that he obtained, and that this acid * might have been produced by some animal or vegetable substance employed in the apparatus. The Society does not hesitate to declare, that with the apparatus which they used in preference, (as being the most simple, and the least liable to the influence of other matters,) they do not think it possible to effect any thing by the action of the galvanic pile, but a decomposition more or less great, of the water used in the experiment.

The Society think the action of their pile only decomposed the water into oxigen and hydrogen gases, and that Pachiani is mistaken, and that the acid found in his experiment was produced by other means than those he announced, and that it is not possible to effect anythi by the galvanic pile in water but its decomposition.

VIII.

Account of an Ancient Geographical Tablet in the Museum of Cardinal Borgia, from † a Memoir presented to the Academy of Gottingen, by PROFESSOR HEEVEN.

IN the Museum of Cardinal Borgia there is deposited an ancient geographical tablet, from which an engraving has

The tablet was found in the museum of Cardinal Borgia.

* M. Giobert, in Van Mon's Journal, pretends, that the acids and salts used in the pile *circulate along the wires*, and pass into any liquor into which they are conducted; which does not appear probable.—B.

The galvanic apparatus used in these experiments is called a pile through the whole of the French paper, though from its horizontal position the appellation does not seem very proper. *Trans.*

† This memoir is entitled, "explicatio planiglobii orbis terrarum faciem exhibens, ante medium sec. xv. summa arte confecti; agitantur simul de historia mapparum geographicarum recte instruenda consilia.

 been

been made, one of the impressions of which is in the possession of the author of the Memoir. This remarkable monument is not a chart drawn by the pen, but a round tablet, of which the design occupies a space about two feet in diameter, on which the hemisphere, known at that time, is represented in coloured enamel, like a round surface. The countries and the places are marked by their proper names, but the limits of the countries are not traced; the mountains, the rivers, the people, and all the things remarkable (as the animals, the battles, the caravans, the bazars, the camps, the wandering tribes, &c.) are represented and explained on it by inscriptions in the latin tongue, but written in German characters. It may be conceived from this first view how interesting this monument is, and also with what art it is executed, so that it is impossible to suppose, that it was made for the use of a private person. Its date is not mentioned, but it may be determined with certainty that it was constructed in the first half of the fifteenth century. In reality, the most recent event marked on it is the victory of Tamerlane over Bajazet in 1402; there is no mention of the taking of Constantinople, or the least trace of any of the discoveries of the Portuguese. Of the geographical charts known at present, that of *Marino Sanudo* at the commencement of the fourteenth century is the only one certainly more ancient; but that of *Andre Bianco* of 1436, which *Formaleoni* has made known, is very nearly of the same time as this monument. No general source of information can be discovered by which the author of the tablet has been assisted. It is not made according to documents from Ptolemy; it more follows those of the Arabians, especially with regard to Africa; of the names which are found in the works of *Marco Paolo,* and the other more ancient travellers in Asia, only some are seen on that part of the world. The extent of Europe is represented as much greater than that of Africa, and at least as large as that of Asia. The following are some of the most remarkable particulars of it: Sweden is set down under the name of *Magna Gothia,* and Denmark is wanting. In Prussia, the seat of the wars of the Teutonic order with the Lithuanians, represented with this inscription: *Hic sunt confinia paganorum et christianorum, qui in Prusia adinvicem continue bellant.* It may be perceived by this, that the Lithuanians were therein considered

The design is two feet in diameter, and done in coloured enamel,

limits of the countries not marked,

accounts inserted in it of various remarkable things.

Why supposed to have been made in the first half of the fifteenth century.

It is the oldest geographical design, except the chart of Sanudo.

Sweden called Magna Gothia in it,

Denmark omitted, Lithuanians called Pagans,

sidered as Pagans, although christianity was introduced among them before this period. Russia appears under the denomination of Tartary, and near the Caspian Sea and the sea of Asof, are represented the famous Bazars of those times. England and Scotland appear at the border, but there is no more room for Ireland. Africa exhibits none of the discoveries of the Portuguese, but the northern half of it was known to the author as far as Soudan. He names not only the villages along the coast, but he moreover knows that the inhabitants of mount Atlas, the people of Barbary are at war with the Saracens. Near these mountains is inscribed, *In illis montanis habitant plures principes et reges, et habitant continuo in tentoriis, et præliantur continuo contra Saracenos, et contra juxta castra et civitates* *. In Egypt the junction of the grand caravan of Mecca is marked, and not only the names of the deserts of sand are inserted, but those also of the places most important to commerce, as Tagaza, Ganusia, &c.

Russia called Tartary in it. Places of the Basors represented.

It contains none of the discoveries of the Portuguese in Africa.

The junction of the Caravans of Mecca noted in Egypt.

The kingdom of Prester John extends in Nubia *ab ostio gandis* (Cape Gardefan) *usque ad fluvium auri*. Bianco likewise sets down the kingdom of Prester John in Africa in the same manner, so that the Portuguese are not the first who have thus described it. Asia does not present fewer singularities. In Asia Minor the camps of the Tartars are represented; *Tartaria reges maxima, qua Tartari cum suis jumentis et bobus excurrunt, civitatem ex multis tentoriis et carutes situant.* India is divided into *India superior*, where the body of St. Thomas is found, and many christian kingdoms, and *India interior, in qua Cathai civitas et magnificanis Imperatoris Tartarorum sedes.* China is likewise inserted in it, and its capital Cambalck (Cambalu Pekin) is also named. On the frontiers of little Bucharia at Organti, (Urgang)

Extent of Prester John's kingdom set down.

Camps of the Tartars in Asia Minor marked.

the divisions of India, and position of Cathai inserted,

and China with its capital Cambalk (Pekin.)

* The Latin of the inscriptions in this paper is not very correct, *continuo* is used in them for *semper*, and *juxta* for *vicina*, the word *situant* is also improper, and some others, but these circumstances perhaps only mark more strongly the authenticity of the account. In the inscription relative to India interior, a small alteration has been made from the memoir in this translation. The word *magnificanis* has been formed from *magni canis* in the memoir, which being so printed, evidently was an error.—B.

The route of the caravans to Cathai; the country of Gog and Magog; and the site of Paradise.

de Oeganis ad Carthagiam vadunt cameli in quatuor mensibus; the caravans going and returning to Cathai are represented. On the eastern border the country of *Gog* and *Magog* is set down, and finally *locus deliciarum* or paradise.

IX.

Analysis of Birdlime. By M. Buillon Lagrange.*

Sect. I.

The Origin and Preparation of Birdlime.

Various opinions on the nature of birdlime;

THE substance known by the name and appellation of *birdlime,* has been classed among the immediate productions of vegetables.

M. Fourcroy,

M. Fourcroy was the first person who considered this matter to be glutinous: he has described it as a species in his "*Systeme des Connoissances Chimiques,*" Vol. VIII. p. 306.

Birdlime, according to this chemist, may be made of the berries of mistletoe, or of the tender bark of the holly, and several other kinds of trees, macerated in water. Although this substance appears to have been hitherto not examined with sufficient accuracy, many qualities have been discovered in it analogous to those of gluten.

Excepting a few chemical properties, mentioned in my "*Manuel d'un Cours de Chimie,*" third edition, I have never found in any work the least elucidation of the nature of this singular substance.

M. Chaptal,

M. Chaptal, in his "*Elemens de Chimie,*" speaks only of its preparation. As the method prescribed by this chemist is nothing different from that in the "*Materia Medica*" of Geoffroy, and in the "*Dictionary*" of Valmont de Bomare, I shall quote the article itself.

Geoffroy, Valmont de Bomare.

Ancient mode of preparing it, by boiling and pounding the berries of mistletoe;

"The ancients made use of the berries of the mistletoe of oak in the preparation of birdlime. The berries being first boiled in water, were pounded, and the hot water was then poured off, in order to carry away the seeds and rhind. At present birdlime made of the bark of holly is preferred. The middle bark is made choice of, as being the most tender and

* Annales de Chimie, Vol. LVI.

green;

green: this is placed in a pit to rot, after which it is pounded in mortars until it becomes a paste, and is then washed and cleansed with water. This substance has been considered as discussive and emollient, when applied outwardly." *at present it is made from holly bark rotted and pounded.*

It is already known that the mistletoe of oak is employed in several pharmaceutical preparations; as, the universal water, the antispasmodic powder, Guttet's powder, &c. *Mistletoe of oak used in pharmacy.*

In England, according to Geoffroy, birdlime is made of the bark of holly. He says, the bark is boiled in water seven or eight hours, till it becomes soft and tender. This is laid in masses in the earth, and covered with stones, placing one layer over another—the water having been previously drained from the bark. In this state it is left to ferment and rot, during a fortnight or three weeks, in which time it changes to a kind of mucilage. It is then taken from the pit, pounded in mortars till reduced to a paste, washed in river water, and kneaded till freed from all extraneous matters. The paste is left in earthen vessels during four or five days, to ferment and purify itself. It is afterwards put into proper vessels, and thus becomes an article of commerce. *English mode of making birdlime: Bark of holly is boiled, rotted, pounded, washed, and kneaded.*

This mode of preparation is not universally followed, as every county has its peculiar way: there are even those who make a secret of the process.

At Nogent-le-Rotrou birdlime is manufactured by cutting in small pieces the second bark of the holly, fermenting them in a cool place during a fortnight, and then boiling them in water, which is afterwards evaporated. *Method pursued at Nogent-le-Rotrou; first rotting and then boiling:*

At Commerci and its environs birdlime is obtained from several shrubs, as the holly, the wild vine (*viburnum lantana*, Lin.) and the mistletoe of every species. *At Commerci.*

The best is that made from the prickly holly, which is greenish; that obtained from the *viburnum lantana* is of a yellowish tint. In using this latter, the epidermus is rejected, and only the second bark employed. *Best sort from the prickly holly.*

The birdlime which I used in my experiments was made from the second bark of holly; and on comparing it carefully with some which had been sent me from Commerci, I found there was no apparent difference between them. I thought this precaution essential to obtaining greater precision in my analysis. It is well known that the birdlime of commerce is never in a pure state; it is frequently a composition of vegetable

Birdlime of commerce frequently adulterated.

table and animal matter; sometimes it is even adulterated with turpentine, oil, vinegar, &c. It was therefore necessary that I should be certain as to its purity; and by the following mode, which I adopted, I obtained birdlime of the very best quality.

The author's preparation of birdlime.

Having procured a sufficient quantity of the second bark of holly, I bruised it well, and boiled it in water for four or five hours: the water being poured off, I deposited the bark in pits, in earthen pans, where it remained till rotten, or at least till it became viscous, moistening it from time to time with a little water. When it had obtained a proper degree of fermentation, it was cleansed, by washing, from all heterogeneous matters.

Sect. II.

Chemical and physical Characteristics of Birdlime.

Characteristics.

Birdlime is of a greenish colour, and of a sour flavour: it is gluey, stringy, and tenacious. Its smell resembles that of linseed oil.

It becomes dry and brittle by exposure to the air;

Spread on a glass plate, and exposed for some time to the action of air and light, it dries, and becomes brown in colour, being no longer viscous. When quite dry, it may be reduced to powder, in which state it is totally divested of its adhesive qualities, and only recovers them on the addition of water.

but viscid again when wetted.

Birdlime reddens tincture of turnsole.

By moderate heat it fuses.

When gently heated in a porcelain vessel it melts, but does not become very liquid; it swells in bubbles, which float upon the surface. This kind of fusion produces small black grains, which render the birdlime grumous: it produces a smell very similar to that obtained from animal oils, on raising their temperature.

If this fusion be continued for some time, the birdlime assumes a brownish colour; but recovers its proper characteristics on cooling.

Strong heat inflames it;

Placed on red-hot coals, it burns with a brisk flame, and creates a great deal of smoke.

Heated in a crucible of platina, it takes fire when the crucible is red-hot; produces a lively flame, which rises to about two decimetres, accompanied with a considerable quantity of smoke,

smoke, which easily attaches to the chimney: this combustion always takes place, although the crucible be taken out of the fire.

A whitish residue is left, which is very alkaline and partly soluble in water. Re-agents demonstrate the presence of sulphate and muriate of potash. leaving an alkaline residue.

That part which water could not take up, on being put into muriatic acid was dissolved with some effervescence. Experiments on the residue.

This liquid is copiously precipitated by the oxalate of ammonia; prussiate of potash gives a blue precipitate. That produced by ammonia is of a pasty consistence, partly soluble in caustic potash: whence may be inferred that the residue, independent of the salts soluble by water, is composed of the carbonates of lime and of alumine, with a small portion of iron. It contains soluble salt and carbonate of lime, and alumine and iron.

Water has very little influence on birdlime. On boiling, the matter does not completely dissolve, but acquires merely a small increase of fluidity, which it loses in cooling, and resumes its primitive consistency. Water has little action on birdlime:

This water obtains no colour; its flavour is at first insipid, afterwards sour, and it reddens tincture of turnsole.

Evaporated to the consistency of a syrup, it becomes coloured, with a mucilaginous appearance, which may be separated by alcohol.

The action of water, therefore, is confined to the solution of a mucilaginous substance, with a small portion of the extractive matter. It takes up some mucilage.

It is not thus with caustic potash. Its concentrated solution forms at once with birdlime a whitish magma, which turns brown on evaporation, with a separation of ammonia. Caustic potash combines with birdlime and gives out ammonia.

This composition is less viscid; it acquires a great degree of hardness by exposure to the air; and its smell and taste are similar to those of soap.

It is chiefly soluble in water and alcohol, there remaining but a few vegetable dregs. These solutions are affected by strong acids; but these kinds of decompositions present no new phenomena to those obtained with a solution of soap. The compound has the habitudes of soap.

The most feeble acids soften birdlime, and partly dissolve it: when concentrated, they act in a different manner. Weak acids soften and partly dissolve birdlime.

Sulphuric acid renders it black and charry: by adding powdered lime, so as to form a thick magma, a separation of acetic Sulphuric acid charrs it.

tic acid and ammonia is procured. There can be no doubt but that in addition to the acetic acid naturally present in birdlime, more is produced by the action of the sulphuric acid.

Hot nitric acid decomposes it, and forms a kind of wax;

Nitric acid, whilst cold, has very little power over birdlime: but on increasing its temperature, the mixture turns yellow, dissolves, and as evaporation advances swells considerably, leaving at last a hard brittle mass. This mass, when a second time submitted to the action of nitric acid, is dissolved, a part being converted into malic and oxalic acids. By continuing the evaporation a yellow matter is obtained, easily friable, yielding to the pressure of the fingers like wax, with a kind of elasticity, and melting by means of a gentle heat.

which forms soap with potash.

Potash combines with this matter, changes its colour from yellow to brown, and forms perfect soap.

Alcohol partially dissolves it.

Alcohol partially dissolves it, and becomes yellowish; its transparency is diminished by the addition of water.

On evaporating the alcohol to dryness, there remains a yellow matter divested of the greasy appearance, which yields a sweet odour in burning.

Cold muriatic acid has little action on birdlime.

Cold muriatic acid has no action upon birdlime: when heated it turns it black.

Oxigenated m. acid alters birdlime considerably.

Oxigenated muriatic acid operates differently. Either by mixing the gas with the water containing the liquid birdlime, or by shaking it in a bottle with the acid in a very concentrated state, the following phenomena were equally observed:

The birdlime quickly lost its colour, and became white; it was no longer viscid, but divided into hard compact portions, containing in their centre a quantity of birdlime, which the oxigenating principle had not reached. This non-oxigenation may be attributed to the difficulty there is of preserving this substance in its liquefied state in hot water, whereby the operation of the acid is confined to its exterior surface.

Characteristics of oxigenated birdlime.

The characteristics of oxigenated birdlime are—

1. It is capable of being reduced to powder.

2. It is insoluble in water, even when heated.

3. It does not become liquefied at a high temperature.

4. It will not turn yellow, nor will it form a resin with nitric acid.

Acetous

Acetous acid softens birdlime and dissolves a certain quantity; the liquor acquires a yellow colour; its taste is insipid.

Acetous acid softens birdlime, &c.

Carbonate of potash produces no precipitate; evaporation gives a resinous residuum, which cannot, however, be brought to a state of perfect dryness.

Certain metallic oxides are easily reduced on being heated with birdlime.

Birdlime reduces metallic oxides.

Semi-vitreous oxide of lead assumes a grey colour, dissolves, and forms with the birdlime an emplastic mass.

Semi vitreous oxide of lead incorporates with birdlime.

Alcohol at 40 degrees, and boiling, dissolves birdlime so long as it is kept hot; it is clear, and of a transparent yellow colour; but in proportion as the liquor cools, it becomes turbid.

Boiling alcohol dissolves birdlime,

A yellow matter may be separated by filtering, which is much softer than the original mass; melts in a moderate heat, diffusing a smell analogous to that of wax, of which it seems to possess all the qualities.

and lets fall a kind of wax by cooling;

The filtered liquor is bitter, nauseous, and acid; precipitating in water, and leaving on evaporation a substance similar to resin.

retaining a resin.

Sulphuric ether may be considered as the true solvent of birdlime: its action on this substance is rapid, first dividing it, and then dissolving it nearly *in toto*, there remaining only a few vegetable dregs. The liquor acquires a greenish yellow colour, and strongly reddens turnsole. On adding a little water the mixture thickens, and the ether swims on the top; but if a sufficient quantity of water be poured in to dissolve the ether, a body of oil is formed on the surface, bearing a considerable analogy to that of lintseed: this may be converted, by the assistance of the semi-vitreous oxide of lead, into an emplastic mass.

Sulphuric ether the true solvent of birdlime.

Water separates oil from the solution.

By evaporating the solution of birdlime in ether, a greasy substance is obtained, of a yellow colour, and of the softness of wax.

Ether by evaporation gives a greasy substance.

Conclusion.

From the foregoing observations it will be perceived how little analogy exists between birdlime and gluten.

A simple comparison will be sufficient to designate the place it ought to occupy among vegetable productions.

General properties of birdlime.

Birdlime

Birdlime is vifcid, elaftic, dries a little in the air, by expofure to which it becomes brown; but is not rendered brittle and irrecoverable like gluten.

It melts in the fire, fwells, and burns with a vivid blaze; but does not diffufe that animal odour which is to be obferved in gluten.

Water will not diffolve birdlime; it merely imbibes the mucilage, the extractive matter, and the acetic acid.

Alcalies diffolve it; when concentrated, they convert it into foap.

Dilute acids foften and partially diffolve birdlime.

Concentrated fulphuric acid renders it black and charry.

Nitric acid turns its colour to yellow, converting the fubftance partly to malic and oxalic acids, and partly to refin and wax.

Oxigenated muriatic acid renders it white and folid, conftituting *oxigenated birdlime.*

Alcohol exerts but little action upon birdlime; it diffolves the refin and deftroys the acid.

Laftly, fulphuric ether diffolves it entirely.

Recapitulation of the points wherein it differs from gluten.

Birdlime, therefore, differs from gluten,

1ft. In the acetous acid which exifts in it.

2d. In being very flightly animalized.

3d. In the mucilage and extractive matter which may be obtained from it.

4th. In the great quantity of refin which may be obtained from it by means of nitric acid.

5th. In its folubility in ether.

X.

Method of purifying Oil. By M. CURAUDEAU.*

The purification of oil confifts in its clarification.

THE purification of oils for combuftion confifts folely in their clarification: It is only fince Argand's lamps have come into common ufe that this fubject has received much attention.

There are many proceffes for the purification of oils, but all are not equally good; and thofe who fell purified oil make a fecret of the method of purification.

* From Cours complet d'Agriculture, Tome XII.

However,

However, as the art of purifying oils ought to be known by those who manufacture them, the processes, which are considered the most economical and simple, shall be here mentioned; by which information they will be able to obtain that profit which those now make who follow this species of industry after them.

Process by sulphuric acid described.

The process for the purification of oil by sulphuric acid, which follows, is little different from that published by Thenard.

Sulphuric acid and water added to the oil; the mixture strongly agitated, and left to settle.

To one hundred parts of rape oil one part of sulphuric acid is to be added, diluted with six times its weight of water; the mixture should be strongly agitated, and as soon as this is completely finished, it is left still till the oil becomes clear; when it is perfectly clear the purification is effected.

The oil separated from the sediment; powdered chalk added; agitated again, and left to settle.

There remains at the bottom of the vessel an acid liquor somewhat coloured: the oil is to be separated from the sediment; and in order to be certain that no acid is retained by the oil, some ounces of powdered chalk is to be added; the mixture should then be shaken, and the oil again left quiet to settle.

The acid in this process separates the mucoso-extractive matter from the oil, which injures its combustion.

The action of the sulphuric acid in this process consists in depriving the oil of all its humidity, although it is itself mixed with water, and in separating from it a mucoso-extractive substance, the presence of which diminishes the energy of the combustion of the oil, covers the wick with charcoal, and produces much smoke: It is then on the abstraction of these principles foreign to the oil, that its quality of giving a good light depends.

Another Method.

Process by flour and water.

The next process to be described has been followed by some manufacturers, who have had good success with it.

Flour and water added to the oil; the whole well agitated, and heated till a separation of the contained matters ensues; in 24 hours after this it is clear.

To one hundred parts of rape-seed oil ten parts of water are to be put, to which one part of wheaten flour has been added; the mixture is to be well agitated, and then to be heated until all the water added has been evaporated, or, more properly, until the oil has ceased to have any union with the substances which it held in suspension: In this state it becomes purified; and at the end of twenty-four hours it is very clear, and does not differ at all in quality from that prepared by sulphuric acid.

In

The heat should be applied gradually, and should not exceed 80° Reaumur.

In the practice of this last process, care should be taken to heat the oil gradually, and not to raise its temperature above 80 degrees of Reaumurs thermometer. (212 Fahrenheit) This heat is sufficient to effect the coction of the flour, and of the mucoso-extractive matter contained by the oil; a greater degree of heat would colour the oil, and deprive it of the appearance most favourable to its sale.

M. Curaudeau led to this process by observing the separation of white sauce into two substances when too much done.

M. Curaudeau was led to this process by an observation which every one may likewise make. It is well known that the sauce called melted butter, when too much boiled is separated into two parts, one which is thick and occupies the bottom of the vessel, while the other part is clear and floats above the first: The lower substance is the caseous part of the butter united to the flour that has been added to the sauce, and which the action of the fire has separated from the oil; The upper substance is the butter deprived of all foreign matter; and in this state it may be called *purified butter*.

XI.

On a peculiar Fluctuation of the River Dordogne, called the Mascaret. By M. Lagrave Sorbie *.

The mascaret takes place only when the waters are low.

THE peculiar movement of the waters of the River Dordogne, which is called the *Mascaret*, takes place twice each day in the summer time, *when the waters are low*, which is an essential condition. A similar motion also takes place on the river Amazons according to the report of M. de la Condamine, when it is named the Pororoca; the same is also perceived at the Orcades, off the north of Scotland, according this author: and M. Sorbie has seen accounts in the publications of some voyagers of its likewise occuring in some of the rivers of Hudson's bay, and also in the Mississipi.

The Pororoca of the Amazons similar to it, a like occurrence at the Orcades, and in the rivers of Hudsons bay, and in the Mississipi.

It is not surprising that this phenomenon does not happen in all rivers; it is not always seen even in the Dordogne. From the most exact observation, if the summer is not dry, and that the waters are not low to a certain degree the Mascaret does not appear, It rarely occurs in winter; it however

* Journal de Physique, LXI. 206.

sometimes

sometimes takes place during very hard frosts, when the cold has diminished the waters by the formation of much ice; but this happens very seldom, and has never been more than three times, in several ages.

There is a *maximum* of depression in the waters necessary to its appearance; Wherefore the mariners in the neighbourhood of Bourdeaux are in the habit of talking of it somewhat in this manner, "The waters have fallen so much, the tide will encrease to day to such a height, we shall have a Mascaret", and they load their barks accordingly, and take precautions to avoid it. The manœuvres of these mariners have caused some naturalists in the vicinity of Bourdeaux to observe long since, that this phenomenon must depend on a natural cause, connected with the bed of the river, since these men can foretel, without being scarcely ever mistaken, by the depression of the water, whether the Mascaret shall appear or not, although sometimes it has not appeared before for some years, because the rains have prevented the waters from diminishing to the necessary degree.

The mariners foretel the Mascaret from the lowness of the river,

No one has yet tried to explain the cause of this singular fact, not even M. Condamine, or if there be any works on the subject, they are unknown to the author though his studies have been particularly directed in the line where such information might occur, and he has read much. In order to enable others to account for the facts, he mentions those which relate to it such as he has himself seen, and such as he has been told have been witnessed for several ages.

no account of the cause of the Mascaret yet published.

In the summer, or, more properly speaking, when the waters are low, there appears at a little distance from the junction of the Dordogne with the Garonne, or at Bec d'Ambes, an accumulation of water, like a promontory, on the shore, which is from the thickness of a ton to that of a small house, and which rolls along with such velocity that no horse, whatever might be his speed, could keep up with it. It follows the direction of the shore, and makes a most frightful noise. The horses and oxen, which feed in the meadows near the river, run away with their utmost speed exhibiting the greatest terror; so much so that they remain trembling a long time after, and cannot be driven back but with much difficulty. The ducks and geese have also been seen to precipitate themselves into the reeds at its approach, with the greatest speed and affright

It consists of an accumulation of water, which appears first at Bec d'Ambes, and rushes up the river with great velocity, and a frightful noise, terrifies the cattle, and the water fowl,

fright, and lie flat there, without being able to come out. Hard bodies, which lie in the way of the Mascaret are struck by it with such force, that the piers, built for the use of the vessels, along the shore are demolished, and some of the stones which compose them, although very large, are driven away more than fifty paces; the strongest trees are torn up by the roots, the barks which it meets are not only sunk, but broken asunder, especially if they are near the shore, or have any hard body lying beneath them. From the place called St. Andre (See the lower part of Plate IV.) on the river, the Mascaret forms itself into waves which half its breadth as far up as Caverne; there it disappears for a short time, to appear again between Asque and Lile like a promontory, and then returns into the form of waves as far on as Tersac; at Tersac it regains its first appearance, which it only quits at Vayne; from Vayne it proceeds along the bank as far as Fronsac, the house of M. de Richelieu; from Fronsac it occupies the whole breadth of the river, passes with a terrifying noise before the village of Libourne, throws the road for vessels belonging to this village into confusion, and afterwards appears at Genilac-les Reaux and at Peyrefite with but very little force. The whole passes in the course of seven or eight leagues.

overturns the piers along the river, and drives the large stones which compose them, more than fifty paces off, tears up large trees by the roots, and sinks and breaks vessels in pieces. Above St. Andre it appears in waves, above Asque is seen in its original form, in waves again above Lile, at Tersac it regains its first appearance; at Fronsac it occupies the whole breadth of the river, passes before Libourne with a terrible noise, and ceases at Peyrefite.

The following is the account, which M. la Condamine gives of the *Pororoca* of the river Amazons, the comparison of the effects of which with those of the Mascaret will tend to establish the theory of these phenomena.

Account of the Pororoca on the Amazons:

In his voyage to the river Amazons, page 193, he relates, that " between Macapa and Cape-Port, where the channel of the river is most confined by the islands, and especially opposite the mouth of the Arawary, which joins the Amazons on the north side, the flowing of the sea exhibits a singular phenomenon. During the three days next the full of the new moons, the times of the highest tides, the sea, instead of taking almost six hours to rise arrives at its greatest heighth in one or two minutes; it may be conceived that this does not happen quietly; there is heard at a league distance a terrible noise, which announces the *Pororoca*, which is the name that the Indians of these parts give to this frightful flood. In proportion as it approaches the noise encreases, and soon an accumulation of water, like a promontory, appears from 12 to 15 feet high; after that another is seen, then a third, and sometimes a fourth, which follow

its noise heard at a league distance. It advances in several waves, each twelve or fifteen feet high,

each other closely, and which occupy the whole breadth of the channel. These waves advance with a prodigious rapidity, break and overturn every thing which opposes them. I have seen in some places a large extent of land carried away, great trees torn up by the roots, and ravages of all kinds committed; every where that they pass the banks are swept clean; the canoes, the pirogues, and even the barks can only escape their fury, by anchoring in deep water. After having examined this phenomenon with attention in different places, I have always remarked that it only takes place, when the rising flood is engaged in a narrow channel, or meets in its way with a bank of sand, or a shallow place, which occasions an obstacle to it; that it was in those places alone that this impetuous and irregular movement of the waters commenced, and that it ceased a little beyond the bank, when the channel became deeper, or grew considerably wider. It is said that something similar to this happens at the isles of the Orcades, at the north of Scotland, and at the entrance of the Garonne, (it should be the Dordogne), in the vicinity of Bourdeaux, where the effects of these tides, is called a *Mascaret*."

which rush forward with great rapidity, and overturn every thing which opposes them, carry away large portions of the land, and tear up trees by the roots.

It only occurs in narrow channels, over sand banks, or shallow places.

It ceased where the channel became deeper or wider.

It appears from what has been cited from M. Condamine, that the effects of the Pororoca are almost the same as those of the Mascaret. Nevertheless there is a marked difference between them in this respect, that on the Dordogne, two kinds of floods take place, one which extends over the whole river, and is similar to that which M. Condamine has observed, and the other which ranges along the shore, rolling more over the deposits which the waters have left, than in the water itself. He says positively in page 194, that "at one or two leagues a frightful noise is heard, which announces the Pororoca; as it approaches the noise encreases; and soon an accumulation of water appears from 12 to 15 feet high, and then another that follows, which occupies the whole breadth of the channel". On the Dordogne the Mascaret rises with great noise, sometimes along the coast in an elongated accumulation, and sometimes in the form of frightful waves, which extend over the whole river; when it follows the shore it only appears in the re-entring angles, and on the sand banks, as is described in the sketch of the plan of the river, which accompanies this account, and which takes in the whole extent where the effects of the Mascaret are perceived. The parts covered with small

Difference between the Mascaret and Pororoca.

further particulars of the Mascaret.

Description of the sketch of the course of the

Mascaret in the river.

small points, indicate the sand banks where the Mascaret always commences; the parts occupied by small lines, are the places where the waves occupy the whole breadth of the river. The dotted parts indicate the re-entring angles, where the sand banks are found which are deposited by the counter current. It is here principally that the Mascaret rolls with all its fury over the mud of the river. On the banks the salient angles are the places where the Mascaret quits the shore, occupies the whole river, and runs upwards, accompanied by many considerable waves, which succeed each other, till another re-entring angle occurs, where it again resumes its first form.

It is thus that these who dwell in the vicinity of Bourdeaux witness without emotion twice each day, when the waters are low, so extraordinary a phenomenon, without any one thinking of examining into the cause of it, or even of communicating the particulars to naturalists.

The tide is the primary cause of the Mascaret.

The primary cause of this rising of the water is the same as that of the tide in all rivers; and if the Mascaret occurs on very few rivers, it is because their beds are not formed in a manner necessary to produce it, and have not the same disposition as the Gironde and Dordogne: they have either too little or too great a current; their waters are not sufficiently low, or when they are, the tide does not continue long enough; finally the re-entring and salient angles are not such as they ought to be. M. Sorbie thinks he could tell before hand whether any river would be liable to such effects, from the form of its plan and the disposition of its bottom; and is of opinion that the cause why more rivers are not subject to the Mascaret, depends entirely on the shape of their beds, and not on any particularity in their tides. The physical cause of that on the Dordogne appears very simple. M. de la Condamine says, that on the Amazons it is always at the narrow parts where it is observed. The cause is not the same on the Dordogne, for there is no narrow parts in almost its whole course: it is nearly every where very rapid, and of small depth, as all those rivers are which have much current. It forms, as may be seen in the plan, many turns and windings, and has few isles: but at each angle a bank of sand is deposited: It descends, notwithstanding these windings, almost from the east to the north-west. As far as Bec d'Ambes, where it unites with the Garonne which is much more powerful than it, and they form together that beautiful arm of the sea, called

The course of the Dordogne described, to account for the Mascaret,

the

the Gironde. The two rivers then descend together from Bec d'Ambes to the sea in a direction from the east to the north-west. All the waters which arrive from the arm of the sea or from the river, advance in a straight line with abundance into the mouth of the Dordogne, instead of mounting up the Garonne, which runs almost north and south as far up as Bourdeaux. The greatest part of the waters which are advancing to the Garonne, ought then, when the current has taken its course, to run up the Dordogne at the beginning of the flood, since its velocity does not allow them time to turn up the Garonne; and thus the water which ought to go to the Garonne, running up the Dordogne, form by their abundance, this effect which Condamine recites: He says that "the tides, which usually take six hours to rise, arrive at their full height in one or two minutes". But on the Dordogne, the tides never come to their highest level in near so short a time, even when the waters are lowest; but in one or two minutes they encrease considerably; which encrease is probably caused by the waves which arrive almost instantly; and the flood raising their masses of water above their natural level, leaves them there to augment the water in the bed of the river in proportion to their bulk. After the Mascaret has passed, the waters of both rivers encrease in the same gradual manner as those of all other rivers.

supposed to be caused by the waters on their way to the Garonne, taking the straighter course up the Dordogne.

M. Sorbie likewise thinks, after all, that the tide of the Gironde may be the cause of the Mascaret on the Dordogne, for it pours its waters into the mouth of the Dordogne in almost a right line; this arm of the sea being at least six times larger and deeper than the Dordogne, ought at the flood to carry up such an abundance of water, as could not enter into the bed of this river without occasioning the accumulation of waters described. The physical cause then of the Mascaret is the considerable mass of water which arrives from the Gironde into the mouth of the Dordogne, and the small depth of this river; since it is known that in rainy seasons, and when the river is a little encreased in size, this circumstance never takes place.

It may also be caused by the tide of the Gironde rushing in a right line into the mouth of the Dordogne, and by the shallowness of this river,

M. Sorbie remarks in conclusion that the facts related shew evidently that the flowing and ebbing of the tides of rivers are different from those of the sea; that the ebbing and flowing of rivers, are only secondary effects of the tides of the sea; that is to say, that the waters of the sea only form a dam to those

remarks on the tides of rivers, supposed to be caused by the tide of the sea forming a dam across their course.

those of the rivers, and that the rivers form by the abundance of their waters, those rapid flood-tides which are observed on the great rivers, such as those of the river Amazons, which ascend from 5 to 100 leagues, those of the Senegal, which advance almost as far up, and those of other rivers almost equally considerable. M. Sorbie thinks that the Mascaret, or the Pororoca, have altogether the same cause as the flood-tide of rivers, and though some slight secondary effects occur, such as those related, that all arise from the same physical cause.

XII.

Description of a secret Lock of ten thousand Combinations. W. N.

Disquisition upon locks. The common lock.

THE common lock usually consists of a bolt, which requires a particular instrument, called the key, to push it backward and forward; and in order that this bolt may be inaccessible to violation, certain impediments or obstacles, usually called wards, are interposed between the key-hole and the bolt, which make it difficult to open the lock by any general or common process.

Bolt, key, wards, pick-locks, skeleton-keys.

The general process for picking a lock, of which the key has not been seen, consists in operating upon the bolt by a small bended instrument or wire; or else by endeavouring to discover the position of the wards by an unperforated key, on the face of which some soft or plastic matter is lodged. And when this situation is once discovered, it is not difficult to file away so much of the key as shall allow it to pass, or else to select, out of a number of skeleton keys, one, of which the form shall admit of its passing through the lock. There are many locks so situated, as for example in the vestries of churches and other little frequented places, as to admit of this slow operation; but it must at the same time be allowed, that the English market presents locks of a number of different constructions, which can neither be picked nor analyzed by the process here mentioned. Nothing is more common, however, than for keys to be entrusted out of the hands of the possessor, or to be hung up, or casually laid down or mislaid. In these circumstances their figure may be taken with

with wax, like the impreſſion of a ſeal, or more ſpeedily by indentation upon a piece of moiſtened paper, or by various other means; and it muſt be admitted, that very little ſkill is required to enlarge the openings of a common key, ſo as to make it paſs the wards of a ſuperior lock.

Secret locks; their ſtructure and imperfection.

Theſe neceſſary and unavoidable imperfections of common locks, have long ago led to the introduction of ſecret locks, which are ſo conſtructed as to require ſome particular manipulation in opening them; ſuch as that the key ſhould be turned twice round, or that it ſhould be turned through a certain ſpace in one direction, and then back again; or that it ſhould act upon ſome delicately reſiſting piece, very likely to be diſregarded by an uninſtructed poſſeſſor of the key; or that a number of viſible parts ſhould be placed in ſome determined order, before the common proceſs of opening, either with or without a key, can take place. Upon all theſe contrivances one general remark may be made, namely, that the poſſeſſor muſt always in perſon open his own lock; for if this be to be done by the mere practice of a ſecret without a key, his cabinet becomes for ever open to him who, by communication or otherwiſe, ſhall poſſeſs that ſecret; and if a key be uſed, his lock, as to that perſon, becomes as ſubject to violation as a common lock.

Methods of violating them.

In the mechanical conſideration of a ſecret lock, we may ſuppoſe the conſtruction to be entirely unknown to him who is deſirous of opening it. In this, according to the experience and ſagacity of the operator, the difficulties will be greater or leſs, and a very ſhallow contrivance may occaſionally preſent a greater obſtacle than a much more elaborate ſtructure. But if we ſuppoſe the ſyſtem of the lock to be known, but the particular conditions of opening it to be ſecret, the examiner will then take for his guide the probable circumſtance that the re-action of the parts may feel conſiderably different, when they are duly placed for opening, than when their ſituation is ſuch as to prevent that effect. By this clue, and by careful examination, moſt of theſe locks may be opened; and it is remarkable, that the better the workmanſhip the more eaſy it is in general to make the intended diſcovery.

Conditions of a perfect lock enumerated.

The following are the conditions which appear to me to be neceſſary in a lock of the moſt perfect kind:

1. That

1. That certain parts of the lock ſhould be variable in poſition through a great number of combinations, one only of which ſhall allow the lock to be opened or ſhut.

2. That this laſt mentioned combination ſhould be variable at the pleaſure of the poſſeſſor.

3. That it ſhall not be poſſible, after the lock is cloſed and the combination diſturbed, for any one, not even the maker of the lock, to diſcover by any examination what may be the proper ſituations of the parts required to open the lock.

4. That trials of this nature ſhall not be capable of injuring the work.

5. That it ſhall require no key;

6. And be as eaſily opened in the dark as in the light.

Theſe conditions are in ſome reſpects liable to the inconveniences already mentioned. I would therefore add the following conditions:

7. That the opening and ſhutting ſhould be done by a proceſs as ſimple as that of a common lock.

8. That it ſhould open without a key, or with one, at pleaſure.

9. That the key-hole be concealed, defended, or inacceſſible.

10. That the key may be uſed by a ſtranger without his knowing or being able to diſcover the adopted combination.

11. That the key be capable of adjuſtment to all the variations of the lock, and yet be ſimple.

12. That the lock ſhould not be liable to be taken off and examined, whether the receptacle be open or ſhut, except by one who knows the adopted combination.

Deſcription of a new lock of combination.

In meditating upon this mechanical problem, I have thought of various conſtructions, but have not yet matured one in which all the above conditions are complied with. The lock delineated in *Plate III.* poſſeſſes the firſt ſix requiſites. *Fig.* 1. repreſents the plate of the lock, of which the other ſide is ſeen at *Fig.* 4. In this laſt figure the middle piece is a handle or knob, repreſented *Fig.* 6, which, when turned, ſerves to ſhoot the double bolt *i k*, *Fig.* 1, by any common connection. In the actual lock this bolt is carried backward and forward by a pin ſtanding out of *Fig.* 2, ſoon to be deſcribed. The other four circles in *Fig.* 4, are handles, repreſented in *Fig.* 5, which ſerve

ferve to move the four wheels feen in *Fig.* 1. Thefe wheels have twelve teeth each, and are faftened by center-fcrews, each upon a flat wheel of the fame tooth; but having only ten notches actually cut, as is feen in the right hand upper corner, where one of the upper wheels is taken off, and is fhewn at *Fig.* 3. Thefe upper wheels have their toothed part confiderably higher than the interior or flat part; fo that they would be contrate wheels if the teeth were cut quite through. But this is not the cafe, except with two of the notches, as may be feen in the two lower wheels more particularly, and alfo in the others. The upper wheels have alfo two of the notches between the teeth ftopped up, as is fhewn in *Fig.* 3; by which contrivance there are but ten fituations for fcrewing each wheel upon its correfpondent under wheel; and thefe fituations are rendered precife, and all relative motion between the two correfpondent wheels prevented by a fmall ftud feen in the uncovered wheel, *Fig.* 1, which fits into one of the notches of the upper wheel when put in its place. The upper wheel has a number on each tooth from 1 to 9 and 0, which are of ufe for placing this ftud. The four under wheels are held in their fituations by four fpring-catches, which allow them to be turned, in one direction only, by means of their knobs or handles; and when any wheel is thus turned round, the finger and thumb will feel the ftroke of the lever, as it fucceffively falls into each notch, until the lever comes to reft upon the fmooth part. This very palpable indication then fhews when to begin to count, calling the firft hold or ftroke of the catch 1; the fecond 2; the third 3, &c.; and the lock is fo conftructed, that when the top wheel of any of the four couple is put on with any number oppofite the ftud, the fame number counted by the catch will place the upper wheel in fuch a fituation, as that its notches, which pafs clear through, will lie in a circle defcribed from the center or axis upon which the great handle turns. And therefore, when each of thefe wheels is put in its place, and the numbers known (and regiftered, or put in the memory by fome artificial affociation, fuch as of the date of the year taken either backwards or forwards, &c.) it is only needful to move each of the four knobs till its catch has paffed the fmooth part, with a number of ftrokes anfwering to its adjuftment, and the circle indicated by broken fhaded lines in *Fig.* 1, will be capable of paffing through

Defcription of a new lock of combination.

Description of a new lock of combination.

through the open spaces of every one of the wheels. *Fig.* 2. represents a contrate wheel, having its irregular portions A, B, C, D, &c. standing up above its plane. These portions are parts of a circle equal to that denoted by the broken shaded parts in *Fig.* 1. The contrate wheel is to be placed in *Fig.* 1. with its face turned down; and being there screwed with its center to the central handle, it serves to open and shut the bolt, which it can only do when the four wheels are in such a situation as to allow the circular edge-parts of *Fig.* 2. to pass clear through their notches. If any one or more of those wheels be turned so as not to correspond with its number, it will be impossible to turn the handle, because every attempt to do so will cause one of the parts of *Fig.* 2. to stop in one of the notches of the wheels through which it cannot pass. The method of opening the lock will therefore consist in setting each wheel to its known number.

As the proper situation of each wheel is only one out of ten, it is nine to one against any operator upon this lock, that he shall not set the first wheel right, supposing all the others in their due positions; but it is true that he may try all round, and will come to the right place at last. If two only of the wheels were deranged, it would be eighty-one to one that he should not set them both right; and he would be deprived of any trial round a single wheel, because the other wheel would always hold against him, and prevent his knowing when the open notch of the wheel under trial presented itself. Three wheels deranged would make the odds 729 to one, and the four would make the odds 6561. In the plate the combinations are said to be ten thousand, from an oversight in taking the ratio of ten to one instead of nine to one. But this is a matter of no consequence as to the principles of the lock, because the number of teeth or number of wheels are capable of variation. If a fifth wheel were added to this lock, the odds would amount to 59049.

As the quantity cut from *Fig.* 2. is not more than was necessary for the clear rotation of the wheels when the lock is shut, this piece, when in every other position, prevents the other wheels from being turned at all.

Letter

A Secret Lock of ten thousand Combinations

XIII.

Letter from Mr. ALEX. CROMBIE, *concerning the Caledonian Literary Society at Aberdeen.*

To Mr. NICHOLSON.

SIR,

THE want of focieties for fcientific and literary improvement, has been long felt in many confiderable towns in Scotland, and I believe in none more than in Aberdeen.

The utility of fuch inftitutions being fo generally acknowledged, it is truly a matter of furprize to find fo few of them in this kingdom, efpecially when the facility of forming them is confidered. Any attempt, however fmall, to promote the interefts of literature, and to diffufe moral, political, or philofophical knowledge among men of all ranks, will ever meet with the marked approbation of the fincere wellwifher to his country; and I am perfuaded you will receive peculiar fatisfaction in being able to communicate to the public the feebleft efforts which may be at any time directed to fo important and defirable an object. Great utility of focieties for fcientific and literary improvement.

In your Journal for December laft, a *traveller* has expreffed his furprize to find no antiquarian or literary fociety, or fubfcription library, at Aberdeen; and I agree with his remark, that thofe who know the refpectability of the place, cannot fail to be aftonifhed at it. To account for fo fingular a fact would perhaps be deemed prefumptuous. I have too much refpect for my fellow-citizens to attribute it to a want of tafte, but I cannot help blaming thofe amongft us who are qualified for fupporting fuch inftitutions, for their want of attention in this refpect. Reference to a letter in a former Journal.

The Profeffors of both Univerfities certainly unite talents with influence and refpectability,—It were to be wifhed that they and other literary characters in town, had more concern for the improvement of the community at large, and would make fuitable efforts to promote it.

It would be doing injuftice to the liberality of the proprietors of the Athenæum and circulating library, to deny thefe inftitutions their refpective merits and advantages. But I apprehend that neither of them is fufficient to fupply the *defideratum* The Athenæum and circulating library.

ſideratum mentioned by your correſpondent. The firſt is principally calculated for the commercial part of the inhabitants, and thoſe who have time to *lounge;* the ſecond, although comprifing much uſeful reading, is ſometimes defective in the ſelection of the books, and affords little opportunity for the union of literary exertions.

Conſideration in favour of a proprietory aſſociation.

A ſociety whoſe books are the property of the individual ſubſcribers, is far better adapted, not only for advancing knowledge and bringing uſeful talents into notice, but alſo for giving a favourable bias to the purſuits of ingenious young men of all deſcriptions, to whom ſuch a ſociety is at all times acceſſible, from the ſmall expence attending it. People become more ſolidly concerned in promoting the ſucceſs of any ſcheme, in proportion as their perſonal intereſts are interwoven with it; and we may therefore conclude, that a man will take more pleáſure, and perhaps derive more profit, from reading a book which he conſiders as his own property, than one only lent him for a time.

Subſcription-library eſtabliſhed Feb. 1805,

Impreſſed with theſe conſiderations, a few perſons in Aberdeen inſtituted a ſubſcription-library upon the 22d February, 1805, under the title of the *Caledonian Literary Society.* Beſides embracing all the periodical publications of merit in Great Britain, our ſtock is enriched with a ſelection of the moſt approved books, either preſented by the members or purchaſed from the Society's funds: Which Society has *already* increaſed to upwards of 100 members, and the liſt is daily augmenting in number and reſpectability.

at a very moderate expence.

It is worthy of remark, that the trifling ſum of ſix ſhillings *per annum* is only required from each ſubſcriber to *The Caledonian Literary Society.* So inconſiderable an expence, contraſted with the great variety of uſeful and entertaining knowledge to be derived from it, muſt form a very powerful recommendation in its favour.

We have been informed with pleaſure, that many perſons in Glaſgow, who are not members of the Society eſtabliſhed there, have contributed liberally to its ſupport by giving books —an example worthy of the imitation of others.

A Philoſophical Society in contemplation.

It is alſo in contemplation to inſtitute a Philoſophical Society, on a plan ſimilar to thoſe of London, Edinburgh, &c. for the purpoſe of receiving occaſional diſſertations on a variety of

literary

literary and other subjects, to be deposited as the property, or, entered into the books of the society; and afterwards published in such manner as the society may direct.

Should any of the friends of science in Inverness, Banff, Peterhead, or other places, be desirous of establishing similar institutions, we will most chearfully furnish them with a copy of our plan and regulations.

We have a sincere wish to see every encouragement given to undertakings so laudable and beneficial, and have with this view made the present communication, to give publicity to ours through the medium of your excellent Journal. The insertion of the above will oblige, Sir,

With respect,

Your humble servant,

ALEX. CROMBIE, *Pres.*

Aberdeen, January 2, 1806.

XIV.

Letter from Mr. James Stodart, *in Answer to a Question concerning the Effect of the Nitrous Oxide, proposed by Dr. Beddoes.*

To Mr. NICHOLSON.

Dear Sir,

Dr. Beddoes, in a paper on the medical effect of respiring the nitrous oxide, published in the last number of your Journal, refers to an account I formerly gave of some unpleasant and rather alarming sensations experienced after inhaling that gas. He attributes the whole to hysteria or nervous affection; at the same time signifying a wish that I would state whether or not that was really the case. In answer to this I have only to observe, that if any such predisposition to hysteria did exist, it was wholly unknown to me. My general state of health was as usual; nor had any thing occurred particularly to affect the mind. I had often inhaled the nitrous oxide under circumstances in every respect similar (at least as far as I can judge) and till that time, so far from experiencing any thing like

Qu. Whether Mr. Stodart was nervously affected previous to his feeling inconvenience from nitrous oxide.

Reply: that he was not.

like debility, the very contrary effect was produced; namely, found and undisturbed sleep in the night, followed by strength and increased chearfulness on the following morning.

Expectation that the nitrous oxide may prove eminently useful, &c.

I very sincerely hope the medical application of this extraordinary agent, directed as it is by the very able hand of Dr. Beddoes, may prove as important and useful in medicine as it is interesting and curious in philosophy.

I have not yet heard of its being tried in cases of suspended animation; it appears to be an experiment well worth making. The subject is perhaps worthy of the attention of the Humane Society. I am with respect,

Dear Sir,

Your's sincerely,

JAMES STODART.

Strand, January 22, 1806

XV.

Description of a Statical Lamp, which maintains a Supply of Oil to the Burner from a Reservoir, placed so low as to occasion no Interception of Light. By A. F.

To Mr. NICHOLSON.

SIR,

Description of a new statical lamp.

I SEND you a sketch of an overflowing lamp, of which the construction will be easily deduced from the figure. Its advantages are, that the flame is supplied from below, and the light is not intercepted, but falls on all surrounding objects as directly as that of a candle. The upper part of A (see Plate IV.) contains the usual apparatus of a lamp, either according to Argand's construction or any other; and the column or tube which supplies the oil may be no longer than that supply and the conditions of the structure may demand. The vase below contains the oil, which is poured in, when needful, at the top of the column, by a funnel or otherwise. The circle round B, C, represents a globular (or cylindrical) vessel, having no communication with the vase except through a neck

a neck or pipe D, proceeding downwards nearly to its bottom; but there is a communication with the external air, through a perforation (represented by a small shaded circle near B) which prevents the atmosphere from interrupting the intended action. The lightly shaded semicircle B represents an hemispherical solid capable of revolving on an horizontal axis, so as to hang downwards and fill the lower half of the globe, when no fluid is present; or it can be raised up by floatage into any other position, according to the quantity and density of any fluid that may be poured in.

Description of a new statical lamp.

Let us now suppose the vessel C to contain any fluid not more than half its capacity, and that the revolving piece B is of such a weight as to be of half the specific gravity of that fluid: it may then be easily understood that the piece B will settle into such a situation as that part of it shall be immersed in the fluid and support it in the vessel, exactly to the height of its axis. For the part of the solid, immersed on one side, is exactly equal to the space above the fluid in that situation, on the other side; and the greater part of B which is on one side of the perpendicular will exceed the smaller part on the other side, by exactly double that quantity. Consequently the immersed part of the solid will be pressed down by twice its own weight; and this is exactly equal to the weight of fluid which it displaces; whence the body and the fluid will be in equilibrio. Let us now suppose the fluid to be brine, at the specific gravity of 12, which may be poured in either at the top or at the side hole, and that oil of the specific gravity of 9 be then poured upon it; and it is manifest that the oil will press the dense fluid upwards into C, as represented in the figure, and that when C is half filled, the oil will stand at an elevation above the axis equal to one half more than the height of the dense fluid, measured from its surface where the oil presses upon it. And, when this adjustment is once made, by putting in the proper quantity of dense fluid, if any of the oil be taken out, or consumed by burning, the pressure will be less, and the dense fluid will rise within the vase. But this rise will not be attended with any depression in the vessel C, because the level will be kept up by the revolving piece B, and consequently the oil itself will be prevented from falling as much as it would have done if this contrivance had not been applied.

I do

Description of a new statical lamp.

I do not disguise the consideration, that as the oil diminishes, the distances between the upper and lower surfaces of the dense fluid must diminish, and a proportional difference or subsidence in the surface of the oil must take place. The proper remedy for this appears to be that the lower surface should be made as large as convenience will allow; that its rise and fall may be less.

With regard to the disposition and form of the spaces which are to contain the oil, it is only needful to observe that they may all be made small or narrow; except that which is alternately to be occupied by the oil, and the dense fluid. If the height of the dense fluid be 12 inches, the lamp may stand 18 or 20 inches high, using salt water as above mentioned.

There are various practical objections to mercury; but if this fluid were to be used, the oil might be raised ten times as high, or the apparatus, if required, might be constructed with a less distance between the surfaces.*

I am, Sir,

Your constant Reader,

A. F.

XVI.

Letter from a Correspondent rectifying some Particulars of Misinformation respecting the Fishery of the North of Scotland.

To Mr. NICHOLSON.

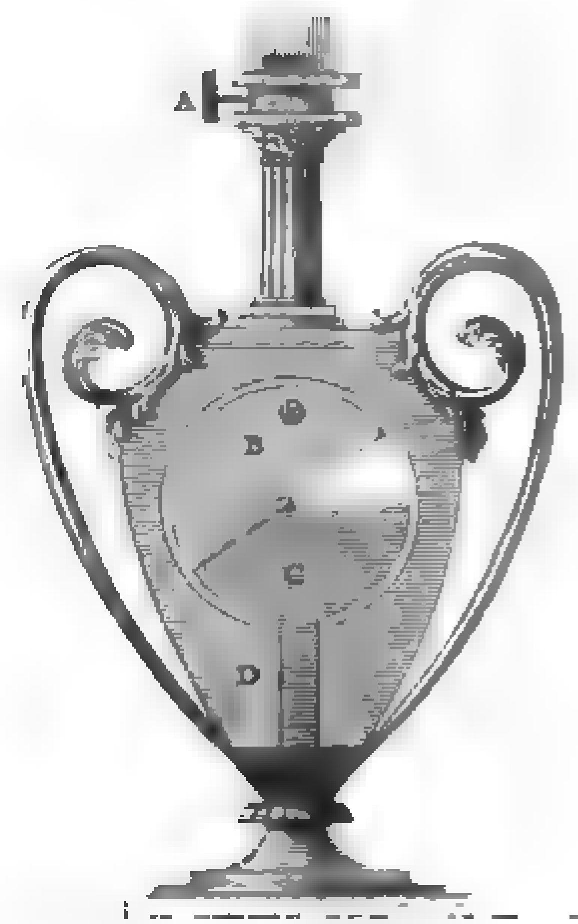
SIR,

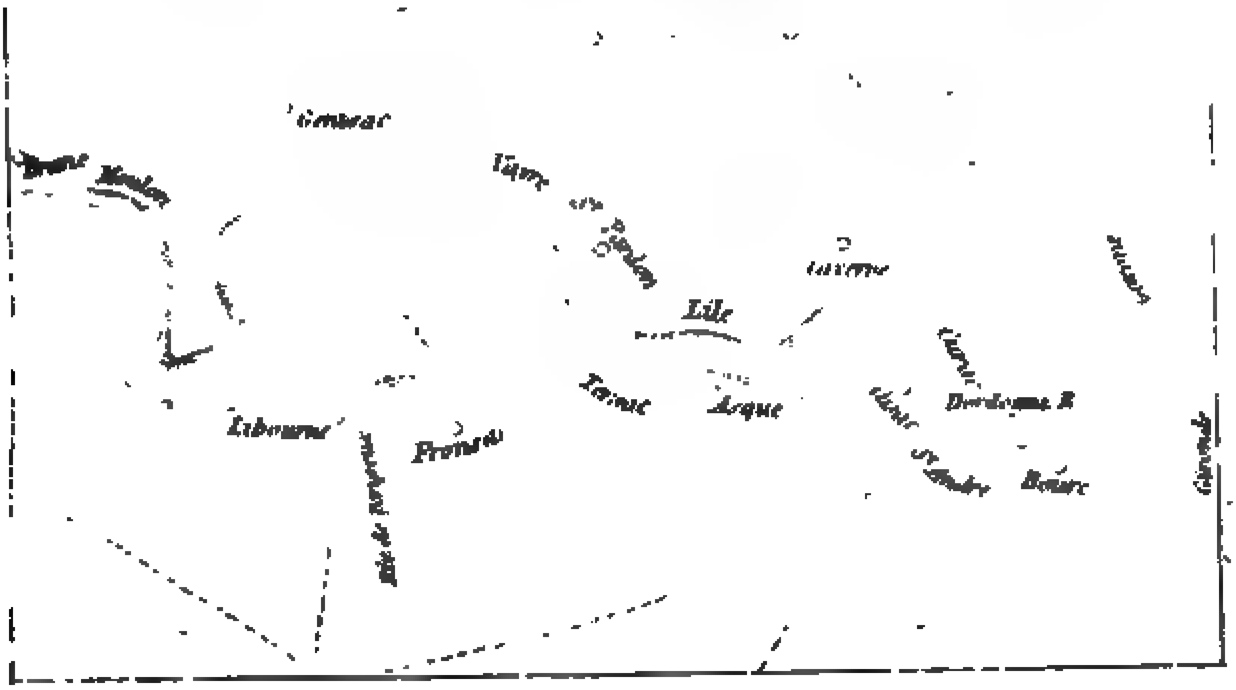
WHEN any important information is communicated to the public, we have a right to expect that it should be given with extreme accuracy; or at least where any doubts exist, with such a degree of diffidence and modesty, as may leave room for avoiding misrepresentation or falshood.

* The contrivance for keeping a fluid at its level by a semi-circular revolving solid was invented by Robert Hooke. See Birch's History of the Royal Society. A. F. has ingeniously adopted it to a lamp which casts no shadow. Hooke's lamp is nearly as faulty as the common fountain lamp in this respect. N.

I wish

Nicholson's Philos. Journal. Vol. XIII. Pl. 4. p 200





I wish *an Enquirer*, in your Journal for December last, had attended to this, before making what I conceive to be a hasty, ill-founded statement, respecting *certain instances of wasteful negligence in some of our fisheries in the north of Scotland*, which it is my duty at present to controvert.—He states:

Erroneous information respecting the fisheries on the north of Scotland.

1st, "That the fishermen of Aberdeen, Banff, Peterhead, &c. never think of carrying their fish along the coast southward, which they might do to Leith in twenty-four hours; or with a good brisk wind to Berwick-upon-Tweed, or even Newcastle-upon-Tyne; but when their respective towns are supplied, *they throw the remainder upon the dunghill for manure!!*"

A fact so improbable as the above, would indeed, require no ordinary share of proof to gain credit to it, and I have the satisfaction to assure you that it is entirely without foundation. The truth is, the number of hands employed in the fisheries in the north of Scotland are so few, and the encouragement given to enterprize and speculation in this important source of national wealth so small, that no more fish is caught than what supplies the neighbouring towns. But even admitting that more were caught, and that we could vend at Leith, Berwick-upon-Tweed, or Newcastle-upon-Tyne, is it not to be supposed that fishers of places nearest to these towns could afford to greatly undersell us?

From various causes this source of wealth is neglected.

When the dog-fish (*squalus catulus*, L.) appear on the coast, our fishers catch a great number of them and dry them for their own private use (for none but themselves and the lower classes of people would use them) and likewise for the benefit of the oil, which they yield in great abundance, and the skin, which is used for smoothing the surface of wood. After they are drained of the oil which they contain, besides keeping a sufficient number for use, they throw the remainder on their dunghills, which produces a *valuable* manure. And no doubt your correspondent may have mistaken these for any other kind of fish.

The dog-fish caught for food, for oil, for its skin, and for manure.

He next observes; "That at Arbroath, another custom, equally as extravagant in its kind prevails, and of which I have been a witness; the crab fishery is so productive, that after boiling them, the bodies of the crabs are thrown away, and *the large claws only* brought to table."

It is

The claws of crabs only are sold at Arbroath, but the bodies are not thrown away.

It is indeed, generally the case here, and in every other fishing town, that the fishers for the most part retain the bodies of the crabs, and only dispose of the claws in the public markets: but that the former are thrown away, is by no means true in almost any instance; for the fishermen find them of far more value in baiting their hooks, than what they could get for them otherwise. Indeed, if it were not for this purpose, it is believed, few or no crabs would be caught at all.

Much profit might be derived by a company if established at Aberdeen for exporting white fish.

Having thus endeavoured to vindicate our fishers from the charge of wasteful negligence, which none who know them will think them guilty of; I cannot conclude without expressing my surprize that no company has yet been established at Aberdeen for exporting white-fish. It is obvious from its excellent situation, and advantages, that very handsome profits could be cleared, if such an undertaking were once set on foot, and well conducted; equal, if not superior to the salmon fishing, which it is well known has been greatly the means of enriching this place.

If you deem the above observations worthy a place in your useful Journal you will oblige,

SIR,

Yours respectfully,

A. L.

Aberdeen, January 3, 1806.

XVII.

Observations and Enquiries concerning the Heat of Air blown from Bellows. By K. H. D.

To Mr. NICHOLSON,

SIR,

Passage from Dr. Black's lectures.

I BEG leave to mention a passage in Dr. Black's Lectures on the Elements of Chemistry, published by Professor Robison, which occurs at page 88, Vol. I.

The author is speaking of the communication of heat, and has, in the former part of the page accounted for the apparent coldness of a stream of air, by its preventing the accumulation

mulation of heat around our bodies, by its impulse and rapid succession, both cooling our clothes faster, and carrying away the warm air that was intangled in them. The Doctor says, "the sensation of coldness, therefore, produced by wind, or agitated air, is so much stronger than that produced by equally cold air in a stagnating state, that we are often persuaded the agitated air is actually colder, until we examine it by the thermometer; and Dr. Boerhaave thought the deception so strong, that he contrived an experiment to remove it completely (Boerhaave Elementa Chemiæ.) He suspended a thermometer in the air of a large room for some time, and noting the degree to which it pointed, he then directed against the bulb of it a stream of air impelled by a large bellows in the same room;—that stream of air would certainly feel to a person who opposed any part of his body to it, considerably colder than the rest of the air in the same room; *but the thermometer is not in the least affected by it.* And it would be easy to exhibit another experiment to shew, that agitated air is not made colder by agitation. A piece of ice, for example, being suspended in the air of a warm room, and blown upon by bellows, instead of being thereby kept the more cool, as our hand would be, and preserved the longer from being totally melted, would certainly be melted so much the faster, than when the air is allowed to stagnate in some measure around it."

that agitation of the air, though it cools heated bodies, does not render the air colder,

nor hotter, though it melts ice.

I take the liberty of troubling you with this in consequence of a communication from your ingenious correspondent, Mr. Richard Winter, published in the last Number of your excellent Journal, where his experiment on the effect produced on a thermometer by a blast of air from a pair of bellows, directly contradicts Dr. Black's assertion, that "the thermometer is not in the least affected by it."

M. Winter found the air from bellows gave out heat.

That there is great truth in Dr. Black's general statement of the fact, of a blast of air cooling a body warmer than itself, by affording a continued series of fresh surfaces to carry off the caloric, I have no doubt, and that it should have an equal effect in warming a body colder than itself, seems equally evident, or by supplying the colder body with caloric. But in the case of the thermometer being raised four degrees, (as stated in Mr. Winter's experiments) we are not told that it was of a temperature lower than that of the air of the room

Questions respecting these facts.

room. How then, Sir, are we to reconcile the refult of your correfpondent's experiment with Dr. Black's affertion, mentioned above?—Are we to fuppofe the blaft of air to have actually acquired an increafe of temperature, and if fo, how has it acquired it? I hope your correfpondent (fhould this ever reach his ears) will not imagine I doubt the accuracy of his experiment; my only object is, the clearing up a circumftance, which at prefent is to me at leaft, not by any means fatisfactorily accounted for. To whom then can I better apply, than to you, if indeed I may venture to hope you may think the object worthy of your confideration? Whether that fhall prove the cafe or not, I muft always feel (in common with thoufands of others) the benefit you confer on the fcientific world, by the eafy means of communication of knowledge to the public, which your Journal affords,

I have the honour to be,

Sir,

Your obedient Servant,

K. H. D.

Tunbridge,
January 19, 1806.

P. S. I do not underftand how the fuppofed greater capacity of a vacuum for caloric explains the facts, whether of the rife of the mercury in the thermometer, or the melting of the ice.

Obfervations on the preceding Letter, by W. N.

It is defirable that the experiments fhould be repeated.

WHEN a queftion arifes concerning the difagreement of facts, the procefs obvioufly indicated is to repeat the experiments; in order that it may be feen what circumftances may have tended to produce miftake, or what may have been the real difference between operations fuppofed to be the fame. Though I have not had an opportunity of doing this, I have neverthelefs thought it proper to make a few remarks.

Agitation enables a fluid to gain the common temperature more fpeedily.

When a body is immerfed in the air, or in any other fluid differing from itfelf in temperature, the body will acquire the common temperature more fpeedily (that is to fay, it will be heated or cooled more quickly) by agitating the fluid, than if it were left undifturbed;—and this for the plain reafon, that more of the particles at the original temperature will come into

int contact with it in the latter than in the former case. These remarks support and explain the facts noticed by Dr. Black and Boerhaave. Agitation of the air is merely supposed, and not that it shall be either condensed or rarified. Many facts concur to shew, that the capacities of elastic fluids for heat are encreased by rarefaction, and diminished by condensation; proofs of which we have by experiments in the air-pump and condenser, and in the late experiments of explosions produced in the chamber of the condensing syringe. If we attend to this law, we must infer that the air in a pair of bellows, being suddenly compressed by a force perhaps equal to one twentieth of an atmosphere or more, will acquire an increase of temperature; and if in this disposition to give out heat, it be made to rush against the ball of a thermometer, it will heat the mercury, and cause it to rise in the tube. Now, in order to reconcile both the results of Mr. Winter, and of Boerhaave to truth, we must recollect that bellows, like the unfortunate traveller in Esop's Fables, can blow hot and cold at the same time. If the thermometer be held very near the aperture, the warm air will heat the mercury; but if it be held at a greater distance, where the warm air has become plentifully mixed with cold, the effect of its temperature may be altogether inconsiderable, while that of the agitation continues to be effective: that is to say, the thermometer if already at the common temperature, will neither rise nor fall; if it be already hot the steam will cool it; or if cool the steam will heat it. Thus it is, to return to our traveller, that we breathe upon our fingers held close to our mouth when we mean to warm them; but when we wish to produce cold, we hold the subject at a distance, and blow at it.

Air is heated by condensation.

Whence the rear blast of bellows will be hot, and the remoter will act only by its motion.

As the thermometer falls in the pneumatic vacuum, I suppose there may be some mistake in the postscript.

XVIII.

Account of the Performance of the patent Ship Economy at Sea, in a Voyage to the West India Islands, and of some Improvement in the Tackle aboard, proved of great Utility. By Mr. J. Whitley Boswell.

To Mr. NICHOLSON.

Dear Sir,

Description of the ship's construction has been published in a former number of this work.

As in a former number of your Journal *, you favoured me by inserting a description of the construction of the ship Economy, built according to my patent, I hope you will also admit the following account of her performance at sea, and of some other matters; of considerable utility to naval concerns.

The subject proper for the Journal as containing an account of an experiment in the arts important to the nation, and on a great scale.

Your Journal is principally devoted to the furtherance of the most useful of all knowledge, that of experiments in Philosophy and the Arts. And to a nation which like this depends on its shipping for most of the many advantages it enjoys over the rest of the world, what experiments can be more important, or ought to be more interesting, than those which concern this subject?

The experiment which has been made on this occasion is entitled to a farther superiority over other usual experiments, an account of the large sum of money required for conducting it, which altogether rather exceeded 5000*l.* and on this occasion it is but justice to mention the spirit with which Wm. Lushington, Esq. of this City, and Richard Griffith, Esq. of Dublin have came forward to assist in making this experiment, whose property the ship principally is, (my share of it being comparatively small to theirs); to those gentlemen this country is chiefly indebted for proving a matter of great utility to its naval concerns, and which sooner or later must be of the greatest advantage to it, when the plan comes into use, though the spirit of the times may defer this period until it shall cease to be of any benefit to us, and others may reap the profit of these gentlemens public spirit and my labour and study; but as I waited till I should have the proof of actual experiment to add, to that of a theory (which though founded on un-

Gentlemen who have assisted in this experiment.

The plan must be of great use to the nation when adopted,

* Vol. IX. p. 166.

erting principles, and of which each part had been often proved in detail before; it could not be expected to convince those whose pressure of business, or want of taste for such studies, deprived of time, or inclination, or made it too great a labour to attend to its demonstration in any other way) I shall hope now, (that my exertions to bring this plan of ship-building into the notice it deserves, when its sufficiency, strength and security is,) supported by actual and severe proof, will meet with a fair and candid consideration, from the direction of our navy, and those whose commercial pursuits lead to employ vessels of great burden.

hopes that the success of the experiment will accelerate this period.

The chief advantage of this method of ship-building is, that it enables the builder to use timber of much less cost, and vastly more easy to procure, with strength and stability superior to the old method, in proportion to the quantity of timber, and to dispense with knee timber entirely.

Economical advantages of this method of ship-building, cheaper timber, more easily procured, may be used in it.

In a national point of view this method is still of greater benefit; for as it admits of timber of fifty years growth to supply the place of that of one hundred, not only the forest lands may be made to produce timber for double the number of ships for our navy in a given time, but private gentlemen would be also induced to plant more timber for this purpose, from the superior profit they could in this case make of their plantations, and the hope it would give them of being able to receive the fruits of their labour during their own lives, which at present can only be expected to be reaped by their grand children.

Its national advantages, the forests could supply double the quantity of the timber wanted in this plan in a given time, more timber would be planted if it was adopted; oaks of fifty years, have more serviceable timber in proportion than those of 100, and occupy only one fourth the ground.

An oak of fifty years growth has also a much greater quantity of serviceable timber in it, in proportion to its age, than one of an hundred years, and four times the number of them at least can stand and flourish at one time at the same extent of ground; so that the public would be benefited by the adoption of the plan every way; for while timber would thus be rendered more plenty, those who prepared it for market would also obtain a greater profit.

Hitherto the price of timber for the navy has been attempted to be kept down by arbitrary regulations, which tended to encrease its scarcity; at last, notwithstanding every effort, the price and scarcity have encreased so much that our government have been forced to the expedient of partly relying on a foreign country for the continuation of the navy; and to depend

The scarcity of ship timber has compelled this country to build ships of war in Russia; danger of this expedient.

depend on the dock yards of Ruſſia for the bulwark of the Britiſh nation, for the defence of its liberties, and of its politi exiſtence, and this at a time when our crafty and implacable has got poſſeſſion of nearly all the foreſts of the reſt of Europ and is making the moſt prodigious exertions to out-numb our navy.

Should induce the trial of the patent plan, in doing which there is no riſk, as its ſufficiency has been proved.

If my plan of ſhip-building tends in ſo great a degree diminiſh thoſe difficulties, and even dangers, as is ſtated above is it not worthy of a trial at leaſt, even if ſome riſk was ru in that trial? but when no riſk is run, when the plan ha been proved, the moſt ſcrupulous economiſt of the publi wealth can ſtart no objection to that trial of it in the navy that the public neceſſity for ſome expedient to ſupply timb for its uſe ſo loudly calls for.

No public money required to make experiments, they have been already made at the expence of the owners of the ſhip.

We aſk no drafts on the public ſource to try experiment on the ſubject, theſe have been already compleatly made a our own expence, and all we demand is our country to con deſcend to reap the fruit of our exertions; if ſhe does, w ſhall rely on her generoſity to recompence us, convinced th ſhe will have ample proof that we have deſerved it; bu ſhould this not be the caſe, we will not reſt contented with having diſcharged our duty, in doing the moſt we could t ſerve her; which if we ſhould be ſo happy as to effect, w will never regret our trouble or coſt.

The performance of the ſhip at ſea cannot be miſtated on account of its publicity.

Having thus ſtated the claims which the ſubject has to pub lic attention, I ſhall proceed to relate the performance o the ſhip at ſea, which, as ſhe ſailed in company with a larg convoy both out and home, is a matter of too public a natur to admit any miſtatement I might wiſh to make, which Go knows is far from my deſire.

On the 22d of Auguſt, 1804, The patent ſhip Econom weighed anchor off Graveſend, with but a ſmall cargo aboar as is uſual for ſhips outward bound to her deſtination, and ſe ſail on her voyage to Trinidad and Grenada; and on the 14t October following arrived at Grenada; her performance o this voyage is beſt ſtated in her Captain's own words, in the following extract from a letter to Wm. Luſhington, Eſq London.

SIR, *Grenada, Oct.* 15th, 1804

The Captain's letter relative to the voyage out.

I have the pleaſure to inform you of the ſhip Economy's ſafe arrival here yeſterday evening. We had a fine paſſag and

and had but one gale of wind: The ſhip performs as well as it is poſſible for a ſhip; is remarkable eaſy at ſea, ſteers and ſails well, and is perfectly tight. In the gale of wind the Epervier man of war ſprung her foremaſt; the Robert Aylward ditto; a brig, Maſter or brig named Swinger, loſt both top-maſts and parted convoy in lat. 14. 30 N. Our ſhip behaved extremely well and never ſtrained a rope yarn.

Ship ſteers and ſails well, is remarkably eaſy at ſea, perfectly tight in a heavy gale, in which other ſhips ſuffer much, ſhe meets no accident:

(*Signed*) ALEXANDER SMITH.

From the period of this letter ſhe remained at the Weſt India iſlands until the 23d of July 1805; being detained there the greateſt part of that time by the arrival of the French fleet, which was afterwards chaſed back to Europe by the gallant and ever to be regretted Lord Nelſon; from the 23d. of July, when ſhe ſailed for England, to the 29th of Oct. when ſhe caſt anchor off Portſmouth on the Mother-bank, ſhe experienced a ſeries of ſevere weather, and violent gales of wind, in which ſome of the fleet with which ſhe returned foundered, and others were obliged to bear away for America for ſhelter. The remarkable bad paſſage home of the Leeward iſland fleet, of which ſhe was one, is too well known to need much deſcription: all ſeamen muſt be ſenſible that three months toſſing on the Atlantic ocean in ſuch hard weather, beating up againſt contrary winds, to a veſſel as deeply laden with ſugar as the ſkrews could compreſs it into her, muſt have been a moſt ſevere trial, and that if ſhe had a ſingle weak part, or defective principle in her conſtruction, it muſt have given out in that time: but while moſt of the other ſhips of the fleet met with more or leſs damage both to themſelves and their cargoes, ſhe bore through all without the ſmalleſt accident, and brought home her ſugar perfectly dry and ſafe; which was not compleatly diſcharged until Jan. 1806 (on account of her detention at Portſmouth, through contrary winds from whence ſhe ſhe did not get to London before the 27th. of Nov. on which day ſhe hauled into the Weſt India dock,) or this account would have been made public before. A further proof of the ſtability of her frame work, is her taking the ground with a full cargo on board without any accident, as may be ſeen more particularly in the following account of her performance home which I received from her Captain.

delayed at the Weſt Indies by the French fleet, returns home, experiences violent gales, and a tedious paſſage of three months, ſome ſhips of the fleet founder through ſeverity of weather. Why this ſevere trial is a ſufficient proof of her ſtability, when deeply laden, took the ground without injury at Trinidad.

DEAR

DEAR SIR, *January* 17, 1806.

"IT is with pleasure that I have leisure to inform you of the performance of the patent ship Economy, during the voyage under my command.

Captains account of the voyage home, In a violent gale the ship performs extremely well, and is a good sea boat: two ships founder in this storm, one abandoned, another large ship rendered unmanageable, and taken in tow, and several others much damaged.

"On the fifth and sixth of September last, latitude 37, 34 N, we experienced a very heavy gale of wind, with an heavy cross sea, occasioned by the wind shifting to different points of the compass suddenly, and blowing with extreme violence: during the whole of the gale, the Economy behaved as well as I ever experienced a ship to do, and much better than could have been expected for so small a ship; in fine, she is as good a sea boat as ever put keel in salt water. During the gale, two ships, it is supposed, foundered; after the gale one was abandoned as not tenable, should another gale of wind come on: the Prince of Wales, a ship of 300 tons, had every thing washed from her deck: The Princess of Wales, a ship of the same size, broke her rudder, and was left in tow of the Hyæna sloop of war. Several other ships met with considerable damage, which proved undeniably the violence of the wind.

The Economy meets no accident, and is very weatherly: tho' deep laden; has a foul bottom which impedes her sailing, she works and steers well.

Notwithstanding the lumbered state of the Economy, we lost nothing off deck, and I don't think there was a ship, large or small in the fleet, that made better weather: she did not sail so fast coming home as going out, but that is easily accounted for, when we consider she was not coppered, and was out fifteen months on a wooden sheathing, with barnacles as long as your finger on her, and the bottom resembling a rock; and was besides laden as deep as she could stow. She works and steers amazingly well. I would not wish to change her if she had been larger, but being only 200 tons, she is too small both for my interest and the West India trade.

She remains perfectly tight after the severe passage, though run aground at Trinadad with a full cargo of sugar;

"The ship has been perfectly tight all the voyage, although we had a very tempestuous passage, and likewise ran her on shore, *sugar loaded*, under the batteries at Trinadad, to prevent her falling into the hands of the French, as we supposed, where we lay for twenty-four hours, until we discovered that it was Nelson's fleet. In my opinion she is one of the strongest ships in the river Thames of her size.

is a very remarkable strong ship,

"The new iron slings and other iron work on the yards exceed my most sanguine expectation, I have seen the ship covered with flashes of lightning when at Trinadad, and never experienced

experienced the leaſt injury from ſo much iron being about the yards, owing to the precaution which I took of ſerving the iron work and paying it with pitch, which I think ſerved as a non-conductor. I have a higher opinion of iron work than ever I had, and think the iron rigging in the plan we uſed to talk about while the ſhip was building, would anſwer to admiration, and might be the means of preſerving the maſts of men of war, when in action, as being leſs liable to be cut with ſhot. When I can manage it, I mean to rig the mizen maſt of a ſhip wholly with iron, to give it a trial. When I examine the bottom, I will give you my opinion of the pieces of ſheathing ſteeped in your preparation to prevent the worms from deſtroying the bottom. The large rollers which you had let in beneath the hawſe holes for the cables to work on, were of very great benefit, and I think ſaved us the labour of two men in weighing anchor, they alſo prevented the wear of the cables very much, and were greatly liked by the ſailors, as making the purchaſe more lively,

of the iron ſlings to her yards, and captain's high opinion of the uſe of iron work in rigging.

State of experiment on part of the ſheathing will be attended to,

great uſe of the large rollers for the cables.

Your's very ſincerely,

ALEX. SMITH."

The iron ſlings which Captain Smith mentions, were on a plan of his own, and different from thoſe uſed in men of war, in not requiring above three or four feet of chain for each yard, and ſerved merely to ſuſpend the yards from the point of the tops; which method greatly ſaved the wear of the maſts, and permitted the yards to work more freely. Iron ſtraps were alſo uſed to moſt of the blocks inſtead of hemp.

Further account of the iron ſlings,

The rollers for the cables were about fourteen inches long and eleven in diameter, and worked on iron gudgeons about two inches in diameter, in braſs ſockets. The rollers which have been hitherto uſed for this purpoſe, were generally much too ſmall, ſeldom exceeding the diameter of the cable; which diminiſhed ſize both increaſes the friction and injures the cable, from the ſmallneſs of the nip which they occaſion; or, in other words, from the acuteneſs of the angle at which the cable is forced to bend in paſſing over them.

and of the cable rollers,

In concluding this account I beg leave to mention, that I could, in building another ſhip, greatly diminiſh the ſpace neceſſary for the tranſverſe forms uſed in my plan, by ſetting them farther aſunder, and forming them of iron, which method

Iron tranſverſe frames may be uſed in future in this place, to ſave room.

—and the fore and aft ribs fcarped in a more economical method.

method is fpecified in my patent, and that I could alfo make a great faving in the timber ufed in the fore and aft ribs, by a method of fcarping them, alfo within the limits of my fpecification. Experience has fince convinced me of the fuperiority of both thefe methods, of which I had fome doubt when I built the Economy, or they fhould have been ufed in her.

It may feem paradoxical to affert that iron is oftentimes cheaper than wood in fhip building, when it can be ufed: but a plain proof of this exifts in the bow of the Economy, of which the three lower breaft-hooks are iron of confiderable fubftance, and yet coft lefs individually than any of the wooden ones above them, though thefe are of no extraordinary girth, or of much curvature.

The Economy will remain a few weeks at the London Docks for infpection.

The Economy will be a few weeks in the London Docks, where fhe has now moved, for the infpection of the public, and where all gentlemen who are interefted in fhipping concerns may fee her conftruction; and thofe who examined her previous to her failing, may convince themfelves that I have exaggerated nothing, as to the found ftate in which fhe has returned from her tempeftuous voyage.

Dear Sir,

Your very humble fervant,

J. WHITLEY BOSWELL.

XIX.

Experiments on the Torpedo. By Meffrs. HUMBOLDT *and* GAY LUSSAC. *Extracted from a Letter of M. Humboldt to M. Berthollet; dated Rome,* 15 *Fructidor,** *Year* 13 (*Sept.* 2, 1805.)

THE curious theory with which Volta has enriched the fcience of natural philofophy, on the fubject of electric fifh having been received as authentic by many naturalifts, renders the phenomenon of the Torpedo worthy of farther inveftigation. You know, my dear friend, what was our impatience to procure thefe fifh, and will perhaps be furprifed that fo much time fhould elapfe without having heard from us on the

* Annales de Chimie, Vol. LVI.

fubject.

subject. At Genoa, we perceived some; but we were then without our instruments. At Civita Vecchia we sought them in vain. But during our stay at Naples we frequently procured some very large and lively ones. In this letter you will find detailed the experiments made by M. Gay-Lussac and myself on the powers of this fish (*Raja-torpeda* of Linneus). M. de Buch, a German mineralogist, well acquainted with all the branches of physical science, was witness to our proceedings. I send you the results, giving simple facts, unmixed with theoretical speculations. Our experiments were chiefly directed towards the discovery of that state of the torpedo when it was least capable of exerting its power upon the human frame. This power has been generally described as electrical; but the sensation produced by it is materially different from that caused by the discharge of a Leyden phial.—Having no other book by us besides the work wherein Aldini* combines the researches of Geoffroy with those of Spallanzani and Galvani, it is not to be expected that we should compare our experiments with those which may have been previously made by other philosophers.

The torpedo found at Genoa and Naples, but not at Civita Vecchia.

The shock of the torpedo feels different from that of electricity.

1. Though the strength of the torpedo is far inferior to that of the gymnotus, it is equally capable of causing painful sensations. A person much accustomed to electric shocks, can hardly sustain that of a lively torpedo of four decimeters (16 inches) in length. The animal acts under water, and it is only when it loses strength that the fluid impedes its action. In this case, M. Gay Lussac observed that the shock is not perceptible till the fish is raised above the surface.

Powers of the torpedo inferior to those of the gymnotus of S. America. Shock of the torpedo more violent than that of electricity. It acts under the water,

2. I observed, when in South America, that the gymnotus gives the most violent shocks, without any exterior movement of the eyes, the head, or the fins: it appeared as tranquil as a person when passing from one idea to another, or from one sensation to another. Not so the torpedo: We observed a convulsive movement of the pectoral fins, each time it gave a shock, which was more or less violent according as the surface was larger or smaller wherein the contact took place.

—and seems to use more effort than the gymnotus.

3. The powers of the torpedo and gymnotus cannot be excited at pleasure, as we should discharge a Leyden phial or a conductor.

Shocks from the torpedo and gymnotus cannot be obtained but by irritating the animal.

* Memoires sur la Torpille, dans l'Essai sur le Galvanisme, Vol. II. p. 61.

conductor. A shock is not always felt on touching an electric fish; it must be irritated before it will give the shock. This action depends on the will of the animal, whose electric powers perhaps, are not kept constantly charged; yet it can recover them with wonderful celerity, as it is capable of giving a long succession of shocks.

The shock obtained by a mere touch with the finger,

4. The shock is felt (the animal being disposed to give it) as well on touching with one finger a single surface of the electric organs, as on applying the two hands to the two surfaces, the upper and under, at once. In both cases it is immaterial whether the person applying his finger or his two hands, be insulated or not.

—but the contact must be direct. Metals seem to be non-conductors of the shock of the torpedo.

5. When an isolated person touches the torpedo with a single finger, it is indispensible that the contact be immediate, as no shock will be felt if a conducting body (of metal for example) be interposed between the finger and the organ of the fish.—For this reason, the animal may be touched with impunity by means of a key, or any other instrument of metal.

6. M. Gay-Lussac having made this important observation, we placed a torpedo on a metal dish, with which the inferior surface of its organs were in contact. The hand which supported this dish experienced no shock, whilst another isolated person irritated the animal, whose convulsive movement of the pectoral fins indicated a most violent emission of the electric fluid.

Experiments which shew that they conduct,

7. When on the contrary, a person held the torpedo in a metal dish in his left hand (as in the preceding experiment), and with his right touched the superior surface of the electric organ, he experienced a smart shock in both arms at the same moment.

8. The same was felt, on placing the fish between two metal plates, whose edges were not in contact with each other, and applying the two hands at once above and below them.

9. But if the edges of the metal plates be suffered to touch each other, no shock will be felt in either arm. The communication between the two surfaces of the organs is, in this case, formed by the plates; and the new connection arising from the contact of the two hands with the plates is without effect.

The organs of the torpedo have no influence on the electrometer.

10. The most sensible electrometer manifested no electrical tension in the organs of the torpedo; in whatever way it was applied, it was not in the least affected; neither, on directing it

it towards the organs, nor in insulating the fish, covering it with a metallic plate, and making a communication between this plate, by means of a conducting thread, and the condenser of Volta, was there any indication (as with the gymnotus) that the animal affected the electric intensity of surrounding bodies.

11. As electric fish, when healthy, exercise their powers as forcibly beneath the water as in the open air, we were led to examine the conducting properties of this fluid. Several persons formed a chain of hands between the superior and inferior surfaces of the organs of the torpedo: the shock was not felt until they had wetted their hands. The action was not intercepted when two persons supported the torpedo with their right hands; and instead of holding each other's left hand, they each plunged a metallic rod into water placed upon an isolated body.

Examination of the conducting powers of water.

12. By substituting flame in lieu of water, the communication was destroyed, until the rods touched each other in the flame.

Flame does not conduct the shock.

13. It must, however, be observed, that in water, as in air, the shock was not perceptible without an immediate contact with the body of the electric fish: the least possible intervention of the water prevented it. This fact is the more remarkable, as it is known that in galvanic experiments, where the frog is immersed in water, it is sufficient to direct the silver forceps towards the muscles to cause a contraction, though a body of water be interposed, equal to one or two millimetres in thickness, or about one-twentieth of an inch.

No shock can be obtained without immediate contact with the fish.

These, my dear friend, are the principal observations which we have made on the torpedo. The experiments, No. 4 and 10, prove that the electric organs of these animals are not susceptible of any intensity or excess of charge. Their action may rather be compared to that of a combination of Leyden phials, than to the conductor of Volta. Without communication no shock could be felt: and having experienced the power of the gymnotus through very dry cords, I imagine, that where I have been affected by this powerful animal without direct contact, it had been occasioned by some deficiency in my insulated state. If the torpedo act by poles, that is by an electric equilibrium which possesses a tendency to replenish itself, experiments 5 and 6 seem to prove that these poles exist near each

Organs of the torpedo not susceptible of any excess of charge.

Doubt whether the shock of the gymnotus can be felt without actual contact with it.

Torpedo supposed to act by an electric equilibrium, the opposite state being very near.

each other, on the same surface of the organ. The shock is felt on merely touching the surface with the finger. A plate interposed between the hand and the organ, (*Exp.* 6,) re-establishes the equilibrium, and the hand which sustains the plate is not affected, because it is placed beyond the current. But if we suppose an heterogeneous number of poles upon each surface of the organ, whence does it arise, that, in covering these surfaces with two metal plates, whose edges do not touch each other, and placing the hands on these plates, the equilibrium should be found in the arms? Why, it may be asked, does not the positive electricity of the inferior surface seek at the moment of explosion the negative electricity of the next or nearest pole, but rather seek it in the superior surface of the electric organ? Perhaps these difficulties may not be insurmountable; yet the theory of these *vital actions* well deserves attentive research. Geoffroy has proved that thornbacks, who give no signs of electricity, are furnished with organs analogous to those of the torpedo. The least injury on the brain of the torpedo destroys its electric powers. The nerves are no doubt concerned chiefly in these phenomena; and the physiologist who should admit the power of vital actions, might with success oppose the theory of the naturalist, who would endeavour to explain all by the contact of the albumino-gelatinous pulp of the nervous laminæ wherewith nature has endowed the organs of the torpedo.

Objections to this notion.

Considerations of theory.

SCIENTIFIC NEWS.

Prizes proposed by the University and Academy of Wilna, in June, 1805.

CLASS OF SCIENCE AND MEDICINE.

First Prize.

To determine whether saccharine secretions take place in other organs besides those affected in diabetes mellitus.

BESIDES the diabetes mellitus of the authors on medicine, are there any other disorders peculiar to man, which, according to experiments well ascertained, produce in different organs a secretion similar to sugar, sufficiently abundant to finally occasion consumption? And what are these disorders?

5 In

In a note on this subject it is recommended to examine for saccharine matter, the fluid substance of colliquative sweats; that produced in the *fluxus cœliacus*, and in the pituitous consumption from lungs, which after death are not ulcerated; and the milk of women afflicted with the *gallactirhœa*.

Second Prize.

What are the true characters and the causes of the malady, which although not exclusively appertaining to Poland, is however called the *Plica Polonica*? Are there any means of curing this disease more succesful than those hitherto employed? and what are these means? To ascertain the true cause and cure of the Plica Polonica.

Third Prize.

What are the principal maladies of vegetables? And what is the true analogy between them and those of animals? Relative to the disorders of vegetables.

CLASS OF NATURAL PHILOSOPHY AND MATHEMATICS.

Prize.

Suppose a canal, through which a certain quantity of water *m*, flows in a given number of seconds, through a transverse section of a given depth and breadth, terminated by the two banks: If on this section a dam is constructed, at the top of which an opening is made for the water to pass, of given dimensions; it is demanded according to what law the water, elevated by the obstacle which the dam presents, will be forced to rise not only at the dam, but backwards along the canal. The law is demanded, by which water rises in a canal behind a dam, at the top of which a given opening is made.

Formulæ are required sufficiently general, to be applied not only to the quantity of water m, but to any other $m + x$. Experience not exactly agreeing with the theory, the necessary corrections must be made to the formulæ, and proofs given from facts and observations, shewing how nearly they approach the truth.

CLASS OF MORAL AND POLITICAL SCIENCES.

Prize.

As the sciences of natural philosophy and mathematics make daily advances, and are enriched with new discoveries, it is demanded—

1st.

Qu. why moral ſciences do not make the ſame progreſs as the phyſical? —If they can be farther improved? —What are the bounds to their perfectibility? —What are the beſt methods to attain this point?

1ſt. Why the ſame does not take place in the moral ſciences?

2d. Whether among the different branches of theſe ſciences, there be any capable of a farther degree of perfection? And what theſe are?

3d. To what degree are they of this nature? And what are the limits to their farther improvement?

4th. What are the moſt proper methods to advance the moral ſciences to this boundary of perfection?

It is deſired that the diſcuſſion of this ſubject may be conducted ſo as to preſent reſults, which may contribute to the perfection of that theory of Legiſlation, which is moſt conformable to the nature of man.

Second Prize.

Tenets of Adam Smith and Dr. Queſnay?

To determine (by making an analyſis of political economy) what are the points in which the leading notions of Adam Smith and Doctor Queſnay agree, and in what they differ, or are oppoſite?

This examination muſt neceſſarily produce reſults uſeful to the progreſs of political economy.

Amount of prizes, and laſt days for receiving memoirs.

The prize for each of theſe queſtions is 100 golden ducats of Holland (46*l.* 5*s.*); and the laſt day for the reception of memoirs on medical ſubjects, the 31ſt Auguſt, 1807; and for the others, the ſame day and month in 1806.

Conditions to be obſerved by the Candidates.

To each memoir ſent in muſt be attached a ſeparate and ſealed note, containing the title of the work, and the name and addreſs of the author: This note will only be opened by the Univerſity if the work ſhall obtain the prize.

Memoirs to be written in Latin, French, or Poliſh.

The memoirs muſt be written legibly, in Latin, French, or Poliſh languages. The packet ſhould be addreſſed to the Rector of the Univerſity of Wilna, and addreſſed to one of the bankers of that city, MM. Keyſer or Karner, that it may go free. The Rector will give a receipt to theſe bankers.

The Univerſity ſhall not be obliged to return either the memoirs or the drawings ſent; but the authors will always be permitted to take copies of them.

Conditions relative to property, copy right, &c. of the memoirs.

The Univerſity engages not to print any of the works ſent them, without permiſſion of the authors; but the authors may, at any time, print them if they think proper.

The

The diſtribution of prizes ſhould take place before the termination of the years in which they are to be determined. The prizes adjudged ſhall be publiſhed in the gazette. Time of diſtributing prizes.

The author ſhall receive his prize from the adminiſtrative committee of the Imperial Univerſity of Wilna, either in perſon or by deputy. The prize will be at his option, either a gold medal or 100 golden ducats of Holland.

The Profeſſors and honorary members of the Univerſity of Wilna, cannot be candidates for the prizes. Profeſſors of Wilna not to be candidates.

Revived Precipitates from alkaline Solutions of metallic Oxides.

M. Klaproth, a little before his deceaſe, diſcovered that the ſolution of the metallic oxides in the alkalis, are as eaſily precipitated in their metallic ſtate, by the other metals ſoluble in the ſame alkalis, as are the acid ſolutions of theſe metals by phoſphorus: He has made a very ingenious application of this proceſs to the analyſis of tin ores, according to the method which is deſcribed in his *Beitraege:* In this operation tungſtein is ſeparated from tungſtate of ammonia, by the addition of zinc, in the form of black flakes. Alkaline ſolutions of metallic oxides precipitated, in the metallic ſtate, by other metals.

Experiments on falling Bodies, by M. BENZENBERG.

M. Benzenberg, profeſſor of phyſic and aſtronomy at Duſſeldorp, publiſhed, ſome months ago, twenty-eight experiments made with balls well turned and poliſhed, which were made to fall from a height of 262 French feet: At a medium they produced five lines of deviation towards the eaſt, according to the determination of the plumb-line, and the theory gives four lines ſix tenths. Theſe experiments were made in the coal-mines of Schebuſch; they are an additional proof, if it were neceſſary, of the rotatory movement of the earth, of which no one now doubts. The laſt experiments, made at Bologna by M. Guglielmini, gave nearly the ſame reſults. A falling body deviates four lines ſix tenths eaſt, after paſſing through 262 feet.

Geography.

Great pains are taking in the conſtruction of an accurate map of Holland: The ſame precautions have been uſed in this buſineſs as in the meaſurement of the degree of the meridian. M. de Zach has publiſhed in his Journal the chart of the triangles which have been completed: They are joined to thoſe Map of Holland.

thofe which M. Delambre made for the great meridian; and the diftance from Dunkirk to Montreal has been taken for the firft bafe. When the triangles are finifhed, a bafe will be meafured towards the north, to ferve for the verification of the work. The Batavian republic have entrufted the direction of this map to Colonel Krayenhoff.

Chart of the White Sea, by General Kautouzoff.

Some months ago there appeared at Peterfburgh a very fine hydrographical chart of the White Sea, of which General Kautouzoff is the author: Many naval officers have worked under his direction for four years, in collecting the materials neceffary to compofe this chart. The coafts of the White Sea, of its gulphs, and of a part of the Northern Ocean, have been laid down trigonometrically. The depths have been carefully founded; and fix of the principal points of the coaft have been determined by aftronomical obfervations.

M. Lartigue's map of America in relief.

M. Lartigue having been engaged for thirty years in conftructing, at the marine depot (of Paris), a large and beautiful map of America in relief, has at length completed it. It is faid that the mountains, and the iflands, and the tints of the fea, are all exhibited in a manner moft capable of interefting thofe who make geography their ftudy.

Expedition of Capt. Lewis up the Miffouri.

Several months ago, Captain Lewis, in America, undertook to afcend the river Miffouri, in fearch of a paffage to the South Sea. Very interefting intelligence may be foon expected from this expedition.

Survey of France.

The work of the government furvey, or *cadaftre*, of France, has proceeded with activity; 2000 perfons are employed in it in the 108 departments.

Effect of Heat on Magnetifm.

Magnetifm deftroyed at 700° of heat.

M. Coulomb has publifhed an interefting memoir on the effect of heat on magnetifm. At 200 degrees of heat, two-fifths of it are deftroyed, and the whole at 700 degrees.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1806.

ARTICLE I.

Experiments on the Temperature of Water surrounded by freezing Mixtures. In a Letter from JOHN GOUGH, *Esq.*

To Mr. NICHOLSON.

SIR, *Middleshaw, Jan.* 29, 1806.

MANY philosophers have turned their attention to the dilatation observable in water when cooled below 40 or 41 degrees of Fahrenheit's scale, and also to the no less singular fact of water retaining its fluidity for a considerable time when exposed to a freezing mixture, without being agitated. But one circumstance, relating to the latter phenomenon, appears to have escaped the notice of them all; which in all probability will prove of some importance to both enquiries. Expansion of water in cooling below 41 deg.

We know from common experience, that when a hotter and colder body come into contact, the former will lose and the latter acquire heat, until they arrive at an equality of temperature. The frequent opportunities every one has of making this observation have authorised it to pass for a general rule; hence it has been concluded, that water in a state of rest may be cooled many degrees below the freezing point, and Explained by a developement of the conjecture that ice may be formed in minute crystals at that temperature.

and ſtill remain fluid. For my part, I adopted the maxim without heſitation, until the peruſal of Dr. Hope's paper, given in the ſupplement to your laſt volume, led me to reaſon in the following manner on the ſubject.

When water is expoſed to a freezing mixture, thoſe particles of it which are in contact with the ſides of the veſſel, are ſoon reduced to a temperature lower than the point of congelation; in conſequence of this, they are probably converted into minute icicles, which impart a quantity of caloric at the moment of their formation to the ſurrounding water, thereby preventing its temperature from ſinking below 32°. Theſe inviſible bodies afterwards begin to aſcend ſlowly on account of the diminution in their ſpecific gravity; and while they riſe towards the ſurface of the water, other particles will approach the ſides of the veſſel in ſucceſſion, and undergo a ſimilar transformation. This proceſs would evidently increaſe the volume of the water without reducing its temperature, ſuppoſing it to be ice-cold at the commencement of the experiment; for the hypotheſis reſts on the ſuppoſition that water freezes as ſoon as it is cooled below the 32nd degree of Fahrenheit's ſcale. This gradual increaſe of bulk will explain the appearances deſcribed by my friend Mr. Dalton, who found that thermometers filled with water continued to riſe when expoſed to freezing mixtures, until the encloſed water congealed ſuddenly, and frequently burſt his inſtruments. The reaſon why agitation accelerated the congelation of water thus circumſtanced appears to be this: When the inviſible icicles become very numerous, the leaſt motion carries them in crowds againſt the ſides of the veſſels; where the ſmall quantity of water contained amongſt them cryſtallizes immediately, and cements the whole into a film adhering to the inſide of the cup. This theory, or hypotheſis, call it whatever you think proper, evidently requires water to be what it really is, namely, a bad conductor of heat; and after forming it, I proceeded to examine the merits of it experimentally, in the following manner.

Experiment. Water at 32° was cooled by a ſurrounding mixture, and became congealed at the ſides by ſtirring, having never ſunk below 32°.

Experiment 1ſt. A ſmall thermometer was ſuſpended at the lower end of a wire, which could be moved in a vertical direction, through a hole in a horizontal bar of wood, fixed over a table for the purpoſe; a veſſel, containing a freezing mixture, of the temperature of 21°, was next placed with its

its centre under the wire; and the bowl of a wine glafs, filled with two ounces of ice-cold water, being then properly placed in the mixture, the thermometer which ftood at 32°, was immediately let down into the water, where it remained ftationary for the fpace of feven minutes. A wire, cooled to the freezing point was now introduced into the glafs, and the water agitated with it; upon which a thick coating of ice formed on the infide of the veffel; but no marks of congelation were obfervable on the wire or thermometer.

Exp. 2. The fame apparatus being ufed, with a mixture, having the low temperature of 6°, the glafs was filled with water of 58°, in which the thermometer fell to 32° in 7¼ minutes, by a ftop watch; at which point it remained ftationary five minutes longer. The glafs was then taken out of the mixture, and the water being agitated, lined the upper part of it for about two-thirds of its depth from the brim, with a porous covering of ice, but the remaining part of it was free from all incruftation.

Exp. 2. Water at 58° was cooled by an intenfe freezing mixture. It was brought to ftationary 32° and funk no lower. When taken out and fhaken the top froze.

I will venture to infer from the two preceding paragraphs, that we have all been under a miftake in concluding that water may be cooled when at reft many degrees below 32° of Fahrenheit, without congealing; at the fame time we are certain, that it will preferve its fluidity, when judicioufly expofed to great degrees of cold, and dilate at the fame time, as Mr. Dalton has proved. Now as the heat never falls below 32° in thefe experiments, the expanfion of the water in Mr. Dalton's thermometers, placed in a freezing mixture, cannot be afcribed to a lofs of temperature, but muft be owing to fome other caufe, probably to that which has been affigned above. As for agitation, the firft experiment feems to fhew its office to confift in bringing the water, crowded with minute icicles, into contact with parts of the veffel much colder than itfelf, where it is concreted into ice.

Hence water cannot be cooled and remain fluid at temperatures below 32°, as generally fuppofed. The water thermometer muft expand in cooling by fome other caufe.

Exp. 3. To examine this part of the fubject with more care, I formed a cup of caoutchouc, the capacity of which for caloric greatly exceeds that of glafs; or, I believe, that of moft other fubftances. Two ounces of water, a little warmer than melting fnow being poured into this cup, it was placed in a mixture of the temperature of 15°, where it remained eight minutes without giving the leaft indication of a tendency to freeze. The cup was now removed from the

Exp. 3. Repetition of Exp. 2 very ftrikingly in a cup of caoutchouc.

 mixture,

mixture, and gently ſhaken; upon which long icicles formed in an inſtant, projecting into the water in all directions, from the caoutchouc to which they adhered. This experiment, I have no doubt, might be made a very beautiful one by a dexterous operator, who is in the habit of exhibiting natural appearances to public aſſemblies.

After diſcovering that water will dilate without any change of temperature from warm to colder, at 32°, I began to imagine that the whole variation of expanſion under 41°, might be explained on the ſame principle, becauſe I believe all the experiments relating to the ſubject, have been made in a cooling medium, not warmer than melting ſnow.

Water expands by cooling between 41 and 32 deg. or begins to cryſtaliſe at the upper term.

In order to try the merits of this opinion, with an inſtrument larger than a common thermometer, I filled a four-ounce phial with water, and fixed an open tube into it, by means of a perforated cork and cement; but this apparatus proved my ſuſpicion to be falſe. For the place of the water being marked on the tube when the temperature was 41°, my bulky thermometer roſe immediately upon being plunged into water of 34°. This fact proves, that water expands by a loſs of temperature between 41° and 32°; or elſe, that this fluid begins to cryſtalize at the upper term; in conſequence of which the lower term, or 32°, is not, properly ſpeaking, the commencement of congelation, but the point at which the cryſtals of water begin to concrete into maſſes by aggregation.

I remain, &c.

JOHN GOUGH.

II.

Account of the Art and Inſtruments uſed for boring and blaſting Rocks; with Improvements. In a Letter from G. C.

To Mr. NICHOLSON.

SIR, *Briſtol, Jan.* 21, 1806.

Improvements in blaſting mentioned.

BY way of appendix to Mr. Cloſe's remarks on the uſe of ſand in ſtemming mines in hard rocks, and his uſeful improvement of the pricker, by making it of copper inſtead of iron, allow me to add two other improvements in the art of blaſting ſtone,

ftone, which my own experience has proved to diminifh confiderably the expence of gunpowder, while one of them, at the fame time, removes all danger from imperfect priming.

Defcription of the art as practifed in the lime ftone rocks in Somerfetfhire.

I fhall alfo, with your permiffion, as many of your correfpondents muft neceffarily be ignorant of the conftruction of the tools, give you a defcription of thofe now in ufe at the village of Shipham, in Somerfetfhire, a village wholly compofed of men, women and children, who mine after lead ore, calamine, and ochre, chiefly in a lime-ftone rock; a numerous band of fome of the ftouteft beings in England.

The iron pricker ufed without bad effect with fpar for ftemming.

Thefe men ftill ufe the iron pricker, becaufe an accident feldom or ever happens to them; owing, I believe, in a great meafure to their ftemming with fpar, and their habit of turning and loofening the inftrument at every half inch they fill.

The tools they ufe are thefe, *Plate* V.

Tools and implements defcribed. A hole eighteen or twenty inches deep and half an inch in diameter is cut by repeated ftrokes of a chiffel, the edge of which lies in the diameter of the hole and is fhifted round between ftroke and ftroke.

A. A round bar of iron, bevilled off at one end, of 18 inches long, and of the diameter of half an inch.

B. A ditto, of 24 inches, to follow when the hole in the ftone is about 12 inches deep.

C. A rod, with a loop for the finger, 25 inches long; at the bottom of which is a round flat plate of iron to draw out the pounded ftone occafionally.

D. A pricker, 24 inches long, with a loop alfo, ufed to preferve a paffage to infert the priming ftraw, while the hole is rammed or ftemmed with E, the iron rammer, 20 inches long, and which, fix inches or more from the end, is formed into a conical groove, very open at bottom, in order to enable the miner to ram round the pricker, and alfo that by its fharpnefs at the end it may the eafier break to duft the pieces of fpar dropped in as faft as wanted.

F. A hammer with a handle and ftrap, about five inches long; the iron head weighing about four or five pounds, according to the ftrength of the operator; for fome have them of fix or feven.

The work is performed wet, and the chipped ftone fcooped out with an inftrument.

They alfo have by them a bottle of water, to pour occafionally into the hole, for the wetter it is the fafter the work goes on.

At every ftroke of the hammer, the miner turns his chiffel, by which means he works the bottom of the mine in a regular circle, and is enabled to keep his perforation true.

When arrived at the depth of 18 or 19 inches, he cleans, and,

The charge of powder is an ounce (which is too much). An iron wire called the pricker is put down in the hole close to the side, and small pieces of spar are dropped in, which are slightly rammed and afterwards more firmly. The whole being full, the pricker is drawn, and a wheat straw filled with gunpowder is put down in place of the pricker.

and, as well as he can, dries his mine; then inserts his charge of gunpowder, often amounting to the unnecessary quantity of an ounce, and dropping the pricker to the bottom, with its side touching the side of the mine, he begins by dropping into it some lumps of spar; and after he has filled up about an inch, begins pounding it round the pricker with his rammer and hammer; tapping gently at first, but soon beginning to ram very hard, all the while frequently turning and loosening the circular pricker.

When the hole is quite filled, he draws it, by giving some gentle strokes on the chissel that he has now passed through the loop to draw it with.

He then takes the upper joint of a wheat straw, the smallest he can get, and having stopped the fine end with clay, if it has no knot; he afterwards places the other end, cut off very bevel and sharp, between his second and third finger of the left hand, close to where the fingers join the palm, forming his hand into a kind of bason, to keep off the wind, and drawing the open end of the straw so low between the fingers that he can but just prevent it from dropping on the ground; when pouring a small quantity of gunpowder on the orifice, and tapping with his other hand on the straw below, to shake it, it speedily is filled.

This straw must be 19 inches long for a hole of 18 at least, and a little shaved away at the bottom, but not cut open of course.

Fire is given by a piece of touch-wood,

When thrust down to the powder the train is compleat, and our operator lastly lights a piece of touch-wood, and places it so that when all on fire, it shall communicate to the train; after which he withdraws to a place out of the line of explosion, and waits its effect.

—which occasions loss of time by its failure and danger, when too rapid.

And here in blowing a well, I found that much time was lost; for not only does the wind occasionally blow away the touch-wood before it is all inflamed, but frequently the damp extinguishes it. I also found there was danger to the workman if it went off too soon, which the wind sometimes occasions, or his companion is too slow in haling him up; and we likewise found that when they worked by the day, and *we* found powder, they used an immoderate quantity.

Improvements. If a cork be thrust down the hole or well pre-

To remedy these two great evils, I pursued the following plan: the first of which was suggested to me by an ingenious neighbour and both had the desired effect.

The

The first experiment I tried was upon a single block of limestone, of about two ton weight. I charged the mine with only the common charge of a musket, as at K, over which I drove a cork, as at H, leaving one inch, or thereabout, as at I, over which I rammed spar; as at G, up to the surface of the rock.

vious to stemming, so as to leave one inch of windage, less powder will be requisite.

I then made a slit in my straw train, as at L, and passed through it, as through a loop, a cut of the German ash-tree fungus; but not liking that, as endangering the loss of the priming powder, I cut the slit in the fungus, as at N, passing the straw through the slit, and cutting a small notch on one side of the straw, as at O. When it was slid down to it, being elastic, it closed there, and filled the notch.

The German fungus or amadou is steadier and more certain than touchwood. The straw may be thrust through a hole in this fungus,

This match burns slow but sure, and no wind can extinguish it. A great advantage, as I have frequently witnessed, in making the new and beautiful towing path on both sides the Avon, from Bristol. One hundred men lose from ten minutes to twenty and more while getting out of the way during the blowing of a mine near the spot they were levelling, and all owing to the slow burning of the touch-wood match, or the wind blowing it aside.

—which burns slowly, but with certainty, and is not blown out by the wind.

This German match is, I fancy, pretty well known; it is merely the fungus of the ash-tree, macerated and hammered until it becomes as flexible as a piece of buff leather, and has been called the German match, I believe, from its general use on the upper Rhine, where, by its means, habitual smokers of tobacco can light their pipes in the open air, whatever may be the weather; and as a piece which scarcely weighs four grains is sufficient to light without danger, the largest mines, while the article is by no means dear, and always safe, inextinguishable, and regular in its burning, nothing can be more useful to the practical miner.

Account of the fungus.

With respect to sand (which I see recommended in a Dublin paper of only last week, as a new discovery in stemming), it will not always succeed, especially in those great mines of the Clifton blasters, where, often 15 or 16lb. of powder are used at a time; but I should think if stopped with a stiff clay, it would greatly encrease the resistance, especially if sufficient windage was left over the powder.

Stemming with loose sand will not (it is thought) succeed in mines upon a large scale.

The experiment I first tried, as described above, on that plan, tore to pieces, and threw four pieces of my rock to a great

The author's experiments were fully successful.

great height, ſhaking and leaving fit for loading, a good cart load in all; while, but for a wall at hand, my Shipham miner, as uſual, deſpiſing novelties, would probably have been wounded, having been with difficulty perſuaded to take that cover at four yards diſtance.

Thus, Sir, I have ſtated what I take to be improvements in this valuable art, and if they afford you or your readers any gratification, I ſhall not regret the trouble of putting them on paper, being always, Sir,

Your grateful reader,

G. C.

III.

Deſcription of a new Parallel Rule, exempt from lateral Deviation; invented by Mr. J. W. Boswell; *with an Account of the Imperfections of thoſe already made for the ſame Purpoſe.*

To Mr. NICHOLSON.

Dear Sir,

Inconveniencies from the lateral deviation of the common parallel rule.

THE common parallel rule of four pieces has been long found inconvenient, on account of the lateral deviation of the moving piece, which cauſes a neceſſity of ſhifting the poſition of the whole rule frequently, when many parallel lines are to be drawn; that, beſides the loſs of time which it occaſions, tends alſo to produce error in the parallelifm of theſe lines. For this reaſon it is in no reſpect ſuperior to the more ſimple apparatus for the ſame purpoſe, formed by a triangular plane of wood or metal, moved along a common rule; and as this latter is more ſteady, and ſerves for other purpoſes in drawing, it is preferred by ſeveral.

It is not ſuperior to the triangular plane and rule.

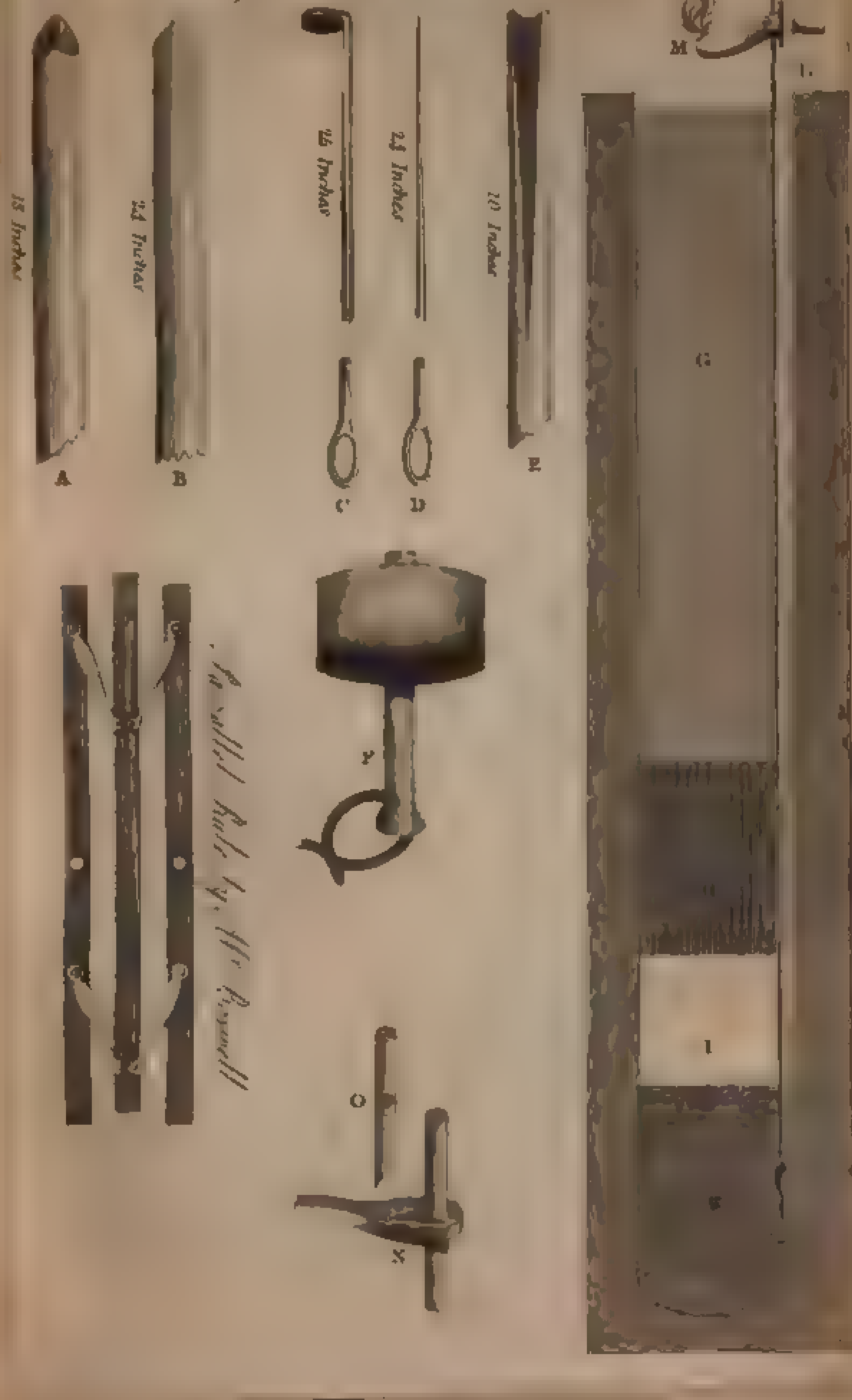
Parallel rules as yet contrived to operate without ſide deviation are ſubject to inaccuracy.

Many inſtruments have been contrived to draw parallel lines without being ſubject to the imperfections here ſtated; but all, that I know of, are more or leſs deficient in correctneſs, from requiring an exactneſs in their formation hardly attainable, or from extreme tendency to have this perfection deranged when attained.

The parallel rule with ſliding joints difficult to make exact, and

The parallel rule with croſſing connectors, and two ſliding joints, is ſubject to both the above inconveniences. The leaſt play

Method of Blasting Rocks



play in the flides, or deviation in the grooves in which they are to move, muft alter the parallelifm of the lines drawn by it; and however exact it may be at firft, the natural wear attendant on its ufe muft demonftrably produce thefe imperfections; to which may be added, that the nicety of workmanfhip which it requires, and its complicated form, muft of courfe render it expenfive.

becomes inaccurate from wear.

The inftrument formed by a rule fupported by two fmall wheels fixed to the fame axis, which axis is placed fo as to be parallel to the edge of the rule, is liable to be imperfect, from any difference in the diameters of the two wheels, or flight inaccuracy in the pofition of the axis.

This rule is alfo very liable to flip on the paper, and is rendered incorrect in its effects by any unevennefs in the furface over which it is moved.

That moving upon two wheels fixed to one axis, has the fame defects, and is liable to inaccuracy from the unevennefs of the paper.

The parallel rule, mentioned in your ninth volume, page 212, requires an exact proportion in the length of each of its parts; and as thefe are all of different meafures, would be liable to error in the firft formation, on this account; and however exactly made, would, after a little wear, foon deviate, on account of the play which this would produce in the joints; the connectors alfo between the two rules, paffing from different extremities, and leaving long fpaces beyond the points of fupport, would thereby occafion any play in the joints to produce a greater deviation from parallelifm in the lines drawn.

That formed by feveral unequal joints difficult to make exact, and liable to become very incorrect in wear from its long projections beyond the points of fupport.

The apparatus for producing parallel lines formed by the drawing board and normal fquare, can hardly with propriety be claffed among the inftruments here treated of; whatever its accuracy may be, its cumbrous form, and the time required for faftening the paper to it, render it for many purpofes very inconvenient.

The drawing board and normal fquare is cumbrous, and waftes time.

Thefe confiderations induced me, about the time when the account of the parallel rule, given in your ninth volume was publifhed, to confider how a parallel rule might be conftructed not liable to fide deviation, and as free as poffible from the defects of the others above ftated. The inftrument which then occurred to me as the beft calculated for this purpofe, I fhall now defcribe; and as I have often examined it fince, if it poffeffed any material defect, it is probable it would have become manifeft before this; in which cafe I fhould not have brought it forward to public notice.

My

Description of Mr. Boswell's parallel rule to prevent lateral deviation.

My inftrument for drawing parallel lines without fide deviation, is formed of three rulers, laid parallel to each other, connected by two pair of moveable pieces, all of equal length, and parallel to each other; thefe pieces, where they meet on the middle rule, have their extremities formed into portions of toothed wheels, which lock into each other, as may be feen in the figure: the effect of thefe fegments of wheels thus acting in each other, is, that all the lateral motion is transferred to the middle rule, while the external rules move only in an oppofite and parallel direction.

The contrivance to prevent lateral deviation cannot affect its accuracy. Its fupport at each end makes it fteady. It is eafily made exact.

This inftrument will not be liable to the incorrectnefs of thofe before defcribed, for the following reafons: 1ft. The toothed fegments being in no way concerned in producing the parallelifm of the inftrument, its accuracy of parallelifm cannot be at all affected by any trifling incorrectnefs of formation in their parts. 2nd. All the connecting pieces being of equal length, can be formed with more certain accuracy. 3d. The connecting pieces paffing from the fame extremities of the external rules, give them a fteady fupport. For thefe reafons, in my opinion, it poffeffes all the fteadinefs and facility of formation of the common parallel rule, while it effectually prevents the fide deviation, to which the latter is liable.

It might be made with but one pair of toothed fegments, but two pair make it look more uniform.

It is not abfolutely neceffary to have more than one pair of the connecting pieces made with toothed fegments; but as thefe fegments are eafily formed in the clock makers engine for cutting teeth in wheels, it can add little to the expence to make the two pair in this manner, as fhewn in the figure, and will make the inftrument look more uniform.

The middle rule fhould be made thinner than the others to prevent friction.

The middle rule fhould alfo be made a little thinner than the others, to prevent friction on the paper in its lateral movement when in ufe.

Novelty of the inftrument confifts in its toothed fegments.

In the defcription of this inftrument, it will be obferved, that the novelty of it confifts in the application of the toothed fegments of the wheels to the ufe mentioned; which I cannot find has ever been before ufed for this purpofe; and I think it highly probable it has not, as, befides its not being known to gentlemen, whom I have confulted on this head, moft likely to be acquainted with fuch matters, the fimplicity of the contrivance would probably have brought it into extenfive ufe, if it had been ever known at any former period.

Reafons for fuppofing this invention to be new.

I mention this only to fhew that, before I claim the priority of invention, I have taken fome pains to inveftigate my pretenfions;

tensions; which I think is incumbent on every man to do on such occasions: for, however fair the claim may be of invention, if a thing is well known to have been before done, it at least produces an aukward sensation to the claimer; for which reason, those who accuse others of doing this, should be the more cautious, that their accusation is fair in all its parts; for oftentimes an external resemblance may subsist between two contrivances, as between my instrument and the triple parallel ruler, and yet a small addition render their effects essentially different; thus the triple parallel ruler admits of side deviation, while my parallel ruler effectually prevents it.

A small addition to an instrument sometimes renders its effects essentially different.

My motive for publishing the account of this instrument is principally because I think it a duty incumbent on every man, who has contrived any thing that may be of use to the world, to make it known as extensively as possible, which it certainly will be by appearing in your Journal.

The instrument from which the figure was drawn was made according to my directions, by Mr. Banks, instrument maker, No. 441 in the Strand, and answers the purpose perfectly well; of course any gentlemen who desire to use parallel rulers of this kind, may have them accurately made at the same place.

This instrument made by Mr. Banks, 441, Strand.

I request the favour of your permitting the insertion, at the end of this communication, of the indication of some typographical errors, made in my paper relative to the performance at sea of the ship Economy, in your last number; and which I am the more anxious to have rectified, as some of them entirely alter the sense of the passages where they occur.

Typographical errors in Mr. Boswell's last paper relative to the ship Economy.

Page 175, line 2, erase *it* before *could*; line 6, transfer the bracket to before *when* in the next line; line 8, for *is* read *are*; erase the comma after *is*; and transfer the bracket to after *proof*; line 9, 10, for *direction* read *directors*; page 176, line 15, for *source* read *purse*; line 21, erase *not* before *rest*; page 179, line 22, for *point* read *front*; line 37, for *forms* read *frames*; page 180, line 3, for *scarping* read *scarfing*.

Some errors of the press are also apparent in the side notes, but I shall not trouble you by pointing them out, as they can be rectified by the meaning of the passages to which they are added. I am, Dear Sir,

Your very respectful humble servant,

J. W. BOSWELL.

Reference

Reference to the Figure. Plate V. Fig. 2.

A A, A A. The external parallel rules, B B the central rule, C D, C D the connecting pieces, D D the segments of toothed wheels in which the connecting pieces terminate, which by their action on each other prevent side deviation in A A, A A.

IV.

Letter from an ENQUIRER, *on the Waste of Fish asserted to be made on the Scottish Coast. In Reply to A. L.*

To Mr. NICHOLSON,

SIR, *London, Feb.* 7, 1806.

Proper spirit of inquiry and publication.

I AGREE most cordially with your Correspondent A. L. of Aberdeen, in page 168, with regard to the accuracy of important information when communicated to the public, and that when doubts exist, it should be given with so much modesty and diffidence, as to shew that the communicator is not certain of his subject. Of the statement I made respecting those instances of wasteful negligence in some fisheries of the north of Scotland, I am not the first; the respectable author of the statistical account of the parish of Peterhead, the Rev. Dr. Moir, has asserted the same, limited to that parish that I did. In the 16th Vol. p. 550 of that work, he says, " turbot (I believe the holybut of the London market) is now caught frequently, and in great perfection. Thirty years ago they were seldom used here, *frequently cast into the dunghill, or left to waste on the sea beach,* they at present sell from four pence to one shilling each, and are rising every day in price;" in the preceding page of that volume we are informed, that " the greatest part of the *cods' sounds,* in this parish, are *permitted to remain and rot on the sea beach,* or, *are cast into the dunghill,* though the use and value of them as an article of food and delicacy at table have been known here for many years," and yet in the following paragraph the Doctor tells his readers, " that the crews of the ships have been sent from this town to Barryhead, *to preserve the sounds,* tongues, and palates of the cod caught *there,* and the owners have always

Scotch fisheries.

found

found a ready market for them!" for myfelf, Sir, before I even hinted through your Journal, at thefe ftrong affirmations, I made it my bufinefs to enquire of fome friends at Aberdeen, of the truth or falfehood of fuch affertions; deeming it then, as that gentleman does at prefent, an improbable ftatement; under thefe circumftances I cannot confider the communication you did me the favour to infert, as militating either againft your correfpondents rule of examination, or my own habitual fcepticifm:—that gentleman, in recommending accuracy of ftatement, ought not to have forgotten it himfelf; he will eafily fee that *Aberdeen* is not mentioned by me as being at all concerned in this wafte of fuftenance. My little note to you has rouzed the attention of A. L.—Is it wandering too much into

"*the fairy regions of romance,*"

to hope that the fubject may obtain ftill farther notice? and continue to do fo until it be made productive of *all the advantages it is capable?* in that cafe, fuppofing defective information in my firft notice of it (and I prefume A. L. will allow I had fome authority for my opinion, and that he himfelf has not been completely accurate) my errors will be eventually attended with good. Your correfpondent well knows, that the affertion of Dr. Johnfon about the fcarcity of trees in Scotland, has had the happieft effects. How far that gentleman's queftion about the vend at the towns I mentioned, may be anfwered in the affirmative or otherwife, I have not yet fufficiently informed myfelf; but certainly under the circumftances I conceive to be true, thofe markets would be preferable to fuch wafte. The men employed may look with confidence for a fpeedy fale; and, if I am not very much miftaken, thefe towns are fupplied from the Yorkfhire boats, the wind therefore which brings the one fet of boats, would impede, if not totally hinder the other. Far am I from wifhing to throw any obftacles in the way of fo excellent a plan, as that for a fociety for exporting white fifh from Aberdeen, but where would your correfpondent fend them to? "*is it not to be fuppofed, that fifhers of the places neareft to fuch towns could greatly underfell them?*" this queftion is not a greater difficulty in the fcheme fuggefted by me of bringing the fifh to Leith, Berwick, or Newcaftle, than it is to that of A. L. The fact is, that by giving that queftion weight, competition, in every bufinefs

Good effects of inquiry and public remark.

business would be undone; for my own part, I do not think it any objection to either of our schemes; nay, our plans considered seriously, are nearly alike. I suggested, rather I believe in the form of a query than otherwise, the propriety of bringing fish where I know a demand exists; but that gentleman opposes my suggestion with the above question, and then proposes a plan on a similar, but more comprehensive scale! Allow me merely to correct at present one more error A. L. has, *unintentionally, I am certain,* committed in the last paragraph of his vindication of the Arbroath fishers (of whose methods, and *probable* waste, I hope soon to obtain a *correct* account.) It is *not* the case even for the most part in *every* large fishing town, that the fishermen "retain the bodies of the crabs, and sell the large claws only. A list of some of the towns in which that custom prevails, would, doubtless oblige many of your readers. If I am correct in my opinion of A. L. all his attempts are for the spread of useful knowledge; his candour will suggest the propriety of viewing mine also in a favourable light.

I am, Sir,

Your's and A. L's,

Friend and Servant,

AN ENQUIRER.

Notice of a Publication of Importance intended by the Literary and Antiquarian Society of Perth.

SIR,

IT is with great pleasure I inform you that the very respectable Literary and Antiquarian Society of Perth, intend giving a selection of their valuable papers to the public; it is much to be lamented that they have delayed doing this so long, as many of their manuscripts throw a very extensive light on the antiquities of that part of the island.

I am, Sir,

Your's truly,

N. L.

To Mr. Nicholson,

Newcastle-upon-Tyne, Jan. 28, 1806.

Letter

Letter concerning a Library established at Aberdeen. From a TRAVELLER.

SIR, *York Hotel, Bridge Street, Blackfriars.*

I AM extremely glad to find, that there is a ſubſcription library eſtabliſhed in Aberdeen. I am aſtoniſhed, however, to be informed from Mr. Crombie's paper, that none of the very learned Profeſſors in that part of the country are engaged in the undertaking! perhaps if the ſubſcription was raiſed to one guinea per annum, much more good might be effected, and thoſe gentlemen would not then ſcruple to join themſelves; their freedom as to pecuniary motives is well known. I hope for the ſake of the general diffuſion of knowledge, to find myſelf equally miſtaken with regard to Banff, Peterhead, and Inverneſs. The ſociety at Aberdeen, though young, ſeems to be conducted with great liberality, as appears evident from their offer of aſſiſtance to the places above mentioned, or any other that may be now forming rules; is it too much for a friend to the ſpread of uſeful knowledge to ſuggeſt to them an extenſion of the benefits of their aſſociation, to thoſe gentlemen who are members of ſimilar ſocieties, whilſt in Aberdeen, on condition of a return of ſuch civilities, ſhould any of their members be where ſuch libraries are? a rule of this kind you, Sir, have mentioned with applauſe in a former number,

Your's, &c.

A TRAVELLER.

February 7, 1806.

V.

A Chemical and Medical Examination of the Gizzards of White Fowls compared with Gelatine, together with an Expoſition of the Characteriſtics of the latter when oxigenated, By. M. BOUILLON LAGRANGE*.

IT has long been underſtood, that the gizzards of white poultry poſſeſſes certain medicinal qualities. The uſe made of it by many phyſicians may juſtify ſome reliance upon the virtues

Gizzards of fowls medically uſed.

* *Annales de Chimie*, Vol. LV.

virtues attributed to it; but no one, I believe, has hitherto thought of analysing this substance.

It occurred to me, that it would be useful to the art of healing, were a few chemical facts added to the knowledge already possessed of the medical uses of gizzard, particularly after reading in the "*Journal d'Economie Rurale and Domestique, ou Bibliothèque des Propriétaires ruraux, Pluvoise, an* 12;" a letter, wherein is announced the success obtained by its use in agues. As this letter contains the details of the preparation, and administration of this remedy, I shall transcribe it at length.

"*Amiens, 25 Frimaire.*

Letter respecting it. Recommended as a febrifuge by the French government.

"YOU mention animal gelatine as a febrifuge, I will inform you of a more simple and less expensive remedy. I know not by what fatality this great specific has been neglected, notwithstanding it was published by government full forty years ago, and in spite of its efficacy, of which I have had long experience; for, of about a thousand cases, in which during that period, I have adopted its use, I can attest the cure of eight-tenths.

"I have resided at Montpellier during fifty-six years; the climate of the place and its environs is mild and salubrious; but the inhabitants along the coast are subject to agues, on account of the vicinity of the Mediterranean, and of stagnant pools, M. de St. Priest, intendant of this province, published the order of government relative to the remedy above alluded to.

Prescription.

"*Remedy.*—This remedy consists of the gizzard of fowls, dried and pulverized.

The gizzards are washed, dried, and pulverized.

"*Preparation.*—Take the gizzard of white poultry, as fowls, turkies, &c. (I never made use of those of black fowls, as pigeons, ducks, &c.) open them, and clear away the gravel they contain; having slightly washed them, let them be put on a string and hanged in the sun, or up a chimney to dry, after which they must be reduced to powder, sifted, and kept in a bottle closely corked.

"*Dose.*—The dose is about a drachm for adults, and from half a dram to a scruple for children.

The dose is one drachm taken in wine.

"*Mode of taking.*—Mix the proper quantity of the powder in a glass or half-glass of good old white wine, and let the patient

patient swallow it about half an hour before the fit comes on, or on the appearance of the precursory symptoms of the fever. This being thrice repeated, it rarely happens that the disorder returns.

"*Regimen.*—A wholesome regimen is all that is necessary during the administration of this medicine, but the patient should carefully avoid exposure to moisture or cold, particularly in the feet."

The foregoing details lead us naturally to the following observation:

Should this substance be considered as gelatine, and possessing the same property of being a febrifuge, as stated by M. Seguin; or should we rather acknowledge it to be possessed of those particular virtues which have been attributed to it by several emient physicians? M. Pia, an old apothecary of Paris, assured me, that full thirty or forty years ago, the powdered gizzard of poultry was recommended in all obstructions of the urinary passage, in complaints of the bladder occasioned by slimy matter, as well as in all nephritic pains.

Questions whether it be gelatine.

"The efficacy of this remedy has long ago established its use;" and the writer adds, "that during my practice in pharmacy, I have prepared large quantities of it; so much were physicians and their patients satisfied with its operation."

Its efficacy,

His method of preparing the gizzards was to choose those of young fowls, and particularly of pullets: after cleansing, rubbing, washing, and wiping them carefully, he strung them, and left them to dry on hurdles between sheets of paper, assisted by the gentle heat of a stove, and not in the sun, which, according to M. Pia, would have spoiled them.

they should not be dried in the sun.

When the gizzards were properly dried, they became friable, almost transparent, and exhibited on being broken a vitreous appearance.

Vitreous appearance.

The powder obtained was of a whitish grey ash-colour, yielding in the mouth a kind of mucilage, and possessing a slightly salt and bitter taste.

Powder ash-coloured, mucilaginous and saltish.

This powder was administered twice a day, (morning and evening) in doses of twenty-four to thirty-six grains, in a glass of the infusion of pellitory of the wall; of bearberries *(uva ursi)* or of lintseed sweetened with syrup.

Dose.

Effects. The efficacy of this remedy as a diuretic and apeient, was ſo much relied on, that the afflicted even omitted the infuſion and took it in pure water with a little ſugar.

Arguments in its favour. The long experience which has been had of the ſalutary effects of gizzard as a ſebrifuge, diuretic, aperient, &c. and the publicity which the government, doubtleſs not upon light grounds, has given to this remedy, are authorities in its favour; and it muſt therefore be an acceptable labour to the phyſician, to furniſh him with new lights upon an object ſo eſſentially intereſting to humanity. This is the motive by which I have been induced to ſubmit the following experiments to the ſociety of medicine.

Inquiries as to its compoſition and uſe. As gizzard has a great analogy to gelatine, I endeavoured to diſcover their ſimilitude. If gelatine be really a febrifuge, gizzard ſhould be ſo likewiſe, particularly as it contains, when freſh, a large portion of that ſubſtance; but whence does it derive its power as a diuretic, aperient, &c.? does it poſſeſs it in common with gelatine? I cannot tell. Or, have the ſaline parts of its compoſition this double property? of this alſo I am ignorant; for practice has not yet aſcertained whether the anti-febrile quality ſhould be aſcribed to the acidulous ſalts rather than to the ſubſtances with which they are combined.

Experiments on recent gizzard. A freſh gizzard preſented the following phenomena.

A. The water wherein this ſubſtance had been boiled acquired a yellowiſh white colour, and flakes were depoſited in cooling; it had a taſte rather inſipid than ſweet.

It reddened the tincture of turnſol.

B. Lime water, and water of barytes produced in this liquor an abundant precipitate, partially ſoluble by nitric and muriatic acids.

C. Ammonia cauſed a leſs degree of precipitation.

D. Oxigenated muriatic acid ſeparated with flakes from the liquor.

E. Cauſtic potaſh, either ſolid or liquid, acted upon gizzard in the ſame manner as upon muſcular fleſh.

When ground together, ammonia was diſengaged from the gizzard; it became ſoft, of a reddiſh colour, and ſoluble in water. If this liquor be evaporated, it will depoſit fibres in cooling. Alcohol, by deſtroying the potaſh, ſeparated a flaky ſubſtance, ſoluble in water.

This

'This aqueous solution gave a precipitate on the addition of lime-water, or muriate of lime or of barytes, as well as of some acids. The precipitate obtained by lime-water may be redissolved by the addition of more water, which proves that the mixture had not become truly saponaceous, but that the potash had merely dissolved the animal matter.

F. The action of certain metallic solutions on the liquor of fresh gizzard was more or less perceptible, according to the facility with which the metal communicated its oxigen to the animal matter.

Nitrates of mercury and of silver, for example, were decomposed, but the precipitates obtained by the action of these salts upon gelatine and the extract, quickly turned black, particularly that of mercury, and they were no longer soluble in nitric acid. The oxides had, therefore communicated a part of their oxigen to the gelatine and the extractive matter, which were thus united to the mercury, now approaching a metalline state.

Oxigenated muriate of mercury was not decomposed in this manner. The circumstances, in fact, were no longer alike: the excess of oxigen which it contains sufficing to oxigenate the two substances. Here the precipitate was very little coloured, and the metallic salt was only restored to the state of mild mercurial muriate.

Some other metallic solutions produced in the liquor of fresh gizzard only gelatinous flakes; such are the acetate of lead, and the sulphate of copper and iron.

G. Aqueous tincture of nutgall changed the liquor into a kind of jelly.

Experiments on dried gizzard.

I have thought these experiments sufficient for demonstrating the nature of those substances which were capable of solution in water; yet as gizzard is not administered in its fresh state, but undergoes a process which might cause a variation in the foregoing results, I again examined it in this latter point of view.

In drying the gizzard, I followed the prescription already cited of M. Pia, and obtained a substance exactly answering his description.

A. Reduced to powder, its taste was insipid, yet partaking strongly of an animal flavour; its colour was a whitish grey.

xperiments on ied gizzard.

B. The aqueous decoction took a light yellow tint, and smelled like chicken broth.

It reddened the tincture of turnsol.

C. Lime-water and water of barytes caused the same kind of precipitate as in the decoction of fresh gizzard.

D. Oxalate of ammonia proved the presence of lime.

E. Oxigenated muriatic acid separated white flakes.

F. Nitric acid had a violent effect upon the dry gizzard; at a mild temperature it dissolved it completely.

Nitric acid at eighteen degrees excited a slight effervescence, and by gradually increasing its temperature, a separation was perceived of azotic gas, then of nitrous gas, and of carbonic acid gas.

The liquor left in the retort was evaporated, in the expectation of obtaining crystals; but on cooling, none appeared. The evaporation was then continued, the result of which was a yellowish glutinous matter, tenacious, and of an excessively bitter and acrid taste.

Water imbibed the acid, and presented all the characters of the decoction of apples.

G. Metallic solutions presented nothing particular, as in the experiments upon fresh gizzard, except that antimonial tartrite of potash was decomposed, forming in the decoction a white precipitate.

H. Aqueous infusion of nut-gall produced a less copious precipitate in this experiment, than it had with that upon fresh gizzard.

I. Dry and friable gizzard was digested in alcohol; but the liquor was scarcely coloured, even with the assistance of caloric.

This alcoholic tincture reddened that of turnsol, and gave precipitates with lime-water and water of barytes, as also with nitrate of silver; a proof that the alcohol has dissolved only the saline particles.

L. The incineration of gizzard left a residue of a saline and alcaline taste. Paper tinged by curcuma became of a deep brown.

This residue was partly soluble in water. The liquor contained sulphate, muriate, and carbonate of potash.

The part not soluble, on being submitted to the action of muriatic acid, discovered carbonate of lime, phosphate of lime, and a small portion of iron.

2 Hence

Hence it refults, that the greater part of the falts contained in gizzard, is the acid phofphate of lime; the prefence of muriate and fulphate of potafh is alfo obfervable.

Thefe falts are not only united with gelatine, but alfo with a fmall quantity of extractive matter. It fhould feem that the latter fubftance, and perhaps the gelatine, is oxigenated by the deficcation of the gizzard; for in this ftate they are lefs foluble in water.

Wifhing to afcertain the difference between pure gelatine, and that which had been oxigenated, I made experiments upon the former, of which the following is the refult.

Pure gelatine acquires different properties, according to the means employed in its oxigenation. Experiments on gelatin,

Of the metallic oxides, fome freely communicate their oxigen to gelatine, as the oxide of red-lead, and the red oxide of mercury; but the gelatine was combined with a part of the oxide, and could not again be feparated completely from it. In treating gelatine with the red oxide of mercury, a part of the oxide was reftored to its metallic ftate, and the remainder affumed a reddifh brown colour. with metallic oxides.

Superoxigenated muriate of potafh heated with gelatine, caufed no alteration in its nature. And other means of oxygenation.

Oxigen gas combined with it but flowly, and in fmall quantity. After being for a confiderable time fubmitted to the action of this gas, the gelatine only fuffered a change of colour; it became whitifh, but its characteriftics are ftill the fame.

Oxigenated muriatic acid prefented the following phenomena.

On pouring oxigenated muriatic acid gas into diffolved gelatine, a whitifh thick fcum appeared on the furface, of a moderate thicknefs, the under fide of which gradually changed colour, and became milky. The white filaments which fwam in the liquor, together with the fcum which floated on the furface, were feparated by filtering, and wafhed in cold and warm water till the water ceafed to redden tincture of turnfol. The fubftance thus prepared prefented the following characteriftics:

1. It was capable of extenfion equally with gluten, and was of a white colour. Properties of oxigenated gelatin.

2. It

2. It was very light, and ſwam upon water.

3. When well waſhed, it retained little or no flavour,

4. Left expoſed to the air, it dried, and fell to duſt.

5. It did not redden the tincture of turnſol.

6. It was ſcarcely at all ſoluble in warm water. On boiling it a length of time, in a ſufficient quantity of water, it was reduced to an infinite number of particles, ſo minute as to be hardly perceptible; but as the heat was lowered, they re-united in a maſs as before the boiling.

7. Heated nitric and acetic acids diſſolved this ſubſtance; but it was precipitated in its original form, by refrigeration.

8. Trituration with cauſtic potaſh produced a ſeparation of ammoniac.

It differs from albumen.

This matter, it will be perceived, is neither gelatine nor albumen, ſince its properties are wholly different.

The gelatin of gizzard is probably oxigenated.

It appears probable, that the gelatine in gizzard acquires by drying, properties analogous to thoſe above deſcribed; which, with the changes obſerved in the extractive matter already mentioned, wonld certainly render dried gizzard leſs ſoluble in water.

We have no means of aſcertaining, for want of a proper object of compariſon, whether this difference be eſſential to the efficacy of gizzard; and I know not if freſh gizzard has ever been adopted in medical practice. I could only wiſh to aſcertain if its febrifuge quality exiſt in the oxigenated gelatine, in the extractive matter, or in the acid ſalt. Indeed, on comparing the quantity of gelatine adminiſtered to patients, according to M. Seguin, with the doſe of powdered gizzard, above-deſcribed, a great difference will be obſerved; and yet according to thoſe who have made uſe of it, a ſmall doſe of powdered gizzard is ſufficient to check the fever.

The compariſon which I have made of gelatine with gizzard is ſufficient to eſtabliſh a material diſtinction between them.

Experiments on gelatine.

Pure gelatine poſſeſſes a weak inſipid flavour; does not redden tincture of turnſol; is mucous and gluey between the fingers; aſſumes in the fire a concrete, ſolid, and tranſparent appearance; and is ſoluble in boiling water.

Solution of barytes or of lime mixed with that of gelatine, cauſes a precipitation of phoſphate of lime.

Sulphates

Sulphates of copper or tin, and acetate of lead, experience no decompofition. Experiments on gelatine.

Nitrates of mercury and filver are decompofed, but the precipitates are much lefs copious than thofe produced with the decoction of gizzards

Solution of tartrite of antimony only thickened the liquor.

Alcohol likewife has but little power over gelatine. The precipitates obtained by means of the water of lime or of barytes, as well as that by nitrate of filver, are fcarcely perceptible.

The decoction of frefh gizzard when fuitably evaporated, leaves a coloured gelatinous matter, foluble in water, which reddens tincture of turnfol; gives copious precipitates with lime-water and water of barytes; decompofes fulphates of iron and copper, acetate of lead, muriate of tin, tartrite of antimonial potafh, and nitrates of mercury and filver; the precipitates refulting from thefe decompofitions are generally too confiderable to be attributed folely to the gelatine.

Dried and powdered gizzard poffeffes characteriftics ftill more diftinct from thofe of pure gelatine, whence I conclude that the latter fubftance has a different operation.

I leave practitioners to decide on the advantages which the medical art may derive from gizzard; it is for them to decide whether much confidence is to be placed in the notice inferted in the Journal d'Economique. And if it fhall appear that the medieal ufe of this material has been attended with fuccefs; it will perhaps be proper to attend particularly to other fubftances which have not hitherto been fuppofed to poffefs any febrifuge virtue; fuch as the falts with excefs of acid, the oxigenated extractive and even oxigenated gelatine,

VI.

On Pirite found in France by M. Coq, Commissary of Gunpowder and Saltpetre Works at Clermont-Ferrant, with an Analysis of this Substance. By J. J. DRAPPIER, *Teacher of Chemistry at the Polytechnic School.**

Crystals of pirite found in the district of Puy de Dome,

M. COCQ found the crystals of pirite, in a porous grey porphyry, with a base of feldspath, and containing crystals of quartz, forming a part of that chain of primitive mountains which support the volcanoes of the district of Puy-de-Dome. These crystals of pirite separate from the rock, and leave in the porphyry an impression perfectly smooth.

He also found at the village of St. Avit, and in the vicinity of Pont-Gibaud, a substance which appeared to be pirite; in both situations it was so indeterminate as to render it impossible to pronounce exactly on its nature. But in returning to Menat, at twelve leagues to the north of Clermont, he perceived the granites resuming the same appearance of those which he had observed near Saint Avit and Pont-Gibaud, sometimes the colour of the feldspath inclined to purple, and oftentimes this substance appearing alone in the mass of the granite, exhibited a beautiful purple.

—and on the way to Menat.

The grey porous granite appeared again at intervals, with the appearance of the crystals observed in the same rock near St. Avit and Pont-Gibaud; at last, after a great many searches, he found the pirite well defined, and assuming a character much more determinate than that of Schoenberg.

Its Physical Characteristics.

Each crystal is a prism of twelve faces, of a blackish or greenish brown, with a smooth surface.

Its colour is a greenish or blackish brown. Its form is a regular hexhedral prism, of which all the lateral edges are truncated, which constitutes it a prism of twelve faces. Sometimes the prism has also a small face at each of the angles of its base, which has not hitherto been remarked in the pirite of Saxony.

The surface of the crystals is smooth, and a little brilliant; in its interior, the pirite is dull, containing at times some particles of mica.

* Journal des Mines, Vol. XVII. p. 307.

Its

Its fracture is unequal, with a fine grain, approaching to a ſplintery fracture.

Its fracture is unequal, with a fine grain.

It admits of being ſcraped by a knife, and yields a duſt of a bright grey colour; it is tender, and does not adhere to the tongue, though it is a little unctuous to the touch.

Yields to the knife, does not adhere to the tongue, and is a little unctuous to the touch.

Beſides the ſize of its cryſtals, their faces, the ſubſtances to which they are found attached, added to the characters deſcribed, eſtabliſh the identity of this mineral with the pirite of Saxony.

The cryſtals found in Auvergne are more perfect than thoſe of Scheenberg; they exhibit no alteration, and the purity of their form removes all doubt of there being any neceſſity to claſs this ſubſtance as a new ſpecies.

The external characters of theſe cryſtals leave no doubt of their identity with the pirite of Saxony.

Analyſis by M. Drappier.

The pirite of France, ſeparated carefully from its bed, and reduced to a fine powder, is attacked and diſcoloured by muriatic acid. This acid diſſolves the oxide of iron, the colouring principle, and a portion of the alumine: but as it leaves a conſiderable reſidue, on which it appears to have no action, M. Drappier thought the method of analyſis ſhould be changed: he then took 100 parts of this ſubſtance, and kept it at a red heat in a crucible of platina for half an hour; after it was cooled, there was a loſs of ſeven parts. The remaining 93 parts were heated in the crucible for three quarters of an hour, with three times their weight of cauſtic potaſh, purified by alcohol. The fuſed maſs, detached from the crucible by diſtilled water, diſſolved entirely in muriatic acid. The ſolution evaporated almoſt to dryneſs, and then dilnted with a freſh quantity of water, let fall a white precipitate, having all the characters of ſilex. This precipitate waſhed carefully and well dried, formed 0,46 of the ſubſtance ſubmitted to experiment.

Analyſis of the French pirite.

One hundred parts loſe 7 by heating.

The remainder fuſed with potaſh diſſolves in muriatic acid.

The ſolution evaporated and treated with water depoſits 46 parts precipitate.

The remainder of the muriatic ſolution was decompoſed by cauſtic potaſh. It immediately formed a precipitate, which ſoon diſſolved again in the exceſs of alkali, with the exception of 2½ parts of oxide of iron.

The reſidue treated with cauſtic potaſh leaves 2½ parts iron oxide.

The alkaline ſolution ſaturated by an acid, depoſited 42 parts of an earth, which had all the properties of alumine. All theſe precipitates, before they were weighed, were waſhed carefully, and heated to redneſs in a crucible of platina.

The alkaline ſolution ſaturated by an acid depoſits 42 parts alumine.

Analyſis

Analysis of the Pirites of France compared with that of Saxony.

Analysis tabulated.

	Pirite of France.	Pirite of Saxony analysed by Klaproth.
Silex - - - -	46,00 - - -	29,50
Alumine - - -	42,00 - - -	63,75
Oxide of iron -	2,50 - - -	6,75
Loss by calcination	7,00	
Loss - - - -	2,50	
	100,00	100,00

Remarks on the difference of the pirite of France from that of Saxony.

M. Drappier thinks, on comparing his analysis with that made by M. Klaproth, that it may be concluded, supposing there was no error in either analysis, that either the pirite of France is not the same substance as that of Saxony, or that minerals having the same external characters, and especially the same form, may vary both in their chemical properties, and in the proportions of their constituent principles. M. Klaproth says that acids have no action on the pirite of Saxony, that he found much difficulty in operating on it by potash, and that, in order to separate its parts, he was obliged to treat it twice with this alkali. The same chemist appears not to have found any water in this substance. This difference, it is true, may be explained, if it is considered that the pirite of Saxony contains more alumine, and that it adheres to the tongue, while that of France has not this property, probably on account of the water which it contains.

VII.

Experiments, shewing, contrary to the Assertions of Morichini, that the Enamel of Teeth does not contain Fluoric Acid. In a Letter from WM. BRANDE, *Esq.*

To Mr. NICHOLSON.

SIR,

Gay Lussac on fluoric acid in animal substances.

HAVING seen in one of the last numbers of the *Annales de Chimie*, an article entitled, " Lettre de Monsieur Gay-Lussac a Monsieur Berthollet, sur le presence de l'acide fluorique dans les substances animales," &c. I was surprised to find that a chemist at Rome, of the name of Morichini, had discovered fluoric

fluoric acid united to lime in the enamel of human teeth. The extraordinary results of these researches, induced me to repeat them; but before I mention the experiments from which I have drawn conclusions different from those of the above-mentioned chemist, it may perhaps be proper to quote that part of Gay-Lussac's letter which relates to the present subject: —and said by Morichini to exist in the enamel of teeth.

"M. Morichini having detached some of the enamel from human teeth, supposed that it might bear some resemblance in its composition to the enamel of the fossil teeth of an elephant, in which, on a former occasion, he had detached fluoric acid; he therefore subjected it to analysis, and perceived, to his great satisfaction, that it contained a large proportion of fluoric acid. Quotation to that effect.

To render these experiments more conclusive, he submitted portions of the two species of enamel, viz. that of the fossil, and human teeth, and likewise fluat of lime, to the action of sulphuric acid, and found that the last of these three substances yielded fluoric acid in the greatest abundance, and that the enamel of fossil teeth yielded somewhat more than that of human teeth; but Morichini remarks, that this difference is merely owing to the presence of animal matter in the two kinds of enamel, and that the disengagement of the acid from the fluat may be retarded, by adding a little gelatine to that substance, after it has been calcined, and then drying the compound. He moreover observes that the vapours which sulphuric acid disengaged from any of these three substances, had the property of acting on glass, of depositing a siliceous film on water, and other properties, which it is scarcely necessary to mention. Morichini says that the enamel of recent teeth afforded fluoric acid as well as that of fossil teeth; and that the sulphuric acid disengaged vapours that corrode glass, &c.

According to Morichini's experiments, one hundred parts of the enamel of human teeth contain 30 parts of animal substance, and 22 parts of fluat and phosphate of lime, with some magnesia, alumine, and carbonic acid. He has not yet been able to separate the fluoric and phosporic acids from each other, but thinks that the proportion of the latter must be extremely minute. M. Morichini has also observed that the enamel of the fossil teeth of the elephant differs from that of human teeth, in containing a smaller proportion of animal substance and phosphoric acid; but he thinks that the phosphoric acid which he found in the enamel of human teeth may have been derived Component parts of enamel of teeth according to Morichini.

from

from a portion of the bony part from which the enamel is separated with great difficulty. But the most interesting and unexpected result is, that fluoric acid exists in animal substances: a discovery of the greatest importance. These experiments oppose the present opinion concerning the composition of enamel, for Mr. Hatchett in his analysis of this substance has only detected phosphate of lime.

Morichini professes to have proved my facts by repeated experiments.

The result of Mr. Hatchett's experiments, together with those which were subsequently published by Mr. Josse, in the Annales de Chimie, Tom XLIII. rendered it necessary for M. Morichini to submit his opinion to accurate investigation, and after having made a numerous series of experiments on the subject, he observes, that he cannot entertain a doubt, that the enamel of human teeth consists chiefly of fluat of lime.

General remarks by Gay-Lussac.

After some observations on the composition of ivory, M. Gay-Lussac concludes this part of his letter, by observing that there is an immense field laid open in that part of chemistry which relates to animal substances, if it were merely to search for fluoric acid. Morichini has undertaken an investigation of the subject: but so much remains to be done, that the exertions of many chemists will be requisite."

The author's experiments shew the contrary.

I shall now relate some experiments, which will shew that fluoric acid does not exist in the enamel of human teeth, but that this substance consists chiefly of phosphate of lime, as originally stated by Mr. Hatchett.*

Enamel of human teeth was ignited, pulverized, and subjected to sulphuric acid. The fumes did not corrode glass.

One hundred grains of the enamel of human teeth, detached from what is usually termed the bony part, but which appears to consist of a substance of the nature of ivory, were kept for a few minutes in a red heat, and then pulverised. The enamel, thus reduced to powder, was put into a platina crucible, in which a piece of a glass rod was placed horizontally in such a manner as to be about an inch and a half above the enamel. Half an ounce of sulphuric acid was then added, and the crucible being covered with a clean plate of glass, the heat of a lamp was applied, and distillation carried on for half an hour. During the process, white suffocating fumes were extricated; but on removing the glass which closed the top of the crucible, neither this, nor the rod below it were in the least acted upon; which certainly would have happened, had

* Vide Phil. Transf. 1799, p. 328.

any

any fluoric acid been present. Finding this, therefore, to be the case, I proceeded as follows:—Fifty grains of the same enamel were introduced into a small glass retort, and a little sulphuric acid being added, distillation was carried on nearly to dryness, but in such a manner, that the gaseous products might be received over mercury. A small quantity of sulphuric acid gas was disengaged, and what remained in the retort, consisted, as far as I could ascertain, of a mixture of sulphate of lime, phosphoric acid, and a small portion of sulphur, arising from a decomposition of a small part of the sulphuric acid by the animal matter, existing in the enamel.

neither was there any extrication of fluoric acid by adding sulph. acid to enamel and distilling over mercury.

I have the honour to be,

Sir,

Your most obedient servant,

WILLIAM BRANDE.

Arlington Street,
Feb. 15, 1806.

VIII.

A Memoir on taking the Levels of the whole Surface of France. By P. S. GIRARD, *Chief Engineer of Bridges and Highways, &c.**

IF the surface of the earth were formed by the revolution of a curve round its axis, it would be sufficient, in order to determine the respective positions of different points upon it, to measure their distances from the intersection of that surface made by the plane of the equator and any particular or assumed meridian.

Thus geographers, considering the earth as perfectly spherical, have determined the position of any given place by the conjunction of two co-ordinates, one of which is the arc of the meridian, comprised between the place and the equator, and the other an arc of the circle parallel to the equator, comprised between the place and any assigned meridian.

The method by which geographers determine the position of a place.

* Journal des Mines, Vol. XVII. p. 297.

As

As these two co-ordinates intersect each other at right angles, it is apparent that the method of geographers, for determining the position of any place on the earth, is the same as that by which the position of a point on a plane is commonly determined.

is not exact, on account of the inequality of the earth's surface.

But this process, which would completely answer the views of geographers, if the terrestrial sphere were regular, ceases to be exact when the irregularities and protuberances are considered, with which the surface of this spheroid is covered.

This true position of a place is in a line perpendicular to that assigned by geographers.

The position of any place depends in reality, according to this hypothesis, on a third co-ordinate, which is supposed to be drawn perpendicular to the point of intersection of the two others.

This third co-ordinate ought to be taken vertically over the place of which the position is to be determined, and its measure reckoned from the place itself to its arrival at an imaginary surface, produced by the revolution of a known curve round the axis of the earth.

The level of the sea affords a spherical surface, from whence to measure those perpendiculars.

But it is known, that if our globe were surrounded by a fluid mass, all other force being supposed to be absent but that of terrestrial gravity, the surface of this fluid mass would be that of a spherical solid, of which the mean surface of the sea, in its actual state, represents a part: It appears then convenient to choose, for the third co-ordinate here mentioned, that portion of a vertical line passing through any place, which is comprised between that place and the mean surface of the sea, supposed to penetrate the globe and to be extended beneath the continent.

This is the best, though not the only method:

We have said that the choice of this line would be convenient; because, in reality, the position of a point on the terrestrial surface may be determined by adopting any other system of co-ordinates; for example, by fixing the position of this point, by three planes mutually intersecting at right angles;

But it is the most simple, and is besides generally adopted.

but, besides the advantage of greater simplicity in the expression of the circular co-ordinates, they have moreover, that of being generally adopted; for the geographical charts, hitherto prepared, may be considered as the projection of the continents and islands on the mean surface of the sea; so that there only remains, in order to render geography perfect, to add to the latitude and longitude of all the places on the earth, the vertical

The true position of a place determined by annexing its

vertical height which they are elevated above the surface of the ocean.

The object of this memoir is to indicate the means of determining this vertical heighth, by their particular application to the territory of France.

It is evident, that all the operations necessary for this determination, may be reduced to a series of levels made in determinate directions.

Nature itself has pointed out these directions, by the lines of *greatest declivity*, which the large rivers, and those which flow into them, form on the surface of the earth.

Thus, France being divided into five principal basons, by the Rhine, the Seine, the Loire, the Gironde, and the Rhone,—the levels of the course of these rivers, from their sources, or from their entrance into France, to their terminations in the ocean, would form the first basis of the work proposed to be undertaken.

After having ascertained this first basis of the general operation, the levels of the streams by which the great rivers are supplied, should be next taken, and these streams should be considered without any regard to those of the third order, by which they are themselves maintained.

At the same time, the levels of the rivers of the second rank, which fall into the two seas, should be taken; such as the Escaut, the Somme, the Orne, the Vilaine, the Charente, the Adour, the Herault, &c.

The declivities of the beds of the secondary rivers being known, those of the rivers of the third, fourth, and fifth orders, &c. should be determined successively, according to special instructions which should be given for this purpose.

By thus classing the operations relative to the general levels of France, and by arranging their results in order, as they were obtained, all the data would be soon collected, which were necessary for tracing the elevation of its territory on a geographical chart already prepared.

This tracing of the elevations would be effected, by joining all the points on one level by the same line.

These *lines of levels* might be supposed to be elevated perpendicularly, one above the other, by a determinate space, conformable to the scale of the chart on which they were traced.

Vertical elevation above the sea to its latitude and longitude.

Management proper for ascertaining these positions shewn in the instance of France.

The levels of the five great rivers of France would form the basis of this operation for that country.

The levels of the streams which supply those great rivers should be next taken.

The levels of the rivers of the second rank taken at the same time as those of the first.

The levels of rivers of the third, fourth, and fifth order taken.

Elevation of the surface of France to be expressed in the rough, by joining all the points on one level by the same line.

5 It

It is evident, that thefe lines would reprefent the borders of the coafts of the fea, if it was fuppofed that its mean level fhould be elevated fucceffively to the fame heighths which they reprefented.

M. Triel prepared the fketch of a map on this plan.

It was according to this idea that M. Dupain Triel prepared a phyfical chart, mentioned by M. Lacroix, member of the National Inftitute, in his introduction to Pinkerton's Geography; a chart which, from the defect of materials neceffary for its conftruction, prefented only the fketch of a work, the extent of which would require for its perfection an union of means, which could not be at the difpofal of any particular individual.*

The order has been pointed out in which this work ought to be executed, and we fhall now examine how it fhould be performed.

The courfes of the great rivers fhould be divided into portions, and the levels of each taken by obfervers at the fame time.

The bed of each of the great rivers muft be divided into a certain number of portions, and each portion fhould be levelled by obfervers, who fhould operate at the fame time.

Thefe obfervers fhould place accounts of their operations at each extremity of the portions of the bafons with which they were charged; and as the levels of the fecondary rivers fhould be connected with thofe of the principal rivers, it would be neceffary alfo to place accounts of the operations at the mouth of each of the influent ftreams.

The levels fhould be taken on the banks of the rivers, without any regard to the furface of the water. If it were thought ufeful to determine the declivity of this furface, it would be eafy to afcertain it, by levels taken at the fame time with the others, at certain diftances from each other.

The refults to be collected into a general fyftem:

When the different obfervers have completed their refpective obfervations, the refults muft be collected, to form the feries of levels of one of the beds. And in the fame manner the levels of all the reft fhould be obtained.

After this, a general fyftem fhould be formed from thofe particular levels, by connecting together the different beds, by operations directed from one to the other, according to thofe lines which would afford the greateft facility.

* Compare this with Mr. Churchill's plan, at p. 224 of our XIth. Vol.—T.

There

There only remains to determine to what agents Government should entrust the performance of the general levelling of France, in order to have it executed with the greatest exactness, speed, and economy.

The engineers of bridges and highways, already placed in the different departments, where this operation should be performed, are evidently the only persons to whom it could be confided, so as to fulfil those three conditions.

The engineers of bridges and highways would be the most proper persons to employ in this work.

In fact, the execution of all projects relative to the establishment of communications by land or by water, require, that the elevation in relief of the country, through which the works should be carried, should be known. The theory and practice of levelling form an essential part of the instruction given to the engineers of bridges and highways; and greater reliance may be placed on the exactness of the results which they might furnish, because the use of the instruments necessary to this operation, is more familiar to them.

On the other hand, there are none of those engineers who could not dedicate some days of the summer to taking the levels of that portion of such great rivers, or streams, as shall traverse his district; and as it is easy to take the levels of four or five kilometres (about three English miles) each day, especially when the line to be levelled is previously determined by the direction of the river or current of water, it is certain, that the engineers of the bridges and highways might collect, in a very short period, very minutely detailed materials for a physical chart of France.

Lastly, these materials would be collected by them with the least possible expence, because Government would neither have to support the cost of extraordinary journies, nor the purchase of instruments, as the engineers are already, by the very nature of their employments, dispersed over the several districts where it would be necessary to operate, and are, at the same time, provided with the different instruments required for this purpose.

They could perform it without expence to the nation.

It may also be added, that the taking the general levels of France appears to be, with the more propriety, a work that should be performed by the engineers of bridges and highways, as they would be the first to profit by this operation in putting their projects into execution.

It would tend much to their own benefit to have it effected.

Suppose then that the engineers of bridges and highways were charged with the performance of this work, let us consider how, after some years, the exactness of the results, which they had collected, could be sufficiently ascertained.

Let us take, for example, the bed of the Loire, whose course is of great extent.

The chief engineers of the departments of the Upper Loire, of the Loire, of the Saone and Loire, of the Nievre, of the Loiret, of the Loire and Cher, of the Indre and Loire, of the Mayenne and Loire, and of the Lower Loire, would be ordered to furnish, during the year, the levels of that part of the course of the Loire which traversed their respective departments.

According to the new organization of the service of bridges and highways, these nine departments require twenty-two engineers, in the district of each of whom would be found a portion of the work to be performed.

The total extent of the Loire is about ninety muriameters (about 550 miles), which being divided among twenty-two observers, would give to each of them little more than forty kilometers (about twenty-five miles) of levels to execute.

Levels of the Loire might be taken by the 22 engineers in that district, in one season.

There is reason to believe, from experience, that the twenty-two engineers employed on the course of the river, would finish, in less than one season, the levels of the whole river.

The same thing may be affirmed of the engineers placed in the departments traversed by the Rhine, the Seine, the Gironde, and the Rhone. It appears then beyond a doubt, that, at the end of the first year, the chief part of this physical chart could be completed, to which the farther details might be afterwards added.

Whatever care may be bestowed in taking levels, their verification is always an useful operation. That of the general levels of France might be made as often, and in whatever circumstances it should be judged necessary. It would be sufficient for this purpose, to direct the newly appointed engineers to repeat, in the departments to which they might be sent, the observations of their predecessors; which, besides the advantage of confirming or correcting the results already obtained, would give an opportunity to the new engineers of acquiring, in person, a knowledge of the elevation of their respective districts in relief.

Levels of France, when taken, might be afterwards verified by the newly appointed engineers.

The

The facility and promptitude with which the engineers of bridges and highways might execute this work, will be apparent, if it be recollected that, at the time when the major part of the great roads in France were formed, and when a general system of internal communications was desired to be established, M. de Trudaine, assisted by M. Perronnet, caused plans to be taken of all the principal roads, from their commencement to the frontiers. There was joined to the plan of the road properly so called, that of the country bordering on it, to the distance of three or four hundred yards at each side; a work which evidently required more time than simply taking the levels of a determined line, such as we propose; and yet the engineers of bridges and highways, or their pupils, employed in taking those itinerary plans, completed from five to six leagues of them each month. Facility of this work.

The general utility of the operation, of which a sketch is here given, will sooner or later determine some of the nations of Europe to undertake it. France, on whose territory has been lately executed some of the finest geological operations which were ever performed, and where, for the first time, a system of universal measure has been established on an invariable basis, seems to be particularly called on, to give, on this occasion, the first example of a work, which, by completing the natural geography of countries, will furnish new facts to geology, and to those different parts of natural history which depend on it. Geological operations already performed in France, urged as a motive to commence this work; which would complete the natural geography of countries.

IX.

Observations and Experiments on the Composition of Water, and other Elementary Doctrines. By H. B. K.

To Mr. NICHOLSON.

SIR,

As two papers have appeared in your Journal, both of which militate against the result of my experiment, and as Mr. Accum has been concerned in one of them, I therefore think it incumbent on me to answer them.

 I was

The gases obtained from water by galvanism, smelled of nitrous gas after explosion, and gave nitre with potash.

I was glad to see my experiments in your Journal, as it has so extensive a circulation. I shall now give you the analytical part to confirm my former experiments; as by them I had, I hope, given strong evidence, that acids are necessary in forming gases. Having collected a great quantity of the gases produced by the galvanic pile, I introduced them into a strong glass tube, closed at one end, the other end I afterwards closed, having previously introduced to the gases a small quantity of a solution of potash: through this tube the electric spark was made to pass, it having small openings to admit the wires of communication. Upon their combustion, the smell of the nitrous acid vapour appeared, both from its colour and smell; and the tube being moved up and down, so as to allow the vapour and the solution of potash to come in contact; the solution being examined some short time after, it gave evident and unequivocal signs of the nitrate of potash.

On Pacchiani and Riffant's experiments.

I see in your Journal, Mr. Riffant's experiments, in answer to Mr. Pacchiani's paper. Indeed, in reading Mr. P.'s experiments, nothing could appear more vague and wild than that water, by having oxigen, the supposed acidifying principle, taken from it, should become a strong mineral acid. Mr. Riffant's second experiment directly contradicts my experiment, on the supposition that water is a compound body; but if examined upon my supposition, that the acids are necessary in forming the gases, and that the water is only necessary in forming the water of composition, I hope I shall be able to prove that his experiment confirms my opinion. There were very little of gases formed by this experiment, and the wires were very much calcined: now this calcination was from the acid, or acids, I proved by repeating the very same experiments; but only instead of distilled water, I used a solution of potash, and instead of the wires being calcined, they were not sensibly acted upon, and the potash became nitrated.—Now, Mr. Nicholson, I (seriously and ardently) call upon your numerous readers to perform this experiment, which I think must be decisive.

Against the doctrine of the composition of water: it is urged that the gases obtained in galvanism vary from different causes.

I can but smile at the French chemists, in making the proportion of the gases so exactly to tally with their opinion of the composition of water; but I have in my experiments found very different results; the kind of gases depending a good deal upon the wires used, the different metals, their length, and the

the different liquors between the plates of the pile; all of which had a fenfible effect upon the gafes, both upon the quantity produced, and their kind; the calcinable wires when long producing the moft inflammable kinds, and the lefs calcinable metals the more of the oxigen kind, and the longer the wires the more in volume were the gafes.

Mr. Northmore, in your Journal, endeavours to prove the formation of the nitric acid from the compreffion of gafes.—Upon inveftigation, his experiments will, I think, be found very vague and inconclufive: that gafes from active compreffion will produce both heat and water, has been long known. The firft experiment was in condenfing hidrogen, oxigen and nitrogen gafes, two pints of each. He fays they produced "white floating vapours, probably the gafeous oxide;" but in experiment the feventh, he obferves, "the hydrogen produced white clouds at firft, *quære* ammonia." So without any chemical examination of thefe white clouds, they are at firft fuppofed to be the gafeous oxide, and afterwards ammonia, juft according as his theory dictates to him. In the fifth experiment, he fays, "and the refult was only a fmell of gafeous oxide of nitrogen, a few yellowifh fumes." Here then the gafeous oxide produces a yellowifh colour, though in the firft experiment it was a white colour.

Remarks on Mr. Northmore's experiments.

The acid produced was, from the fame vague opinion, fuppofed to be the nitric; but this he endeavours to examine in the next experiment; firft by a good teft, in expofing it to lime water; and he fays, "Some yellow particles were feen floating upon the lime water; thefe particles probably arofe from the refinous fubftance ufed in faftening on the cap of the receiver being diffolved by the nitrous gas formed during condenfation. Here then was the lime water affected. I fay with confidence, thefe flocculi in the lime water were from the carbonic acid produced, and why they appeared yellow was from their being feen through the gafes, being clouded with an orange colour, which, as he obferves, they put on when they were condenfed.

That acids are neceffary in forming oxigen gas, I hope appears very clear from my experiments; therefore when it forms combuftion with inflammable bodies, it is rational to fuppofe that an acid will appear upon its decompofition. If the combuftion is active, as in the French experiments in condenfing oxigen

Remarks on Mr. Northmore's experiments.

oxigen and hidrogen gases, the heat produced is so active as to make an explosion of the gases; but if a slow combustion, it will leave the oxigenized acid in a gaseous state, as carbonic acid gas, which, I suppose, was the case in Mr. Northmore's experiments. His next experiment of examining the acid: He compressed the gases upon two scruples of the solution of potash; he says, "there was scarce enough acidity to tinge the edge of the test paper; of course, I could not effect the formation of the nitrate of potash." But always to assign some reason for the failure, he says, "This quantity (of gases) was hardly sufficient for the receiver's capacity;" but there was the same quantity in this experiment as in the others; nay, in the next experiment (the sixth) there was identically the *same quantity and in the same proportions*; and in this fifth experiment, he found so little acid, as he says, "Scarce enough acidity to tinge the edge of the test paper; of course I could not effect the formation of the nitrate of potash." Now upon the supposition that the carbonic acid was formed, it would unite with the potash, and therefore the mixture would be less saturated with it: But if the acid was so strong as he speaks of in the sixth experiment, from the very same process, as he says, "Which moisture was strongly acid to the taste, coloured litmus, and when very much diluted with water, acted upon silver." Now if Mr. Northmore will consult the writings of chemists, (Dr. Black's lectures, for instance); in Vol. II. the doctor says, "that the nitric acid requires a little water to reduce it to the strength of aqua fortis; in order to act upon silver, therefore, in this experiment, the acid must have been in the concentrated state of the nitric acid, as it required water to be diluted to make it act upon silver; but probably Mr. N. does not know that water impregnated with hidrogen gas will colour silver; which I suppose to have been the case here.

This reasoning must appear to be most extraordinary: this vast quantity of nitric acid produced was even to penetrate into the cap of the receiver; but very unfortunately for this supposition, chemists are of opinion that acids will not dissolve resins. Mr. Hatchett has promoted their operation upon each other by using the strong nitric acid; but this was a difficult and tedious process, not during the transitory action of a little time, by compression; and where the resin was so concealed;

cealed; fo that the acid could not get to act upon it, being placed within the cap of the receiver; therefore the fmall quantity of weak acid formed in Mr. N.'s experiment could not rationally be fuppofed to have penetrated to it, even if it was in a high concentrated ftate; but it muft have been much diluted with water, as there was water alfo produced in this experiment: Alfo, if it was in this high concentrated ftate, and in that abundance as to enter into all the crevices, it would eafily have been detected, and his fifth experiment was for this purpofe, but it failed; he could find no nitrous acid.

Remarks on Mr. Northmore's experiments.

In experiment the feventh, he fuppofed he had formed ammonia, and he fays in this very experiment, "Some vapour was generated, which was, as ufual, ftrongly acid." How comes it that this acid, which was fuppofed to find out the refin, fo perfectly concealed, could not find out the ammonia, which was formed along with it in the procefs, and fo univerfally difperfed as to form white clouds.

The third experiment: "Two pints of carbonic acid and two of hydrogen was fubjected to condenfation. The refult was a watery vapour, and a gas of rather offenfive fmell." This compreffed gas I found to be fimilar to Mr. Cruikfhanks's gafeous oxide of carbon from the acid air and the phlogiftic air faturating each other.

Mr. Northmore apologifes for giving thefe experiments "until he had brought them to a greater degree of perfection," but at the conclufion he alfo fays, "Befides the above, I have made various other experiments with different gafes, &c." But as he fays nothing more of thefe *imperfect experiments*, there are no hopes of his correcting them; he appears to have exhaufted his refearch, and we have feen with what fuccefs. There appears fuch an ardent defire to fupport the Lavoifierian theory: but if it has always failed from the experiments of Lavoifier himfelf, Mr. Cavendifh and others, I am afraid we have little to expect from thefe new fupporters. I might make many other obfervations, but thefe will, I prefume, be thought enough.

It will be expected in contradicting Mr. N.'s experiments I fhould make fome of my own; I muft own my apparatus was not fo good as his; yet I hope fufficient to prove my opinions. I had the barrel of a large blunderbufs, and ftopped its priming hole, and having filled it either with fand or diftilled

Experiments of compreffion of the gafes.

distilled water, I then tied to its mouth a bladder filled with the different gases I wanted to compress. Upon pouring out the sand or water into the bladder, the gases entered the barrel, and then having a strong iron ram-rod made perfectly air-tight, it was forced down upon the gases by a long iron lever, by which means I was capable of making a stronger condensation than reducing them to one fifth of the volume.

They gave out heat and moisture.

The results of these experiments were, that all the different gases, by being compressed, gave out heat and moisture: The hydrogen gas, the greatest proportion of moisture to its specific gravity.

Oxigen and hydrogen gave acid.

That when oxigen and hydrogen gases were compressed, there was an acid which produced flocculi in lime water; and that nitrogen gas was not necessary to the production of the acid, but rather retarded its production. The nitrogen gas obtained by the nitric acid and animal substances ought not to be used in these experiments, as it is partly acid of itself; but the nitrogen of the atmosphere ought to be made use of, being previously passed through lime water.

Mr. Nicholson, I have condensed this communication as much as possible, in order that it might not occupy too much room in your Journal. I am

Your's, &c.

H. B. K.

London, February, 15, 1806.

X.

On the Construction of the Sails of Ships and Vessels. By MALCOLM COWAN, *Esq. Captain in the Royal Navy.* *

The sails of ships have been long without improvement.

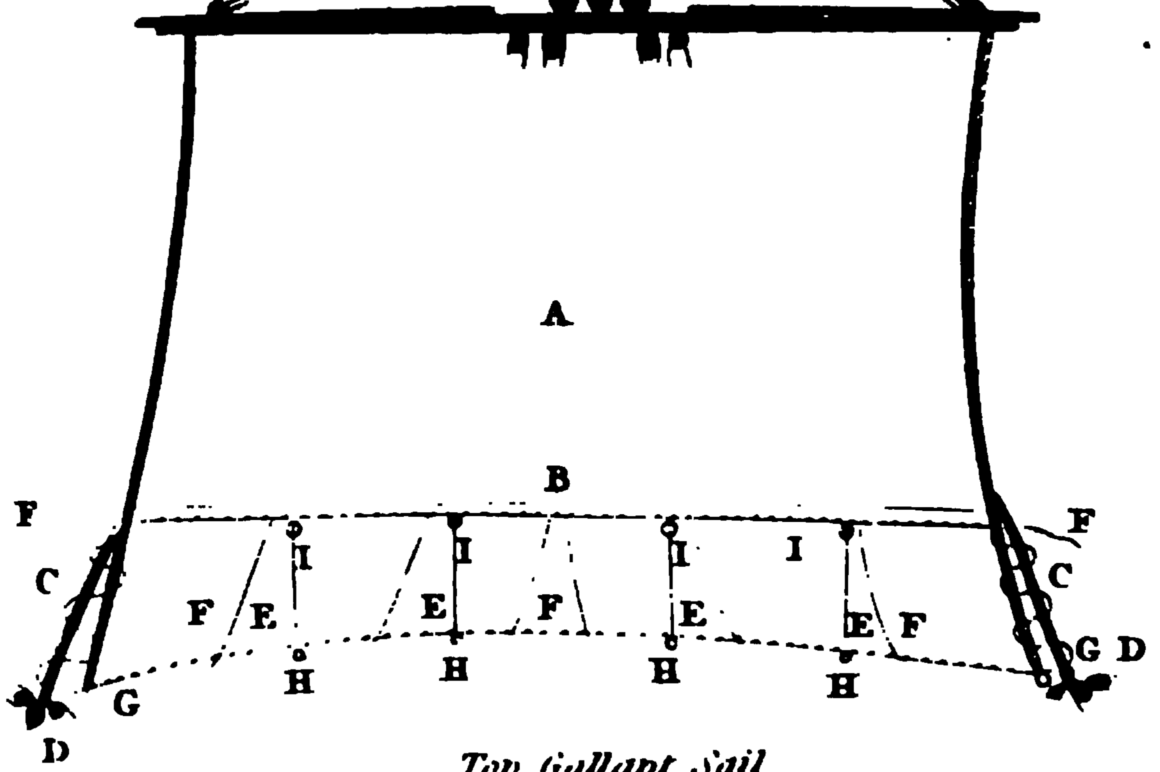
IT appears from the construction of the sails of ships and vessels, belonging to every nation, that it is a subject no one has hitherto taken much pains to investigate; but the maritime world have been content to use them, as they found them, every one following the beaten track of his predecessors, without examination.

* Extracted from an essay, by the author, who has letters patent for the sails.

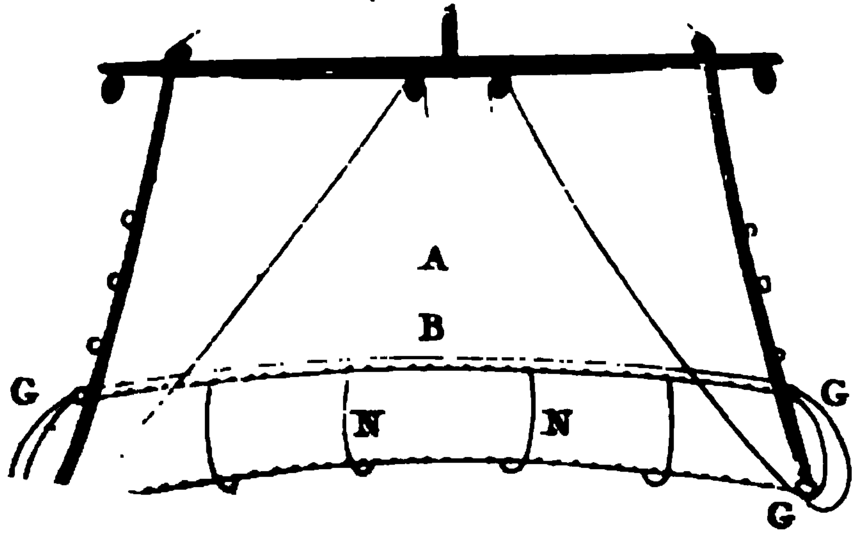
That

Improved Sails by Capt. Cowan.

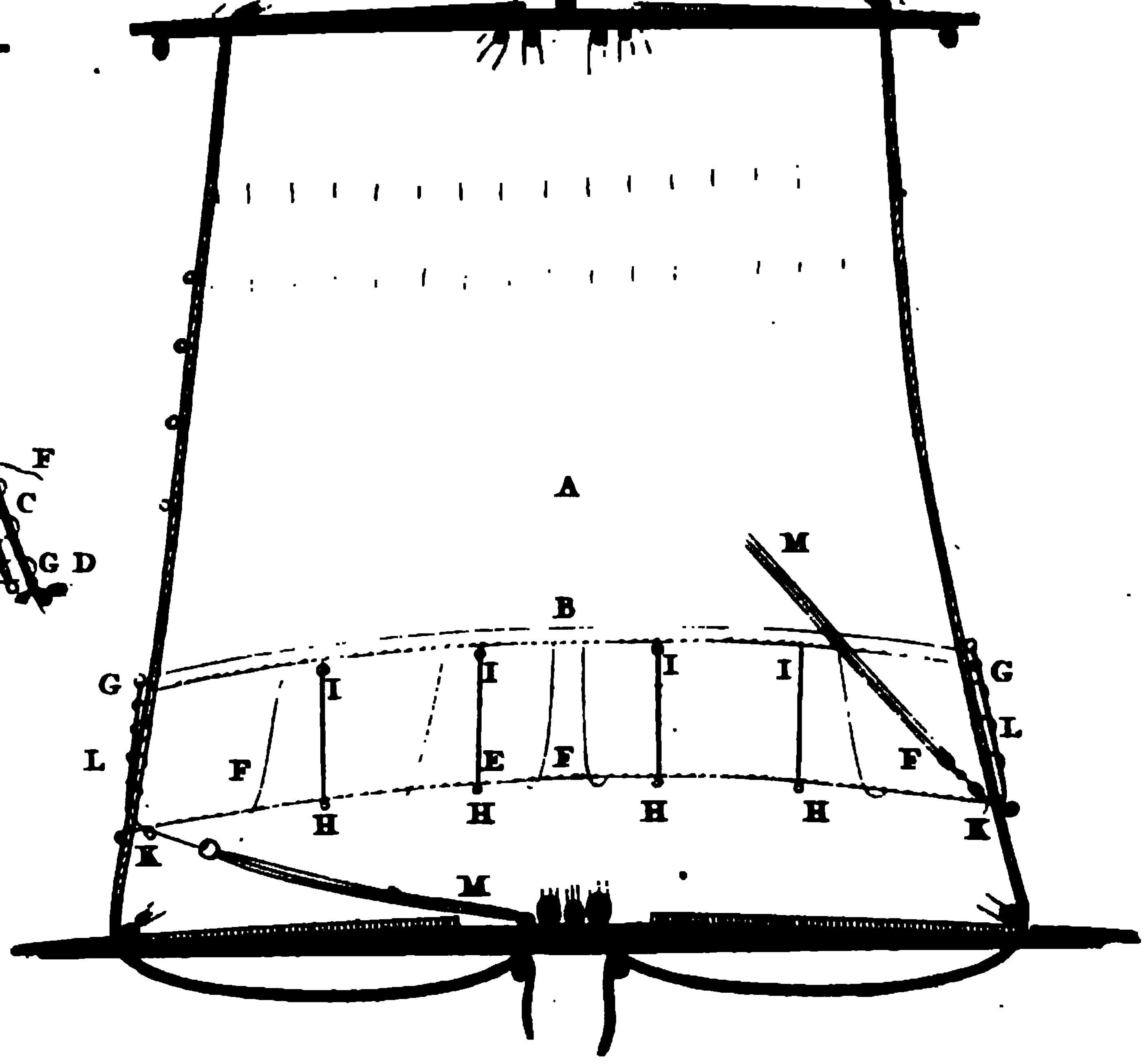
A Main Course.



Top Gallant Sail.



Top Sail.



That the sails of ships have been hitherto so constructed by all European nations, so as to be only managed with great labour and danger; and that when managed with the greatest skill, they are very far from being of that utility which they ought to possess, and are capable of having, is incontestible.

Ships are driven on shore every winter, which might, with proper sails, have escaped every danger. The loss of one sail, in many situations, is followed by the inevitable loss of the ship and crew. Sails are often split in hauling up to reef, and it may be necessary to reef a sail that is worn, to preserve it from splitting; hence the necessity of the sails being constructed to reef without starting tack or sheet.

They are worked with difficulty and danger.

Many ships have been lost by not having time, or drift, to haul their courses up, to reef them on the yard, by which they risk their splitting; a circumstance which alone must convince the seamen of the utility of having sails that can be reefed without taking their effect off the ship.

Many dangers may be avoided, by carrying sail with safety to the masts and yards. A ship can carry top gallant sails that reef at the foot, with safety, when other ships must furl theirs; an evident advantage in many situations.

The top sails of ships, with one or two reefs at the foot, can be reefed in a minute by one seaman at each lower yard arm, while they remain set with the top gallant sails over them, by only settling the hallyards; by which a ship in squally weather, on many occasions, would have a great advantage, particularly in chace, &c. or when caught by a sudden shift of wind on a lee shore, or obliged to haul suddenly to the wind from sailing large.

Advantages of sails which are reefed at the foot instead of the head.

The facility with which sails that reef at the foot, can at all times be managed, would enable ships to make quicker voyages, and prevent them often, when weakly manned, from detaining fleets; by the difficulty and danger of carrying sail, being entirely removed, must enable merchant ships to be navigated with fewer hands, which would be a considerable saving of expence, and a great advantage in time of war in particular, when men are so scarce.

If the sails were made with horizontal cloths and seams, the sails would stand better, particularly in a gale of wind; as the strongest direction of the cloths and seams would be opposed to the greatest force of the wind, which acts horizontally;

The seams ought to be horizontal.

tally; and should the sail split in that direction, it would still remain full, and be less liable to blow away altogether, which is generally the case when a sail splits in a vertical direction. Storm stay sails set purposely with the cloths horizontal, have proved this beyond a doubt.

Many seamen are lost every winter, by falling overboard from the yards while reefing the sails, as it is more dangerous and requires longer time to perform in a gale of wind, than furling the sails, which is not so often necessary as reefing.

Other advantages from improvements in sails, &c.

Ships may sometimes avoid a lee shore, by carrying a timely press of sail, and when in that perilous situation, in a gale of wind, the safety of the ship may solely depend on the sails being kept set; though it may be necessary to reduce them, either to save them, or ease the ship. The common sails require to be hauled up, to be reefed, at the risk of splitting them, at a time perhaps, when the ship is in imminent danger, from the want of sea room; and the best seamen of the crew must be sent on the yards when they possibly may be much wanted on deck.

Whole fleets are often caught by a sudden shift of wind, of a lee shore, thrown into confusion, and obliged immediately to reef their sails, at the same time the ships may require the whole of their crews on deck, to attend the working of the ship, to keep clear of each other; particularly when it happens in the night time, with the wind squally and variable.

When ships from foreign voyages, enter the English or Irish channels, in the winter time, when the days are short, and the nights long, with weak or disabled crews, or men not accustomed to cold or frost, such as Lascars, Negroes, &c. it is with the greatest difficulty they can be prevailed on to go aloft; but should they get on a lee shore, which all ships are liable to, and with a helpless crew, nothing can exceed the horror of their situation, should they not be able to proportion their sail to the wind in time to save the ship.

Naval improvements are of great importance to the state.

To facilitate the working of ships, by the most approved means, is an object of greater consequence to a maritime nation than many are aware of, even in a commercial point of view. The little alteration that has been made in shipping for many years past, shews with what indifference attempts at improvements have been regarded, many of which have been

been tried, proved, and neglected, while others have failed from the unavoidable expence, necessarily attending all experiments on a large scale, which require repetitions to bring to perfection; or from partial interests or prejudices, being opposed with success (which not unfrequently happens) to improvements of general advantage. And many are apt to suppose that particular arts and sciences are brought to the highest degree of perfection they are capable of, though experience every day convinces us to the contrary.

The largest ships might be much more easily navigated, if the improvements on capsterns, windlasses, blocks, hawse-holes, &c. were universally adopted from the great reduction of the friction.

The following explanation will be easily understood by those who are acquainted with the construction of a ship, See Plate VI.

The courses and top gallant sails are to be reefed from the deck, and the top sails by one man at each lower yard arm.

A. The after-part of the sails. Description of the improved sails.

B. A strong band on the after-part of the sails, sewed on at the upper part only, and roped at the lower part.

C. The long clews of the course, formed by the bight of the leech rope and rope of the reef band with thimbles, seized in above the tack blocks, for lashing the lower clews to.

D. The tacks and sheets fitted to the upper clews of the courses with thimbles above the tack blocks.

E. The buntlines, brought up through the thimbles H, on the foot ropes of the sails, and bent to the cringles I, on the ropes of the reef bands.

F. A small rope or gasket, rove, occasionally as a reef line, through eyelet holes, under the reef bands, and made fast to the middle sail, for confining the sail when reefed, in the wake of the reef bands.

G. Thimbles in the clews and earings.

K. Thimbles on the foot rope with the earings rove through them.

L. The reef tackle pendants, passing through thimbles in the clews and leech of the top-sail, and brought up and bent to the cringles above the upper reef band.

M. A boom tackle or burton hooked to the reef pendants.

N. The crow-foot legs to the top gallant buntline.

N. B. The reef bands are fewed by the upper part, to the after part of the fails, to prevent the rope from girting the fails, when the whole fail is fet.

The rope of the reef band of the courfe, is the fame fize as the common foot rope, and the foot rope muft be in proportion to the rate of the fhip: for the firft rates, 3½, or 4 inch; fecond rates 3½; third rates, 3 inch rope: as the quantity of fail below the reef band does not require fo ftrong a foot rope, as when the whole fail depended on it.

The rope of the reef bands of the top fails, fhould be fmaller than the leech ropes, as the foot of the fail will be confiderably ftrengthened, when reefed.

Thefe fails are not fo heavy as the common ones; a 74 gun fhip's courfe is reduced in weight about 200lbs. as the points, bands, and eyelet holes of the old reefs are not required, nor any additional geer.

Men of war will find one reef at the foot of the top fails, very ufeful in chace in fqually weather, or when obliged to haul fuddenly on a wind, &c.

Merchant fhips will only require two reefs in the top fails, as the fquareft part of the fail is taken off, by reefing at the foot inftead of the head, but more reefs may be added if neceffary.

Inftructions for reefing and fetting the fails.

When the courfes are to be reefed, caft off the lower clews, from the thimbles in the upper clews, haul up the flack fail by the buntlines. and haul tort the reef line, one part at a time, from the middle of the fail, towards the clews, and make it faft round the upper clews, fo as to confine the lower clews.

To fet the fail, reeve a few turns of the lafhing for the clews, and haul them down, overhauling the reef line, and buntlines.

To reef the top fails, fend a man up to each lower yard arm, fettle the hallyards, and haul the fail down by the reef tackles, and pafs the turns of the earings, through the thimbles in the earing cringles, and on the foot of the rope, and make them faft. Hoift the fail tort up, haul through the flack of the buntlines, and haul tort the reef lines on each fide towards the clews, and make faft,

The top gallant fails are reefed from the deck, by the clew lines, and a fingle buntline with a crow-foot.

The

The buntlines and reef line will confine the flack fail, when reefed, clofe up the wake of the reef bands; and the buntlines will only require to be kept hand tort, as is ufual, to prevent them from chafing the fail.

The flack fail of the roof of the top fail, will be kept extended tort acrofs the foot, by the reef pendants paffing through cringles in the leech.

The ends of the clewlines may likewife pafs through cringles, in the leech of the top gallant fails if neceffary.

The reef lines, if neceffary, may be in feparate pieces, made faft in the middle and quarters of the fail.

XI.

Experiments on condenfed Gafes. By T. NORTHMORE.

To Mr. NICHOLSON,

SIR,

I NOW take the liberty of prefenting you with a continuation of my experiments upon the condenfation of the gafes, but firft beg leave to make one obfervation, viz. that the quantity of gas faid to be injected in each experiment, cannot (particularly in the preceding article) always be depended upon; for its tendency to efcape is fo conftant and powerful, as frequently to elude every effort of mine to prevent it, and if it can find no other exit, it will fometimes efcape by the fide of the pifton of the forcing pump. In the preceding experiments I have endeavoured as much as poffible to obviate this evil, but not always with the fuccefs that I could wifh. Experiments on condenfed gafes.

Repeating the eighth experiment mentioned in my former letter, (fee Vol. XII. p. 372-3) viz. the condenfation of nitrogen upon lime *, in order to difcover the caufe of the lofs of colour in the nitrogen, I perceived that this arofe from its fixation, and a nitrate of lime was the refult. This experiment, on account of the elafticity of nitrogen previous to its change of habitude, requires fome caution; for one of my beft receivers, three-eighths of an inch thick, was Nitrogen condenfed upon lime, produced nitrate.

* Your marginal note fays erroneoufly *lime-water*.

fhivered

shivered in pieces with a violent explosion, after I had set it aside to see the effect of time upon the compressed gas.

Nitrogen and gaseous oxide of carbon condensed, gave nitrous acid, &c.

Experiment 9. Upwards of a pint of nitrogen was condensed, and upon this I pumped one pint of gaseous oxide of carbon. The colour of the nitrogen was destroyed; nitrous acid was formed; and upon collecting the liberated gaseous oxide, it burnt not unlike alcohol. The two gases together were at first highly elastic.

Explosions attributed to nitrogen.

From the facility with which nitrogen becomes united and fixed in various bodies, and from its expansive force when liberated from that state, I know not whether I am sufficiently warranted in suggesting an opinion, that the explosive force of various compounds may in a great measure be attributed to the sudden liberation of this fixed gas. To this cause I partly attribute the fulminating silver of Berthollet; the fulminating gold, and various nitrates; and the detonation which accompanies the decomposition of ammoniac by oxigenated muriatic acid gas.

Attempt to fire phosphorus by condensed air.

Exp. 10. Having been unsuccessful in my endeavours to inflame phosphorus by the compression of atmospheric air, (see *Exp.* 4.) I now tried oxigen, but with little better effect. The phosphorus appeared to be somewhat discoloured, and I thought had a tendency to liquify, as it does when put upon a heated plate of iron. Indeed I have no doubt that some heat is generated by the condensation of air, since the thermometer rises upon external application to the receiver.

Oxigenated muriatic acid gas gave a yellow and highly volatile fluid by condensation.

Exp. 11. Upon the compression of nearly two pints of oxigenated muriatic acid gas in a receiver two and a quarter cubic inches capacity, it speedily became converted into a yellow *fluid*, of such extreme volatility under the common pressure of the atmosphere, that it instantly evaporates upon opening the screw of the receiver. I need not add, that this fluid, so highly concentrated, is of a most insupportable pungency. When atmospheric air was pumped into the empty receiver, it was speedily filled with dense white fumes. There was a trifling residue of a yellowish substance left after the evaporation, which probably arose from a small portion of the oil and grease used in the machine, mixed with some of the concentrated gas; it yielded to sulphuric ether, and destroyed vegetable colours.

This

This gas is very injurious to the machine, and on that account difficult to work.

Exp. 12. Upon half a pint of oxigen was injected one pint of oxigenated muriatic acid gas. The result was a thicker substance which did not so soon evaporate, and a yellowish mass was left behind. Oxigenated muriatic acid and oxigen afforded a thicker fluid.

Exp. 13. Upon half a pint of nitrogen was injected one pint of oxy-muriatic gas. The result was a still thicker substance, and the yellow colour deeper, nor did it appear to act so powerfully upon vegetable colours. Much of the grease of the machine was carried down in both these last experiments, which formed part of the yellow residue, and yielded only to ether. Oxigenated muriatic gas and nitrogen.

Exp. 14. Having condensed about a pint of carbonic acid, the receiver very unexpectedly burst with violence. This circumstance I attribute to the vicinity of the furnace, and I mention it to guard others against standing too near a fire in these experiments; nor perhaps may it be useless to add another precaution, that of using goggles, or at least a thick plate of glass when examining the results. Receiver burst, caution.

I now took a new receiver of three cubic inches of capacity, and pumped in one pint of carbonic acid, and upon this rather more than a pint of oxigenated muriatic acid gas. Carbonic acid, and oxigenated muriatic acid.

The union produced a light sap-green colour, but no fluid, though as usual the oil of the machine had retained enough efficacy to destroy vegetable colours.

Exp. 15. Upon rather more than a pint of hidrogen, which was highly elastic, were compressed two pints of the oxigenated muriatic gas. The result was a light yellow-green colour, and no fluid. Some smoke or vapour seemed to issue out of the receiver upon turning the screw, and the gas was highly destructive of colouring matter. Oxigenated muriatic acid gas on hidrogen.

Exp. 16. I now proceeded to the muriatic acid gas, and upon the condensation of a small quantity of it, a beautiful green coloured substance adhered to the side of the receiver, which had all the qualities of muriatic acid; but upon a large quantity, four pints, being condensed, the result was a yellowish-green glutinous substance, which does not evaporate, but is instantly absorbed by a few drops of water; it is of a highly pungent quality, being the essence of muriatic acid. As this gas easily becomes fluid, there is little or no elasticity Muriatic acid gas easily made fluid by condensation.

elasticity, so that any quantity may be condensed without danger. My method of collecting this, and other gases which are absorbable by water, is by means of an exhausted florence flask (and in some cases an empty bladder) connected by a stop-cock with the extremity of the retort.

An idea here occurs to me, that the facility of fixation which is the property of the compressed muriatic, oxy-muriatic, and some other gases, may be made of some utility to the arts, since by previously pouring in a little water, or other fluid into the receiver, an acid may be obtained of almost any degree of concentration.

Sulphureous acid gas condensed by pressure.

Exp. 17. Having collected about a pint and a half of sulphureous acid gas, I proceeded to condense it in the three cubic inch receiver, but after a very few pumps the forcing piston became immoveable, being completely choked by the operation of the gas. A sufficient quantity however had been compressed to form vapour, and a thick slimy fluid of a dark yellow colour began to trickle down the sides of the receiver, which immediately evaporated with the most suffocating odour upon the removal of the pressure. This experiment corroborates the affirmation of Monge and Clouet, mentioned in Accum's chemistry, vol. I. p. 319. viz, that "by extreme artificial cold, and a strong pressure exerted at the same time, they rendered sulphureous acid gas fluid. From the injury which this gas does to the machine, it will be very difficult to perform any experiments upon its elective attractions with the other gases.

I remain, Sir,

Your obedient humble Servant,

T. NORTHMORE.

Devonshire Street, Portland Place,
Feb. 15, 1806.

On

XII.

On the Probability that Muriatic Acid is composed of Oxigen and Hidrogen. In a Letter from Mr. J. MARTIN.

To Mr. NICHOLSON,

SIR,

LATE experiments in galvanism have furnished sufficient grounds to suspect, that the muriatic acid is an oxide of hidrogen, and I have been somewhat strengthened in this supposition by the well known fact, that hidrogen gas is always liberated upon effecting a solution of tin in muriatic acid: this phenomenon has been accounted for, by supposing the water which held the muriatic acid in solution to be decomposed; its oxigen seizing the metal which thereby became disposed to be taken up by the acid and the hidrogen, the other constituent part of the water being liberated under the form of gas: however plausible this hypothesis might seem, I did not think it perfectly satisfactory, for if the acid consisted of oxigen and hidrogen, part of the oxigen might unite to the metal to render it fit to be dissolved by the remaining acid, and its hidrogen of course given out under the gaseous form, in this case no decomposition of the water would take place, or at least these phenomena might happen without that decomposition. To clear up these doubts I procured an earthern tube into which was introduced some iron wire; the tube was made to traverse a furnace; to the one end was luted a bent tube, brought under the shelf of a pneumatic trough, and to the other was adapted a tubulated retort, containing some muriate of soda carefully freed from its water of crystallization. When I supposed the iron wire was sufficiently ignited, I affused some dense sulphuric acid over the muriate of soda; as soon as the atmospheric air which the vessel contained was nearly expelled, hidrogen gas was liberated from the other extremity of the tube in considerable quantities, mixed however with a small portion of muriatic acid gas; after the operation had been suffered to go on some time, the apparatus was taken to pieces, and crystals of muriate of iron were found in the tube. May we

Facts induce in favour of the position, that muriatic acid may be an oxide of hidrogen.

Experiment. Muriatic acid gas disengaged from decrepitated sea salt by sulphuric acid, was passed over ignited iron. Hidrogen was liberated.

It is inferred that this came from the acid.

we not from this experiment be sufficiently authorised to conclude, that muriatic acid is composed of oxigen and hidrogen, and that hidrogen gas is liberated in consequence of part of the oxigen of the acid uniting to the metal to predispose it to unite to the remaining acid?

It is to be remarked, that the hidrogen gas was liberated in such abundance as to do away every idea, that it might proceed from any water which the gas accidently held in solution.

Should you deem these observations of sufficient value, an insertion of them in your valuable journal will greatly oblige,

Sir,

Your most obedient,

and most humble Servant,

J. MARTIN.

Crown-Court, Old Bread Street,
February 20, 1806.

XIII.

Substance of a Memoir read before the Society of Emulation, at Amiens, by Messrs. REYNARD *and* FACQUER, *on the foul Air of Oil Cisterns* *.

Fatal effects of the foul air of an oil cistern.

M. ACHILLE POULAIN, soap-maker at Amiens, and one of his workmen having been killed by the foul air of an oil cistern, into which the latter had fallen in an attempt to cleanse it, and the former in endeavouring to save the man's life, Messrs. Reynard and Facquer were induced to make an analysis of the deleterious vapour which had caused this melancholy accident.

Dimensions of the cistern.

The cistern measured about twelve feet in every direction. Its mouth is secured with a small cover which completely excludes the external air.

Appearance of the oil.

The vegetable oil, of which only a small quantity at a time had been deposited in this cistern, was thick, viscid, and even in some places gelatinous, yielding a strong rancid effluvium.

* Annales de Chimie, Vol. LVI.

A lighted

A lighted candle on being let down into the ciftern, was inftantly extinguifhed.

The furface of lime-water, when included for a few minutes in a broad veffel, was flightly tinged with prifmatic colours.

To obtain the gas for experiments, bottles filled with water were lowered into the cisterns, and emptied at various depths.

On the gas obtained from about two feet below the mouth of the ciftern, the following experiments were tried: Obfervations on the gas.

1. A cylindrical veffel being filled with the gas, kept in contact with lime-water, during fifteen days, with frequent fhaking, caufed a fmall diminution in the bulk of the gas.

2. The fame experiment repeated with ammoniac offered a fimilar refult.

Thefe two experiments denote the prefence of carbonic acid gas.

3. The gas remaining from the two former experiments, when put in contact with liquid hydrogenated fulphuret of potafh, underwent an abforption of eight centimes; which muft have been oxigen.

The gas taken within a foot of the bottom of the ciftern afforded fimilar refults, only the proportion of carbonic acid gas was greater. That which remained after the effect of reagents was azote, as the following phenomena prove. It contained carbonic acid.

1. A lighted candle was extinguifhed by immerfion in the gas at the upper part of the cylindric veffel; but it remained burning if the veffel was previoufly opened for a few feconds.

2. The veffel when reverfed loft none of the gas contained in it; and the light was extinguifhed when introduced.

3. The luminous combuftion of phofphorus in oxigen gas (the formation of nitric acid with this gas and oxigen gas not having been tried), was confidered a pofitive proof of its nature.

This noxious gas was found to contain,

Analyfis.

Upper Part,		Lower Part,	
Azotic gas - -	86	Azotic gas - -	80
Oxigenated gas - -	8	Oxigenated gas - -	6
Carbonic acid gas -	6	Carbonic acid gas -	14
	100		100

 The

Chemical agency insufficient to destroy the foul air.

The nature of this gas does not admit of purification by lime or ammonia. These indeed destroy the carbonic acid, but have no influence on the azote.

Mechanical means more effectual.

Mechanical means are the only methods by which any considerable quantity of this air can be speedily removed; such as the firing of gun-powder, the use of ventilators, &c.

Destructive effects of confined air caused by the presence of azotic gas.

The result of this analysis is rather surprising, as, instead of a superabundance of carbonic acid gas, which was supposed to be the cause of the destructive effects of this confined air, azotic gas has been found—a gas lighter than atmospheric air.

Theory.

The theory of this result seems to be, that the oil having deprived the enclosed air of its oxigen, leaves only the azotic gas at liberty.

XIV.

Extract from a Memoir, by Messrs. FOURCROY *and* VAUQUELIN, *on the Phenomena observed in, and the Results obtained from Animal Matter, when acted upon by Nitric Acid. Read at the National Institute, by* A. LAUGIER.*

Berthollet's experiments on azote,

THE existence of azote in animal substances has been determined by the experiments of M. Berthollet, and the disengagement of this principle, when treated with nitric acid, is among the most useful of modern discoveries in chemistry.

—repeated.

Messrs. Fourcroy and Vauquelin, on repeating these experiments on muscular fibre, have added some interesting results to this valuable fact.

The following is a summary of their experiments, and of the results which they obtained.

Nitrous acid with muscular flesh gave azote and some carbonic acid.

SECT. I. A mixture of 150 grammes of muscular flesh, with an equal quantity of nitric acid, at 32 degrees, and water, put into a mattrass, and heated till it boiled gently, gave 96 cubic inches of gas, containing nine-tenths of azote, and one-tenth of carbonic acid.

The residuum contained fibrous matter, yellow liquor, and a greasy substance.

The residuum consisted of, 1, Matter which had not lost its original fibrous formation; 2, a yellowish liquor; 3, a greasy substance, of a yellow colour, which floated on the surface of the liquor.

* *Annales de Chimie*, Vol. LVI. p. 37.

After

After separating the grease, and filtring the liquor, the residue was submitted to the following experiments.

To boiling water it gave a yellow colour, and the property of reddening vegetable blues: After washing in several waters, it continued to turn the colour, though it ceased to give acidity. Washing rendered its colour deeper than at first; and when diffused in a little water, it still reddened paper of turnsol.

Its solution in alcalis was of a deep blood colour. It was precipitated by acids in yellow flakes.

This matter feels fat and pitchy; has a rancid smell, and very bitter taste. The fibrous matter resembles fat.

The fusion and swelling which it undergoes when placed on hot coals, the greasy vapour, and fetid colour, produced by this operation; the small quantity of coal which it leaves, shews its resemblance to fat substances, notwithstanding its acidity.

SECT. 2. On a closer investigation of the yellow matter, the following characteristics and properties were observed:

It so saturated alcalis as nearly to mask their properties.—Its combinations with potash and ammonia lathered like soap and water, and are not decomposed by carbonic acid, but precipitated the solutions of mercury and lead in yellowish white flakes. It saturates alkalis.

The yellow matter decomposed alcaline carbonates, in the cold, with effervescence, and likewise the acetate of potash, with the assistance of water, and a gentle heat. Decomposes carbonate.

The authors of the memoir next made use of alcohol, and found that the yellow matter was composed of a small quantity of fat, which was taken up by the alcohol; and of an acid, which, on account of its colour, they denominated "*yellow acid.*" This acid, when deprived of its fat, which occasions an alteration in its properties, was of a deeper colour, more readily reddened the paper of turnsol, did not melt in the same manner as before, nor exhale the same rancid smell, but fetid and ammoniacal vapours. It is a yellow acid and fatty matter.

The yellow acid is dissolved in the fat, to which it communicated acidity and rancidness. It combined with ammonia, and deprived it of its smell; and by distillation it yielded all the products of animal substances. Its constituent principles, therefore, are azote, hydrogen, carbon, and oxigen; and it must be placed among animal acids. The yellow liquor is an animal acid, consisting of azote hydrogen, carbon and oxygen.

SECT. 3.

SECT. 3. The combination of yellow acid and fat, on being again submitted to the action of nitric acid, at a temperature of about 50 degrees, underwent no remarkable alteration.—Its colour changed from yellow to white; its specific gravity was diminished, as was likewise its bulk; but without any motion or effervescence in the acid. Blue colours were deeply reddened by it; it dissolved, as before, in the ley of potash, to which it communicated an orange-red colour, and had an extremely acrid taste. The action of nitric acid upon this yellow matter seems confined to giving it properties which make it approximate to an oily state, without destroying its original acid character.

Experiments on the nitric acid wherein the muscular flesh had been decomposed.

SECT. 4. It was of importance that the nitric acid with which the muscular flesh had been decomposed, should be examined. Its yellow colour resembled that of the solution of chromate of potash. When saturated with carbonate of potash, the liquor at first acquired an orange colour, afterwards it became turbid, and deposited a small quantity of orange-red powder. On distillation, this mixture afforded a clear liquid, void of colour, of a rancid smell, containing a little ammonia, probably formed by the nitric acid. What remained in the retort, was of a blackish brown colour, but it was not farther examined.

A colourless liquor, having the same taste and smell, was afterwards obtained by distillation of another portion of the nitric acid used in the decomposition of the muscular flesh. The liquor remaining in the retort became yellow by concentration, and its re-action upon nitric acid was quickly perceived in a copious emission of red vapours. When reduced to 40 grammes, flattish crystals were formed in a thick mother-water, whose tenacity was similar to that of the solution of gum.

This mother-water possessed an acid bitter taste, and on the addition of a little caustic potash, became of a blood-red colour: mixed with alcohol, it deposited a white flaky sediment, which afterwards formed itself into fine semi-transparent grains, of a pleasant acid flavour.

Five decigrammes of this salt, on being calcined, left 21 centigrammes of yellowish very light residuum, which effervesced and were dissolved in nitric acid, and on being evaporated produced crystals of sulphate of lime and nitrate of potash.

This

This saline precipitate, obtained by means of alcohol, was ascertained to be a mixture of sulphate of lime and acidulous oxalate of potash.

The mother-water, after precipitation with alcohol, gave a second precipitate with lime-water, consisting of oxalate of lime. After this double operation with alcohol and lime-water, the mother-water, on being gradually evaporated, became converted into the brown viscid syrup, of a bitter taste, like that of walnut shells. This being mixed with a good quantity of alcohol, coagulated, and threw down a plentiful precipitate of white matter. This matter was very pure malate of lime, the alcohol having retained the yellow acrid substance.

The learned authors of the memoir, of which we have given this detailed extract, conclude from the facts above stated, Conclusions.

1. That the muscles contain potash, lime, and sulphuric acid, or perhaps sulphur burned by nitric acid.

2. That a portion of the muscular fibre, or rather the cellular membrane with which it is enveloped, was converted by the action of the nitric acid into oxalic acid and malic acid.

The alcohol employed in the separation of the malate of lime, held in solution, 1, A small portion of nitrate of lime; 2, A very bitter red-brown matter, possessing the flavour of walnut rhinds, of which more will be said hereafter; 3. A small quantity of that detonating matter already found in indigo: it was in this case obtained by concentrating the alcoholic solution, and separating it by the addition of carbonate of potash, in the form of granulated crystals, very inflammable, and very detonating.

Sect. 5. The importance of the results obtained from the foregoing analysis will be readily understood; particularly if a comparison be made of the knowledge hitherto possessed, with the extensive notions here opened to the view, of an object so interesting in the consequences which may be drawn from it, in the applications which may be made to the animal economy, and which, as will be shewn, leaves scarcely any thing more to be desired. Importance of the foregoing analysis.

The disengagement of azotic gas, the formation of carbonic acid, of fat, of oxalic acid, and of a bitter substance, constitute the whole that was known respecting the treatment of animal Discoveries added to what was formerly known on this subject.

animal subſtances by nitric acid; to this is now added the diſcovery, 1, Of a yellow inſipid matter, of little ſolubility, though acid, and which immediately ſucceeds the fleſhy fibre; 2, Of another yellow matter, bitter, more ſoluble, and equally acid, which remains diſſolved in the nitric liquor; 3, Of an inflammable, detonating ſubſtance, which is alſo retained in ſolution; 4, and laſtly, of the formation of malic acid.

It appears, and is the opinion of Meſſrs. Fourcroy and Vauquelin, that the yellow and nearly inſoluble matter is the firſt degree of change produced upon the muſcular fibre; it paſſes quickly to the ſecond degree of alteration and of acidity, whoſe product is the more ſoluble yellow matter: this, by a third degree of alteration is ſucceeded by the inflammable detonating ſubſtance, being the third and laſt term of the decompoſing action of nitric acid. The authors of this memoir attribute the ſucceſſive formation of theſe three compounds to the ſubtraction of part of the azote, and of a more conſiderable portion of the hydrogen: by this means the proportions of their elements are changed, and there remains an exceſs of carbon and of oxigen, which produces the ſtate of fat and acidity already noticed. As to the proportion of the conſtituent principles of theſe three compounds, it is a problem of too remote a nature for its ſolution to be readily diſcovered.

Acidity of the yellow ſubſtance not cauſed by nitric acid.

Meſſrs. F. and V. examined if the acidity of the yellow ſubſtances might in any meaſure ariſe from nitric acid; but, after a careful inveſtigation, they were ſatisfied that it was in no degree preſent.

Formation of oxalic and malic acids.

The formation of oxalic and malic acids belongs to the white mucous ſcales of the cellular membrane. Comparative experiments of the effects of nitric acid on the white membranaceous organs, which furniſhed plenty of theſe acids, and very little of the fat yellow matter, led the authors to this concluſion.

Sect. 6. A few inſulated facts, which hitherto have ſcarcely appeared to be ſuſceptible of any uſeful application, ſeem to unite with thoſe preſented by this analyſis; and the learned chemiſts, to whom we are indebted for it, have not omitted to connect them with the other facts. Such are thoſe which are obtained by examining the bilious concretions in certain animals; thoſe in the gall-bladder of the ox and elephant: and the analogy which appears to exiſt between bile, the

the colour of the skin in persons afflicted with the jaundice, and also their urine, and the yellow substance treated of in this memoir.

Analogy of the yellow matter to bile, jaundice, &c.

New experiments made with a view to confirm these suspicions obtained the most happy results. The red matter of bilious concretions, when separated from the bitter green matter with which it is combined, displayed similar properties with the first yellow matter obtained from muscles acted upon by nitric acid.

Bilious concretions.

From the urine of a young man troubled with a slight jaundice, they obtained a red substance, whose identity with the matter formed by muscles and nitric acid was remarkable. To obtain this, they evaporated the urine to the consistency of honey, and treated the residuum with alcohol: this contained, besides much of uree, sal-ammoniac, and acetate of soda, of which the patient made use, the red substance they sought for.

It was found in the urine of an icteric subject.

From these experiments, made with skill and ability, may we not conclude with the authors, that the jaundice is occasioned by a superabundance of this matter introduced to the cutaneous absorbent system; that this is what gives a yellow colour to bile and bilious calculi, which display, on analysis, the same properties; and that the yellow acid is dispersed throughout the animal economy, either by the oxigenation of the muscular fibre, or of the sanguineous fibrine, from which it is formed?

Jaundice occasioned by a superabundance of the yellow acid; which also causes the yellow colour of bile, &c.

Neither can we avoid admitting a striking analogy between this yellow acid matter, and the acid found in fat after long exposure to the air, or that has contracted a yellow hue through disease, and fat treated with nitric acid to form oxigenated pomatum.

Resemblance of the yellow acid and rancid matter of fat.

It must be confessed that these conjectures assume much probability, when we consider that the acetate of soda, alcaline carbonates, and yolks of eggs, are the remedies best adapted for the cure of the jaundice, and form also the best chemical solvents of the yellow acid, or of the acid and fat matter, which so evidently characterise the jaundice.

Other facts.

After what has been said, it must no longer be imagined that the hope of tracing the cause of morbific affections, is altogether chimerical: nor that discoveries in chemistry, and attentive researches

Chemical researches not to be neglected by physicians.

researches respecting animal matter, will not enlighten the physician on the nature of diseases, and the means of curing them.

XV.

Remarks relative to Dr. Herschel's *Figure of Saturn. By* An Observer.

To Mr. NICHOLSON.

SIR,

Singular circumstance that Dr. Herschel's figure of Saturn had not been before observed.

ON reading in your Journal, Observations on the singular Figure of the Planet Saturn, by Dr. Herschel, from the Philosophical Transactions; when I saw the engraving of the figure, as described by the Doctor, resembling a parallelogram, one side whereof is the equatorial and the other the polar diameter, with the four corners rounded off, so as to leave both the equatorial and polar regions flatter than they would be in a regular spheroidical figure; I was surprised to find, on enquiry, that so remarkable a figure had not been noticed before by other astronomers, whose telescopes were supposed to define objects very correctly, with powers considerably exceeding 160 times, by which power the Doctor could distinguish Saturn from the spheroidical figure of Jupiter.

Former obs. of the Doctor did not shew it.

In the year 1776, the Doctor relates he perceived the body of Saturn was not exactly round, and in 1781, that it was flattened at the poles, at least as much as Jupiter. In 1789, the Doctor being then prepossessed with its being spheroidical, he measured the equatorial and polar diameters, and supposed there could be no other particularity to remark in the figure of the planet.

It is evident, from the Doctor's former observations of Saturn and Jupiter, that the visible difference in their figures was not, before last year, observed so distinctly, owing to the superior excellence of his 10-feet telescope of two feet aperture, but that, when observed, he afterwards found the other telescopes gave a similar disparity.

Q. whether there was no deception in the telescopes.

As the figures given by former astronomers, and even by the Doctor himself, of both Jupiter and Saturn, were spheroidal,

dal, it may be requisite, before any intricate researches are attempted (as mentioned by the Doctor at the end of the communication), to be well assured that his telescopes have defined the figures of the planets accurately, which at present admits of a doubt, and which may be cleared up about the time of the next opposition of the Sun and Saturn, in April next.

The following may prove the necessity of such an enquiry:

Experiment. An object seen by oblique reflection from a spherical mirror, is rendered oblong.

Place a circular or spherical figure before a concave mirror, which mirror must be so inclined, that when the object is above the head of the observer, it may be seen, by reflection, in the center of the mirror *: If seen within the focus, the object will be represented oval in a vertical direction, and when beyond the focus, in a horizontal; which figure will be more and more oval as the angle is enlarged.

Your's,

AN OBSERVER.

XVI.

Experiments on a Mineral Substance formerly supposed to be Zeolite; with some Remarks on two Species of Uran-glimmer. By the Rev. WILLIAM GREGOR.†

Description and analysis of a mineral from Cornwall.

THIS mineral is raised in a mine called Stenna Gwyn, in the parish of St. Stephen's, in Branwell, in the county of Cornwall; the principal production of which is the compound sulphuret of tin, copper, and iron.

Description.

Two species of this mineral are found, assuming a marked difference in external character.

The first and most common one consists of an assemblage of minute crystals, which are attached to quartz crystals, in tufts, which diverge from the point of adherence, as from a centre. These tufts vary, as to the number of crystals, of which they

* If the object is small, it may be enlarged by a concave eye-glass.

† Phil. Trans. 1805.

are

Description and analysis of a mineral from Cornwall.

are composed, and are light and delicate in the forms which they assume, or they are grouped together according to a variety of degrees of proximity and compactness. Sometimes they fill the whole cavity of a stone, with little or no interruption; in other specimens they are seen partially spreading over the sides and pointed pyramids of quartz crystals.

In some cases these grouped tufts adhere very pertinaceously to the stone which bears them; in others, they are easily separable, in comparatively large pieces, from the quartz, the impressed form of which the pieces thus separated retain. The surface of these, which was in immediate contact with the quartz, exhibits the several minute crystals of which the mass consists, matted together in various directions.

These crystalline assemblages are, in general, white; a nearer inspection of the individual crystals proves that they are transparent. Sometimes they are stained of a yellowish hue by ochry water.

The size of these crystals varies considerably in different specimens. Sometimes they assume the appearance of a white powder raised up in small heaps, upon the surface of the stone, to which they adhere. In other specimens they resemble a tender down. And the larger sort varies, in relative size, in the proportion, perhaps, in which a human hair, horse-hair, and a hog's bristle, severally differ from each other in magnitude. They seldom exceed a quarter of an inch in length. The figure of these crystals is not easily ascertainable, on account of their minuteness. By the help of a very powerful microscope, they appear to consist of four-sided prisms; where these are broken off, the section exhibits a rhomboidal, approaching indeed to an elliptical figure, from the circumstance of the angles of the prism being worn away; but that the prism itself is rhomboidal, cannot be inferred from hence, unless we could be certified, that the section were at right angles with the axis of it.

Imbedded amongst these crystals two species of crystalline laminæ are frequently discoverable; the one consisting of parallelopipedon plates with truncated angles, applied to each other, of a green colour of various tints, from the emerald to the apple-green: the other species, consisting of an assemblage of square plates, which vary in thickness. The angles of the several square laminæ, which are applied to each other, are not

not always coincident. They are of a bright wax yellow. The sides of the largest of these square laminæ is about a quarter of an inch. This last species is frequently found adhering to the sides of quartz crystals, in the cavities of granite. Description an analysis of a mineral from Cornwall.

The other species of this mineral consists of an assemblage of crystals closely compacted together in the form of mammillary protuberances, in general, of the size of small peas, intimately connected with each other. A stratum of these about $\frac{1}{4}$ of an inch thick, is spread upon a layer of quartz, in the cavities or fissures of a species of compact granite. The striæ of which these mamillæ consist, diverge from a centre, like zeolite. Some of the individual striæ, in some cases, overtop their fellows, in these globular assemblages, and evidently assume, on their projecting points, a crystallized form.

A.

(1.) The detached crystals of the former species are easily reduced to powder, of a brilliant whiteness. At the temperature 56° of Fahrenheit, its specific gravity was found to be 2,22.

(2.) The hardness of the more compact species is sufficient to scratch calcareous spar. At the temperature 55°, its specific gravity was 2,253. It does not imbibe water.

(3.) Some of the crystals exposed, on charcoal, to the flame of the blowpipe suddenly and strongly driven upon them, decrepitate: if they are gradually exposed to the flame they grow opaque, and become more light and tender: but they show no signs of fusion under the strongest heat.

(4.) The phosphate of soda and ammonia takes up a piece of this mineral without effervescence, but it swims about the fused globule, unaltered. Borax dissolves a fragment of a crystal, and the globule remains transparent.

(5.) Some of this mineral, reduced to a fine powder, was mixed with about half its weight of pounded quartz, and kneaded with water into a ball: but as soon as the mass became dry, all cohesion was destroyed, and it fell into powder.

(6.) Sulphuric acid, poured upon some of it, caused no effervescence, nor was there any perceptible vapour extricated.

(7.) Some of the pulverized crystals were put into a crucible of platina, and sulphuric acid was poured upon them. The

Description and analysis of a mineral from Cornwall.

crucible was covered with a piece of glass, and placed in warm sand. On examination of the crucible and its contents, after some time, it appeared that the greater part of the mineral had been dissolved, but the surface of the glass cover was not in the least affected.

(8.) Some of the crystals were introduced into a small glass retort, to which a receiver was adapted. The retort was exposed to the heat of a charcoal fire. A fluid distilled over into the receiver, which had a peculiar empyreumatic smell. It changed litmus-paper to a faint red. It produced no change in a solution of nitrate of silver; but it caused a white precipitate in a solution of nitrate of mercury. I attributed these phænomena, at the time, to a small bit of the feather with which I had swept the powder into the retort, and which, I thought, had fallen into it. A slight whitish crust was also produced in the neck of the retort, but the smallness of the quantity did not admit of examination.

(9.) Some of this mineral, exposed to a red heat for about ten minutes, lost in weight at the rate of 25½ *per cent*. Another portion, exposed to a stronger heat for more than an hour, lost 30½ *per cent*. This operation was performed in a crucible of platina; the cover of which gave some indications as if a slight portion of the finer parts had been volatilized.

Some of the compact species, after exposure to a red heat for one hour, experienced a diminution in weight of 30 *per cent*.

(10.) The sulphuric, muriatic, and nitric acids, aided by a long digesting heat, effect nearly a complete solution of this substance. The quantity of the undissolved residuum is diminished in proportion to the purity of the mineral employed.

(11.) The nitrate of silver, as well as the muriate of barytes, produce no change in the solution of this substance in nitric acid.

(12.) The solutions of this substance in muriatic and nitric acids, cannot be brought to crystallize.

B.

(1.) I selected some of the crystals of this substance, as free as it was possible from extraneous matter. 50 grains grossly pounded were exposed, in a platina crucible, to a red heat for one

one hour. They weighed, *whilst still warm*, 35⅞ grains, which is a loss of 28¼ *per cent.* 25 grains of the same parcel, from which I had taken the former, exposed to a heat of longer continuance and greater intensity, were diminished in weight, at the rate of 30¼ *per cent.* Description and analysis of a mineral from Cornwall.

(2.) The powder still preserved its pure whiteness. It was transferred into a matrass, and nitric acid poured upon it, which soon began to act upon it. The matrass was placed, for many hours, in a digesting heat. A solution of the whole of the substance, except a small portion, was effected. I added a few drops of muriatic acid, and continued the digestion.

(3.) The acid was now diluted with distilled water, and poured off from the residuum, which consisted partly of a fine spongy earth, and partly of fragments of quartz. It was caught on a filter and sufficiently edulcorated. The last portion of edulcorating water dropped through the filter of an opalish hue.

The residuum, dried and exposed to a red heat, for ten minutes, $= \frac{3}{16}$ of a grain, $\frac{1}{16}$ of which consisted of fragments of quartz, $\frac{2}{32}$ was found to be silica, and $\frac{2}{32}$ alumina.

C.

(1.) The clear solution and edulcorating water were poured into a large matrass and boiled, and whilst boiling, the contents were precipitated, in white flakes, by ammonia.

(2.) When the ammonia had ceased to produce any further precipitate, the clear fluid was decanted, and assayed with carbonate of ammonia. But its transparency was not in the least disturbed.

(3.) This clear fluid, together with the edulcorating water, with which the subsided precipitate had been washed, was gradually evaporated. When its volume was considerably diminished, a separation of a spongy earth took place, more copiously than I had reason to expect, and the quantity of it was still further increased by a few drops of ammonia. This earth, thus separated, was sufficiently edulcorated, and added to the former precipitate.

(4.) The fluid was again evaporated, and at last transferred to a crucible of platina, and the salt reduced to a dry state: on redissolving this salt in distilled water, a minute portion of

 earthy

Description and analysis of a mineral from Cornwall.

the silica, in B, D, and F, $= 3\frac{1}{16}$; the oxide of iron (D.) $= \frac{3}{16}$, and lime F, $\frac{1}{16}$; the volatile parts of this substance $= 15\frac{3}{8}$ in the 50 grains employed.

The sum total of these is - - - - -	$47\frac{1}{16}$
Loss - - - - - - - - - -	$2\frac{15}{16}$
	50

I have subjected these crystals, as well as the harder speices of this mineral, to analysis by means of direct solution in sulphuric acid, and have found in each case the same fixed ingredients, viz. alumina, a small portion of silica, and a very minute quantity of lime. Both these latter ingredients are, I think, essential to the composition of this fossil, as I have always discovered them in the purest specimens. In this mode of analysis I experienced the same difficulty and tediousness of delay in bringing the last portions of the solution to crystallize into alum. This anomalous circumstance I have reason to attribute to a particular combination, which takes place between the sulphate of alumina and lime, silica, and potash. In my examination of the compact species there was no appearance of the sulphate of lime until the last; and in every experiment, previously to the fresh appearance of crystals of alum that had been long delayed, silica and sulphate of lime were deposited.

I forbear entering into any further details concerning my former experiments on this curious fossil, as I have reason to think that it will still require a more particular and minute examination, on account of another ingredient which eluded my notice, and which may possibly impart to its peculiar character. The scarcity of it has been hitherto a great bar to my experiments; I shall record, however, a few facts which I have lately observed, in the hope that at a future time I may be able to resume my examination of it.

I was induced to pay more attention to the volatile ingredients of this substance*. With this view, I introduced some

* Mr. Humphry Davy, whose well known skill and sagacity have probably rendered the researches of another person superfluous, had, I found, been engaged in the analysis of a mineral which is thought to be identical with the subject of these observations. He informed me that he had observed a peculiar smell, and acid properties in the water distilled from the substance which he examined.

of the crystals into a small retort, adapted a receiver unto it, and exposed the retort to a charcoal fire. The neck of the retort was soon covered with moisture, which passed into the receiver; and I observed a white crust gradually forming in the arch and neck of the retort.

Description analysis of a mineral from Cornwall.

On examination of the fluid in the receiver, it was found to have the same empyreumatic smell that I had observed before. It resembles very much the smell which that fluid is found to have which is distilled from the white crust that surrounds flint as a nucleus.

It changed litmus paper to a faint reddish hue. It produced no change on a solution of nitrate of silver, and scarcely a perceptible one, on that of nitrate of mercury.

The crust formed in the neck of the retort consisted of thin scales, which after the vessel had been dried, were disposed to separate from the glass in some places, but in others they firmly adhered unto it. They were opaque, like white enamel; and reflected the colours of the rainbow. A portion of this substance exposed to the flame of the blow-pipe upon charcoal turned at first black, and then melted into a globule, that exhibited somewhat of a metallic splendor which soon grew dull. This substance is soluble in water; on evaporation of it, it assumes, at the edges of the fluid, a saline appearance, which, as the moisture evaporates, becomes earthy, opake, and white. Some of the solution changed litmus paper to a faint red. Lime and strontian waters produce in it white clouds, which a drop of nitric acid removes. Muriats of lime and barytes produce no change in it. Nitrate and acetate of barytes disturb its transparency, the effect produced by the latter is more evident. Nitrate of silver produces no effect; but nitrates of mercury and lead cause copious precipitates, which are white and soluble in nitric acid. Phosphate of ammonia and soda produced a white precipitate. Oxalate, tartrite, and prussiate of potash did not affect it, nor did sulphate of soda. Ammonia was dropped into it, but the fluid preserved its transparency. But carbonate of ammonia instantly caused a white precipitate, which was not redissolved by an excess of the precipitant; upon some of this subsided precipitate a concentrated solution of potash was poured and shaken with it, but it was not

[Desc]ription and [an]alysis of a [min]eral from [Cor]nwall.

fenfibly diminifhed. But if after edulcoration it be diffolved in nitric acid, and potafh be added, no precipitate is produced.

Carbonate of potafh caufes a white precipitate when dropped into the aqueous folution of the fcaly fublimate.

The fupernatant fluid was poured off and gradually evaporated, but it became repeatedly turbid, nor could I by means either of the filter or alcohol prevent a recurrence of the fame effect. Nearly the fame refult takes place when carbonate of ammonia is ufed as the precipitant.

Some of the white fcales were moiftened with fulphuric acid. No vapour arofe.

Some of the precipitate obtained by means of carbonate of potafh from the watery folution of this fubftance, was, after fufficient edulcoration, diffolved in fulphuric acid; the folution, on due evaporation, produced permanent cryftals, fome of which refembled alum, but others feemed to differ from it in external character. Ammonia decompofed the folution of them in water, and a few drops of liquid potafh diffolved the precipitated earth. The quantity was too fmall for further experiment.

If diftilled water be poured into the retort and boiled in it, fo as to diffolve what adheres to the neck and cavity of it, a further folution is effected, but differing in fome meafure from the folution of the fublimate collected from the neck of the veffel. This latter folution is found to contain lead. If nitric or muriatic acid be poured into the retort, fo as to diffolve what *ftill* remains adhering to it, the prefence of lead becomes more evident. Whence does this metal arife? I have reafon to believe that it arifes from the glafs retort, which is corroded by the acid of the foffil extricated by heat. But what acid is it? It does not feem to be either the phofphoric or fluoric acids, the latter of which became the firft object of my fufpicion.

The opinion which Mr. Davy fuggefted to me feems more probable, that it is of vegetable origin. Oxalic acid, on the authority of Bergman, may be volatilized; yet fome of its properties are very extraordinary and do not accord with this idea.

I decompofed the watery folution of the fcales by nitrate of lead, and after a fufficient edulcoration of the fubfided precipitate

tate, I dropped upon it some sulphuric acid. No fumes were perceptible. The sulphate of lead was separated by the filter, and the clear fluid, which passed through it, was gradually evaporated; small crystallizations were formed, the figure of which I could not ascertain; some of them were exposed to the flame of the blowpipe in a gold spoon; they did not burn to coal, nor give out any empyreumatic smell nor fuse, but they assumed an earthy appearance *. Description and analysis of a mineral from Cornwall.

Uran-glimmer.

I shall add a few desultory remarks upon the yellow and green crystals, which frequently accompany the fossil.

I considered them to be the two species of uran-glimmer which had been examined by the celebrated Klaproth.

The yellow cubic crystals are light. Their specific gravity, taken at temperature 45° Fahrenheit, was 2,19.

Exposed to the flame of the blowpipe on charcoal, they decrepitate violently. A piece of this substance is taken up by phosphate of ammonia and soda, without effervescence, and communicates a light emerald green colour to the fused globule.

By exposure to a red heat, this substance loses nearly a third part of its weight. It then becomes of a brassy colour.

It is soluble in the nitric and muriatic acids; but I could procure no crystallized salt from the solution of either of them.

By evaporation to dryness, and redissolving the mass, some silica is separated.

A

(1.) A certain quantity of the yellow crystals were dissolved in nitric acid. Muriatic and sulphuric acids successively dropped into the solution produced no sensible change. The contents of the solution were precipitated by ammonia,

* I subjected some of the Barnstaple mineral, with which Mr. Rashleigh kindly furnished me out of his cabinet, to experiment, with a view of ascertaining whether it would produce the same volatilized saline crust, as the stenna gwyn fossil, and I found that it did.

in

Description and analysis of a mineral from Cornwall.

in white clots, mixed with some of a yellowish hue. Ammonia, added in excess, betrayed no sign of the presence of copper.

(2.) The ammonia, on evaporation, was found to have held a portion of the mineral in solution. A fresh portion of ammonia dissolved more, but in a less quantity, at each succeeding affusion of it.

(3.) The precipitate, which had resisted the ammonia, was boiled in a silver crucible, with a solution of potash in alcohol, diluted with distilled water, and a considerable portion of the substance was dissolved by it: the potash and the ammonia had dissolved rather more than half of the fixed ingredients of it.

(4.) The edulcorated residuum, which was of a dirty yellow colour, was transferred to a crucible of platina, and moistened with sulphuric acid, which was abstracted from it, in the sand-bath. The brownish-gray mass was elixated with distilled water, which dissolved nearly the whole of it. The residuum consisted of a white heavy powder, which, tried in different ways, was found to be *sulphate of lead*.

(5.) The solution effected by sulphuric acid was greenish. On evaporation, a salt was produced, of uncommon brilliancy, resembling scales of mica, or silver leaf. These diminished in quantity at every fresh solution and evaporation, and at last they could not be reproduced; but a confused crystallized mass remained. How far the platina crucible may have contributed to this phenomenon I cannot ascertain.

(6.) The solution of the saline mass was precipitated by potash, of a dark brown colour. The potash held nothing in solution. I redissolved the precipitate in nitric acid, and precipitated the solution by ammonia, of a bright yellow colour, peculiar to the *oxide of uranium*, with which it agreed in other properties.

(7.) What was dissolved by ammonia (2.) amounted to nearly $\frac{1}{3}$ part of the fixed ingredients. It was white, inclining to ash-colour. It tinged phosphate of soda and ammonia of a light green. It was soluble in sulphuric acid, except a few gelatinous flakes. The solution was greenish; gradually evaporated, it shot into a number of minute stellated crystallizations, which were circular, and consisted of rays diverging from a centre. They were, in general, colourless; a few of them

them were tinged of a smoke-colour. They soon became deliquescent. Upon evaporation, the same crystallizations were produced. After a time, some detached, regular, and permanent crystals were formed, which were colourless. Their figure I could not accurately ascertain. They were exposed to a red heat in a platina crucible. No ammoniacal vapour was perceptible. The crystals melted into opaque globules: some of these were transferred to a small glass, and distilled water was poured upon them. No solution took place apparently: on shaking the glass, the globules fell to pieces into gelatinous flakes, which were white. Some of the supernatant fluid was tried with muriate of barytes, which produced a cloud. But neither ammonia nor prussiate of potash caused any change in it. It is soluble also in nitric acid: the solution formed a confused crystallized mass, which soon became deliquescent. Zinc, immersed in it, caused the separation of white gelatinous flakes. Iron caused no change. Ammonia and potash threw down white precipitates, a portion of which were redissolved. The carbonates of soda, potash, and ammonia produced white precipitates. Prussiate of potash threw down the contents of the solution in distinct flakes, of the colour of mahogany; and the solution of galls in alcohol caused a light yellow powder to subside. It is soluble also in muriatic acid; the solution is a very dilute green. It requires an excess of acid to hold the substance in solution: which, after a time, deposits crystalline grains of a yellowish colour, which require a large quantity of water to dissolve them.

Description and analysis of a mineral from Cornwall.

Acetic acid does not dissolve this powder.

(8.) What was dissolved by potash (3.) was of an isabella colour: it was tried with nitric, muriatic, and sulphuric acids, neither of which could dissolve the whole of it. What resisted the two former acids was found to be silica. That which remained undissolved by the latter, was silica and sulphate of lead. Evaporation of the latter solution, betrayed also the presence of lime, in the state of sulphate. The nitric and muriatic solutions, on evaporation, deposited nitrate and muriate of lead; and sulphuric acid dropped into them produced a small quantity of sulphate of lime.

The nitrate and muriate of lead were decomposed by sulphuric acid, and the lead reduced on charcoal.

Ammonia

Description and analysis of a mineral from Cornwall.

Ammonia precipitated what remained in these solutions, and rediſſolved a part of the precipitates, which agreed in properties with that ſubſtance before mentioned (2.); the remainder was of a brighter yellow. But I could not bring the ſolution of it in nitric acid to cryſtallize.

B.

(1.) Some of the yellow cryſtals, which had not the ſlighteſt appearance of being contaminated with extraneous matter, were diſſolved in ſulphuric acid. *Silica was ſeparated;* and the preſence of *lime* and *lead* proved by the appearance of their reſpective *ſulphates.*

(2.) If ſulphate of ammonia is dropped into a ſolution of this mineral in nitric or muriatic acids, no change takes place, *immediately.* But on evaporation, a yellowiſh cruſt is depoſited, which is inſoluble in water. A ſolution of carbonate of ſoda in water, boiled on it, becomes yellowiſh-brown, and the greater part of it is diſſolved. The reſiduum, which is white, is reduced on charcoal to a globule of lead. What the carbonate of ſoda had diſſolved was found to be *oxide of uranium.* Sulphuric acid *alone,* does not produce this depoſited cruſt.

(3.) Some perfectly pure cryſtals were diſſolved in muriatic acid. Some ſilica was ſeparated. A few drops of ſulphuric acid were dropped into the ſolution, which produced no immediate change: on evaporation a white powder ſeparated, which conſiſted in part of ſulphate of lime. The remainder, expoſed to the flame of the blowpipe, was reduced to globules of lead.

The ſolution was decompoſed by ammonia, which rediſſolved a part of the precipitate; and, after edulcoration, the precipitate was diſſolved by nitric acid, and precipitated again by ammonia, which held a leſs quantity in ſolution. The edulcorated precipitate was now boiled with a ſolution of carbonate of ſoda, which diſſolved a large portion of it. The ſolution was yellowiſh-brown, and contained oxide of uranium. What was undiſſolved by the carbonate of ſoda was diſſolved in ſulphuric acid, and ſeemed to be the ſame ſubſtance as that which the ammonia held in ſolution. A. (2.)

The

The ſcarcity of this beautiful mineral has precluded me from operating on ſuch a ſufficient quantity, as a regular and rigid analyſis required.

Deſcription and analyſis of a mineral from Cornwall.

The ſubſtance, which is held in ſolution by ammonia, has ſome peculiar properties that ſeem to diſtinguiſh it from uranium. And if this mineral be the uran-glimmer, I have certainly detected the oxide of lead, lime, and ſilica in it, which have not hitherto been conſidered as ingredients of that foſſil. The green cryſtals differ in no reſpect from the yellow, except in containing a little of the oxide of copper.

XVII.

Examination of different Methods of ſeparating Nickel from Cobalt. By M. C. F. Bucholz.*

THE want of nickel and cobalt in a ſtate of purity induced M. Bucholz, to make experiments himſelf on the means of procuring them, and to repeat thoſe of others.

A. The able chemiſt Hermſtadt propoſed to ſeparate oxide of cobalt and oxide of nickel, by diſſolving the nitrate or ſulphate of cobalt, impregnated with nickel, in ammonia; and expoſing the ſolution to a ſingle evaporation. This M. Bucholz tried in the following manner, for the reverſed purpoſe.

M. Bucholz repeats Hermſtadt's method.

1. An ounce of cobalt ore *(cobalt ſpeiſe)* was diſſolved with heat in four ounces of nitric acid of the ſpecific gravity 1,220, and mixed with an equal quantity of water; which produced a reſidue of three drams of oxide of arſenic, in the form of ſmall cryſtals. When the ſolution mixed with half the quantity of water, coloured of a dull green, had been filtered and diluted with a great quantity of water, it depoſited a little of the oxide of biſmuth. Cauſtic ammonia was then mized with it to exceſs, until no farther apparent ſolution took place of the precipitate obtained. That which was not diſſolved, of a dull reddiſh white, was a compoſition of arſeniate of cobalt with a little of the oxide of biſmuth, and the oxide of iron.

One oz. cobalt ore diſſolved in nitric acid.

Depoſits 3 drams of arſenic.

The ſolution filtered and diluted depoſits a little biſmuth oxide.

Cauſtic ammonia added.

The undiſſolved reſidue is arſeniate of cobalt, with oxides of biſmuth and iron.

* Bucholz, &c. Journal of Chem. III, p. 2.

The

By evaporation oxides of cobalt and nickel are precipitated.

The ſolution being filtered, appeared of a beautiful blue; it was then evaporated at a gentle heat, by which about two drams of a bright green precipitate were obtained; which proved to be oxide of nickel, united to oxide of cobalt. The filtered liquor being then afterwards evaporated at the heat of a ſtove, depoſited ſtill an oxide of the ſame quality.

The ſaline maſs obtained, re-diſſolved, filtered, and boiled with cauſtic potaſh produces pure oxide of nickel.

The ſaline maſs of ammoniacal nitrate of nickel, of a deep green colour, which had been obtained by the evaporation, was re-diſſolved, filtered, and kept in ebullition with an exceſs of cauſtic potaſh, until the evaporation of the ammonia was compleated, by means of which a dram and half of oxide of nickel was ſeparated, which did not appear to contain any more oxide of cobalt.

Sulphuric acid tried.

2. As the ſeparation was not effected very well nor with much facility by the former method, the effect of ſulphuric acid was tried. For this purpoſe, an equal quantity of water was poured on the oxide obtained as before, and ſulphuric acid added till all was diſſolved by the aid of heat. It then evidently gave out an odour ſimilar to that of oximuriatic acid, although there was not any muriatic acid uſed. A like phenomenon, on a ſimilar occaſion, was before obſerved by the author (which is mentioned in the firſt ſection, page 18, of *Deitrage zur erweiterung*, for 1799.) The ſolution was then treated with ammonia as before, until the whole was almoſt diſſolved. The reſidue, which was oxide of cobalt with a little oxide of nickel, had the colour of verdigris. When the ſolution was evaporated at a gradual fire, and ſeparated by filtration from the precipitate, of which the greateſt part was oxide of cobalt, it was ſubmitted to ſpontaneous evaporation: It then cryſtalized without any farther ſeparation, partly into priſmatic cryſtals in groups, and of a green colour, and partly into cruſts united together, and blue at the edges. the eſſay of the oxides procured by potaſh from the ſolution of the cryſtals, as well as from the mother water, ſhewed that they contained cobalt almoſt in equal proportions,

Gives an odour of oximuriatic acid.

Treated with ammonia depoſits oxide of cobalt.

The ſolution cryſtaliſed.

The cryſtals contain cobalt and nickel.

The laſt experiment repeated on a larger ſcale.

3. Mr. Bucholz repeated the former experiments on a larger ſcale, in hope to obtain a better cryſtallization, and operated on eight ounces of cobalt ore, from which the firſt cryſtals, of a blueiſh green, obtained by a proceſs ſimilar to that laſt recited, and which weighed about five ounces, were again diſſolved in 32 ounces of boiling water: This ſolution was evaporated

till a pellicle was formed, and, after being filtered, near a stove, that it might cool slowly and crystallize. end of 48 hours, the greatest part of the salt was cry- in beautiful tetrahedral rhomboidal pyramids, short, a yellow green, of which the lateral faces formed an 115 and of 65 degrees, often with one extremity d, and always with an angle of 132 degrees towards inating face. This result proves that this salt forms adily into regular crystals by cooling than by slow eva- . All the crystals were then collected, washed with and again dissolved, and the nickel separated by boil- solution with potash till the ammonia was disen-

Produces fine crystals in tetrahedral rhomboidal pyramids.

The crystals dissolved and the nickel separated as before.

well to free this oxide from carbonic acid as to judge been purified from cobalt, it was dissolved in nitric d treated with pure ammonia in the same manner as n described. The liquor of a fine blue colour, (and ich a residue of five grains, which seemed to be an cobalt, had been separated by filtration), was eva- to dryness. After another solution then made, it de- an oxide of a beautiful bright green, which, after shed and dried, weighed half an ounce. The liquor, ssed the filter, was analysed by pure carbonate of the heat of boiling water, which then produced 170 f oxide of nickel, of a pale green, united to carbo- r a little of it was dissolved in muriatic acid, and some lution spread upon paper. On heating it afterwards, became yellow, and inclined but very little to a green. oxide of nickel, which separated spontaneously during oration, was dissolved in disengaging much oximuri- l; spread on paper, it exhibited the colour, when of a sympathetic ink of cobalt highly saturated; from it follows that it was more rich in cobalt than that pro- m the precipitation.

The oxide obtained dissolved in nitric acid and treated with ammonia, evaporated and re-dissolved deposits a green oxide.

The filtered liquor yields by potash 170 grs. oxide of nickel.

Which contains less cobalt than the spontaneous precipitate.

xides collected in those two ways, dissolved in nitric huric acids, after becoming grey, (which the author to be occasioned by the nickel dissolving first, and the greatest part of the cobalt remaining to the last, h opinion was not confirmed by other experiments this subject.) These oxides made lightly red in the nged their colour to a dark grey, and then, as well as on

The oxides dissolve in nitric and sulphuric acids.

Give out nitrous acid by fire, and by sulphuric acid;

on the addition of sulphuric acid, a disengagement of nitrous acid took place from the residue obtained by evaporation, which was also caused by the addition of an alkaline lixivium: With ammonia the same effects were produced which have been before mentioned.

Sulphites and nitrates of ammoniacal nickel always contain cobalt.

The results of the foregoing experiments are:—The sulphates and nitrates of ammoniacal nickel separated from cobalt ore, retain always some cobalt in their composition, and it is impossible from the method of Hermstadt modified in the preceding manner, to obtain an oxide of nickel without a mixture of cobalt.

The oxide of nickel remaining in the salt after evaporation contains very little cobalt.

b. By partially decomposing the ammoniacal nitrate of cobalt by evaporation, an oxide of nickel is obtained, very rich in cobalt, which contains nitric acid; and the oxide of nickel which remains undecomposed in this salt, retains a very small quantity of cobalt.

Dr. Schnaubert's method of obtaining pure oxide of nickel.

B. Doctor Schnaubert has published (in Tromsdorf's Journal of Pharmacy, vol. II. p. 66) a method of obtaining the oxide of nickel pure: Which consists in dissolving the metal of nickel mixed with cobalt, or its oxide separated from other substances, in nitric acid, in precipitating it by the carbonate of potash, and in heating it to a white heat, after washing and drying it. In this manner he always procured a yellow oxide, on which he caused very strong sulphuric acid to boil; which gave him a solution of oxide of nickel of a grass green, while the oxide of cobalt appeared in the form of a yellow residue. He proves the purity of the sulphate of nickel prepared in this manner, by the property which ammonia has of precipitating it of a bright green, and when added to excess, of re-dissolving it with a beautiful deep blue colour; but this argument appears insufficient to those who know that oxide of nickel, although mixed with many hundredth parts of cobalt, does not, however, experience any perceptible change in the colour of its precipitates, nor in its ammoniacal solutions.

His test of its purity defective.

He has not mentioned his proof that the other oxide obtained was cobalt, —nor the degree of heat to be used, —nor the strength of the sulphuric acid employed.

Besides the omission of indicating the means by which he was convinced that the oxide, which was the residue of the sulphuric acid solution, was really an oxide of cobalt, with the vague precept of heating the oxide acquired, without the least direction relative to the degree of the fire, and the uncertainty which he leaves of the degree of strength of the sulphuric

c acid which he used, altogether throw doubts on the fs of the process indicated, which the following expe- may elucidate.

Experiments on Dr. Schnaubert's process.

portion of the carbonic oxide of nickel, A 4, was l during an hour in a strong fire to a red heat approach- ite heat. The oxide while hot was of a brownish ; after cooling it assumed a grey colour inclining to , but not yellow. The oxide obtained by the evapo- A 4, having been treated in the same manner was still more grey than the preceding. The carbonic oxide of was placed again for half an hour in a white heat; ot it was yellow inclining to brown, but when cool, it y inclining to brownish yellow.

hirty grains of this oxide made red (hot), were put hours to digest, with ninety grains of pure sulphuric the specific gravity 1,860. Being then heated, the mediately swelled up with an explosive noise, and ex- a yellow substance inclining to a green; by means of n with half an ounce of water it was dissolved, ex- ut a grain of a yellowish-grey powder, which proved oxide of nickel mixed with cobalt and a little dirt. ive grains of oxide of nickel, (obtained by heating o redness 60 grains of ammonical nitrate of nickel l by evaporation), afforded the same result, and the enomena, on being treated in the same manner: The ide being heated for half an hour to whiteness, using ows at the same time, did not afford a yellow mass, of a yellowish grey inclining to a green, which had e effect with sulphuric acid that has been already re-

The experiment repeated with diluted sulphuric acid.

e experiment was again repeated with diluted sul- cid; 160 grains of ammonical oxide of nickel, which n precipitated from many solutions were exposed for hour to the most violent white heat, under the ope- f the bellows, after which they weighed 75 grains. stance was of a greenish yellow here and there, and ish grey where it touched the crucible; being broken ced a black grey powder. It was mixed with a dram uric acid diluted with five drams of water; at that here was a rapid disengagement of gas, and on heating ture it evidently gave out hydrogen gas. After a suffi- cient

cient ebullition, water was added, and the solution decanted off clear. The residue was treated again with weak sulphuric acid, and then gave a residue of ten grains which was by no means oxide of cobalt, but oxide of nickel mixed with cobalt, as its solutions in the acids and in ammonia proved. The two preceeding solutions were each separately analysed by pure potash, and the precipitate was besides heated with an excess of potash, and then washed and dried. At the proof each of the precipitates afforded cobalt, which was always most pure in that of the first solution; for the solution in muriatic acid, laid on paper, and heated, inclined perceptibly to a yellow, while the precipitate of the second solution produced a stain of a clear and pure green. It is strange that the first solution afforded more oxi-muriatic acid than the second.

The residue oxide of nickel mixed with oxide of cobalt.

Each of the precipitates of the foregoing solutions afford cobalt.

These experiments, and others made by the author, but not related, prove,

A. That the oxide of nickel heated either slightly or violently does not assume a yellow colour; and if this colour was observed by M. Schnaubert, it must have been caused by some substances which entered into the composition of the oxide, or perhaps by the mixture of a little arsenic.

The experiments prove that nickel oxide does not become yellow; cause of the mistake of Dr. Schnaubert on this point.

B. That it is impossible by M. Schnaubert's method, to obtain an oxide of nickel exempt from cobalt; since it does not even effect a separation of the two oxides so far as to be perceptible to the eye.

It is not possible to obtain pure nickel in his way.

C. M. Bucholz hints here at several experiments he made with a view to find an acid which would form an insoluble salt with one of the oxides, and one easy of solution with the other, but which, as they did not succeed, he does not mention; and as the method proposed by Mr. Lehman (in the *Cadmiologia*, part II, page 110) of fusing fifteen or twenty times, to a commencement of vitrification, a mixture of nickel and cobalt, in order to scorify all the cobalt, would be too troublesome and expensive, as would that also indicated by Bergman (*Opuscul, Physic, et chem,* Vol. II. p. 246—249) of repeating the fusion three or four times with from 8 to 12 times the quantity of pure nitre. The process indicated A 4, (consisting of a partial decomposition of the ammonical nitrate of nickel), alone remained to be repeated. For this purpose oxide

M. Lehman's method too troublesome and expensive, and M. Bergman's also,

the process A 4 repeated.

oxide of nickel, (which was separated from the triple salt, not dissolved at the first evaporation, by carbonate of potash, was treated repeatedly, (in such a manner) that after dissolving it in nitric acid, recourse, was had to the use of ammonia and evaporation as before described. In this method was obtained, entirely free from cobalt, an oxide. separated by potash from the triple salt, which had been redissolved after evaporation, and which oxide had the properties mentioned in the memoir printed in the second volume of the Annales de Chimie.

The oxide entirely freed from cobalt in the last operation.

The oxide which was separated by evaporation from the ammoniacal nitrate of nickel, was in the last operation entirely freed from cobalt; it only contained a still, as has been observed, a little nitric acid. The oxide of nickel, which, after having been laid bare by evaporation, still contains cobalt, may naturally undergo the same operation over again.

This method recommended for the present, the ammonia may be saved during the process.

This method may be made use of untill one more expeditious is discovered by farther experiments, since it does not occasion any considerable expence, for by potash, the evaporation of the ammonical nitrate of nickel may be effected in a retort, and also the subsequent decomposition of the triple salt, and thus the ammonia may be separated for other uses; in like manner, in works on a great scale, a part of the nitre may be recovered from the last operation, by the evaporation of the water in which the substance has been washed.

and the nitre recovered.

XVIII

Sugar prepared from Beets. By M. HERMBSTADT.*

THE method of M. Achard for extracting sugar from beets, was so expensive, that it was of no advantage for common use. M. Hermbstadt, of Berlin, has practised another method, which is easily performed, and affords hopes of rendering this sugar cheaper than that from the sugar-cane; which is as follows:

The expressed juice of the beet is clarified by lime,

After having bruised the beets in a mortar, M. Hermbstadt submits them to the operation of a press, to extract the juice from them; which is then placed in vessels, and clarified with lime in the same manner as cane-sugar.

* Sonnini's Journal, Tom. II. p. 431.

When

and then evaporated to a ſyrup. A coarſe ſugar is produced on cooling.

When this operation is finiſhed, the liquor is evaporated to the conſiſtence of a ſyrup: It is then left to cool, and a courſe ſugar is obtained, of a dark-brown colour: At the bottom of the veſſel a ſyrup remains, which may be uſed for domeſtic purpoſes.

From 100 lb. of this coarſe ſugar, 80 lb. of refined ſugar may be obtained.

From 100 pounds of the coarſe ſugar, eighty pounds of well cryſtallized ſugar are obtained by the firſt refining, which ſugar is not at all inferior in quality or whiteneſs to that of the cane. The whole operation may be completed in two days.

It is probable M. Hermbſtadt uſed the common field beet, or root of ſcarcity.

The particular ſpecies of beet which M. Hermbſtadt uſed in his experiments, is not mentioned; but it is moſt probable that this chemiſt made uſe of the common field beet, known in Germany by the name of *mangel wortzel*, the culture of which is ſpread through many cantons of Germany. This variety, however, contains leſs ſugar than all the other ſpecies of beets; and, for this reaſon, M. Sonnini is of opinion, that if ſugar can be obtained from beets with profit and economy, more ſucceſs would be obtained by ſubmitting to the operations deſcribed, the ſmall red beet, called in France that of *Caſtlenaudery*, which is the ſweeteſt of all.

A better produce might be obtained from the ſmall red beet of Caſtlenaudery.

XIX.

Method of ſtacking Turnips, to preſerve them through the Winter. By Mr. JOHN SHIRREFF, *of Captain Head, near Haddington, N. Britain.**

Rapa ſolo molli et aere humidulo lætantur.

Preſervation of turnips through the winter.

SATISFIED, from obſervation and experience, that turnips are the foundation of the beſt huſbandry on almoſt all ſoils and ſituations in the arable diſtricts of Great Britain; and that this crop ſhould always be drawn, except from blowing ſands, or light mooriſh ſoil, on both of which it ſhould always be in part conſumed on the ground with ſheep; convinced alſo, that turnips, if poſſible, ſhould be off all ſoils, and the land

* Soc. Arts, 1805. The premium of 30 guineas was awarded for this method.

ploughed

ploughed up before the middle of December, at the lateſt, to ſecure the ſucceeding corn crop, and graſſes, or clovers, with either of which every field that carried a turnip crop the preceding ſeaſon, ſhould, in almoſt every caſe, be ſown down; and impreſſed with the many high advantages attending this practice, as ſoon as my pea and bean ſtubbles are ploughed up, and ſown with wheat, my turnips are begun to be drawn, and ſtacked up for uſe during the following winter and ſpring. If the diſtance of the turnip-field from the homeſtead does not exceed a quarter of a mile, two double horſe carts only are employed, and more in proportion to the diſtance of the turnip field, or number of hands you may be able to command to carry on the work. One clever driver is ſufficient for two carts, and two for three carts, &c. one cart being always in the field loading or loaded. On being brought home, the turnips are inſtantly tumbled out at the ſtack; which is done with great facility, from the conſtruction of the carts in this diſtrict, which to convenience and ſtrength likewiſe add lightneſs, to enable horſes to move at a ſmart pace with them when empty. The turnips tumbled out of the cart, are trimmed of their leaves, and cleaned of any earth that may adhere to them, by women, &c. before being put into the ſtack. Old table-knives do very well for the purpoſe, and the leaves ſhould be cut off cloſe to the root; the back of the knife being uſed for removing any pieces of ſoil that may ſtick on the turnip. Preſervation of turnips through the winter.

Women, &c. trim the turnips, and put them into ſtrong coarſe wicker baſkets, to be carried forward by a man, who hands them to another, who lays them into or on the ſtack. The ground on which the turnips are placed ought to be dry bottomed. If that is not the ſort of ſoil where you find it moſt convenient to make your ſtack, a quantity of boulders may be put on, regularly ſpread over the ſpace, to the thickneſs of at leaſt eighteen inches. My corn-rick yard, being dry ground, has been uſed as the place for keeping my turnips in. The ſtacks have been made about ten feet wide, by driving a row of ſtakes into the ground parallel to the wall of the yard, which ſerves inſtead of another row. The wall is only about five feet and a half high, and the ſtakes are driven to the ſame height. The inſide of the wall and ſtakes are lined with compact bunches, or ſheaves of wheat-

 ſtraw,

Preservation of turnips through the winter.

ftraw, about ten inches in diameter, placed horizontally on the ground or boulders, and introduced, as wanted, during the operation of ftacking. A tire of the largeft turnips are placed one above another, on the infide of the bundles of ftraw, more particularly on the fide guarded by the ftakes, till the pile reaches the height of five feet from the ground, or from the boulders, if it has been found neceffary to fpread any over the ground. The inner part of the ftack is at the fame time gradually made up with turnips put in promifcuoufly; along which a plank is laid, and occafionally fhifted as the pile rifes, for the man who builds the ftack to ftand on without bruifing the turnips with his fhoes. When the pile of turnips is reared, in the manner defcribed, to the height of above five feet, it is gradually contracted inwards, on both fides, at an angle of about forty-five degrees, like the roof of a barn; the largeft turnips being ftill piled on the outfide, till the roof is fo far completed. The ftack is every day fo far finifhed in height as it is extended in length, and is covered with wheat ftraw thatch, roped down with twifted bands of oat ftraw before evening, to fecure the ftacked turnips from rain that may fall during the night. The thatch is laid on a foot thick, and fecured in the fame fimple, effectual manner, that corn-ricks are covered in Northumberland, Berwickfhire, and the Lothians; with this difference only, that the ftraw is four times as thick laid on the turnip, as on the corn, to exclude cold as well as wet; and that there is a rail of wood ftretched, hanging horizontally at the tops of the wall and ftakes, to fix the ftraw ropes to, which fecure the thatch on the ftack. The end of the ftack is every night covered with bundles of wheat-ftraw, which are removed next day, or when building recommences.

Three men are employed in the field to load and difpatch the carts, occafionally affifting four women who draw the turnips, ftriking off the top root with a ftrong heavy knife, leaving the turnips on the tops of the drills as drawn and chopped, with the leaves all in one direction, to be readily laid hold of by the men who lift them up to the cart. The horfes pafs along in the fpace between the two rows or drills of the turnips, which may be drawn: and, being at thirty inches apart, and the extremities of the wheels about five feet from each other, it is evident a wheel runs in the middle of

of each space between the contiguous drills, without injuring the turnip, whether drawn or not. When the cart is about to turn, after being loaded, the men move the turnips to make room for the horses, putting them into the cart as part of the load.

Preservation of turnips through the winter.

Expences of drawing, carting, trimming, stacking, covering, &c. a statute acre of good turnip,—at the distance of not more than a quarter of a mile from the stack.

	£.	s.	d.
Two double-horse carts, and one man -	0	16	0
Two men loading, drawing, building, &c. -	0	8	4
Seven women drawing and trimming - -	0	4	1
Two girls trimming - - - - -	0	1	0
Four ditto and boys ditto - - - - -	0	1	8
Twisting ropes, drawing thatch, thatching, waste of thatch, stakes, &c. say - - -	0	3	6
	1	14	7

The above is a fair average of the expence of securing somewhat more than twelve and a quarter statute acres last season, which was all I drew; and one field of two acres, one rood, thirty-three perches, was so far distant as to require three carts, and two drivers. That field, however, was first drawn, and the weather being fine and moderate, more work was done in proportion to the length of the day, which was also longer. Women and children cannot, indeed, exert themselves with spirit, in raw cold weather. October is perhaps the best month to draw in. It is a question with me, whether the average of the acres that are under turnip in the island, if the weight exceeds twenty-four tons, does not cost more, merely for drawing and carting only. When it is considered that this operation is performed often in cold, frosty, and stormy weather, and that frequently much snow may be to be removed before the turnip can be seen. If no snow has fallen before the frost sets in, the turnips must be hoed up with instruments for the purpose. Many are cut, and much left in the ground of the lower part of the root. After all this labour, what is obtained is frequently no better than a lump of ice, environed with earth, frozen so firmly to its surface, that nothing but thawing in

Preservation of turnips through the winter.

cold water can ever render it fit to be touched by the mouth of any animal whatever.

Admitting, however, the expence of drawing and carting to be the fame, all that can be ftated as extraordinary expence is the coft of trimming and facking, which amounts to 11s. 3d. an acre. On the other hand, we have the advantage of having fine frefh clean turnips, always fecure and at command, to carry on feeding and breeding ftock; at the fame time that all lofs by rotting in the fpring months is prevented, which is frequently thirty and even fifty per cent. on all the crop that remains in the field, after the firft of February. Above all, the practice of drawing and ftacking before winter, by admitting of early ploughing to mellow the foil, fecures a valuable corn, and fucceeding clover crops. When all thefe circumftances are maturely weighed, the expence of eleven fhillings and three-pence will, to every enlightened agriculturift, appear but trifling to obtain fuch very valuable advantages. The writer of this little effay has had the fatisfaction of having excellent crops after his turnips, this feafon; while almoft every other crop in the neighbourhood was indifferent; and fome on rich dry loams, high rented, by being fown in the months of April and May, on the fpring ploughing, after turnips eaten off with fheep, were fo miferable, as evidently to pay *nothing* after expences of labour, feed, and reaping. The young clovers too, fown with thefe crops, have almoft entirely perifhed from want of moifture. The lofs of the crop and clover feed is not all: the fyftem fuffers a derangement, the confequences of which none but practical men can calculate.

One thing remains to be noticed, which is, that twenty-fix young cattle, cows, and yearling calves, were kept nearly three weeks on the turnip trimmings, with oat-ftraw along with them, to their improvement; and that many more might have been kept, had they been provided in time. A quantity of good manure was made; and, eftimating all advantages arifing from the confumption of the leaves in this way, at no more than 3d. a head per night, for the keep of each beaft, the amount will exceed the expence of trimming and ftacking the whole crop of turnips on twelve acres and a quarter.— The leaves that remain on turnips after Chriftmas, are either unfit to be eaten, or wafted by the frofts.

T. SHIRREFF.

XX.

Account of some Specimens of Basaltes from the northern Coast of Antrim. By the Rev. Dr. William Richardson.*

THE Reverend Dr. William Richardson, late F. T. C. D. having sent to Dr. Hope a collection of specimens from the northern coast of Antrim, with a catalogue and observations, the specimens were exhibited, and the observations were read in the Royal Society, March 1803. Remarks on the basaltes of the coast of Antrim.

Siliceous Basalt.

Dr. Richardson discovered the fossil to which he gives this name, in the peninsula of Portrush, four or five years ago. It abounds also in the Skerry islands, a reef of rocky islots extending from the northern point of Portrush-head for about a mile eastward. A small part of every one of those islots is formed of this stone, while the remainder consists of coarse basalt, similar in all respects to that on the east side of the above-mentioned peninsula. It is met with in one or two other places.

This stone is arranged in strata, from ten to twenty inches thick, all steadily parallel to one another, and every stratum, as far as can be observed, preserving an uniform thickness through its whole extent. When these strata are quarried into, they appear to be constructed of large prisms, generally pentagonal, which when broken divide into smaller prisms. This internal prismatic construction frequently gives an irregular or shivery appearance to the fracture, which however is often conchoidal, and the grain as uniform as in the Giant's Causeway basaltes.

The beds of this fossil are remarkable for containing marine exuviæ in great abundance, particularly impressions of *cornua ammonis*. The flat shells and impressions contained in these stones, are steadily parallel to each other, and perpendicular to the axis of the prisms. It must be observed, that the prismatic construction is never interrupted by the shells dispersed through it; the planes which separate the prisms passing equally through the shells and the stone itself.

* Edinburgh Transf. Vol. V.

The

Remarks on the basaltes of the coast of Antrim.

The grain of this stone passes by insensible shades from a high degree of fineness, until it become undistinguishable from that of the common columnar basaltes.

The name of Siliceous Basalt, which Dr. Richardson employs, was first given to this fossil by Mr. Pictet of Geneva, when he visited Portrush, in a tour through Ireland two years ago. He considered it as a variety of basalt, containing a greater proportion of *silica* than usual.

The strata of siliceous basalt, both at Portrush and the Skerry islands, generally alternate with strata of equal thickness of a coarse-grained basalt of a grey colour. The materials of the strata grow into each other, so as to form one solid mass, from which it is easy to quarry pieces in the confine of the two strata, with a part of each adhering; but the coarse basalt, as it approaches very near to the fine, always abates somewhat of its coarseness; yet the line of demarcation is left completely distinct.

(The conclusion in our next.)

SCIENTIFIC NEWS.

Almanack printed at Constantinople.

Almanack printed at Constantinople.

FOR the first time an almanack has been printed at Constantinople, under the direction of Abdorahman. The printing-office was established in 1716, by *Said* (who had been at Paris with his father, the ambassador), and by Ibrahim, an Hungarian: Achmet the Third patronized them, and they printed many books; but an almanack was never before printed.

Observatory at Bavaria.

Bavarian observatory.

The Elector of Bavaria, a few months before the arrival of the French armies, caused an observatory to be erected in the neighbourhood of Munich. The situation chosen for its construction, takes in an extensive horizon. Professor Seyfer, a celebrated astronomer of Guttingen, was nominated director of this establishment.

Establishments

Eſtabliſhments for Natural Philoſophy in the Ukraine.

The rich land-owners in the Ukraine and Volhinia, have contributed largely for the eſtabliſhment of Lyceums for teaching natural philoſophy, at Krzeminico, and at Winnica. The library and philoſophical apparatus of the King of Poland, have been purchaſed for this purpoſe. M. Sniadecki has received a ſum equal to 500*l.* to purchaſe teleſcopes and clocks; and no expence is to be ſpared in properly furniſhing the obſervatories with inſtruments. Eſtabliſhment for natural philoſophy in the Ukraine.

Obſervatory at Moſkow.

M. Goldbach, an able aſtronomer of Leipſic, has been nominated profeſſor of the univerſity of Moſkow, with a ſalary equal to 250*l.* He is to have the direction of the conſtruction of a new obſervatory, to furniſh it with inſtruments, to make regular obſervations, and to inſtruct ſome young men in practical aſtronomy who have been previouſly inſtructed in the preparatory ſciences, and to give a courſe of lectures in theoretical aſtronomy in one of the halls of the Univerſity. Obſervatory at Moſkow.

They poſſeſs many of Cary's teleſcopes, of different powers; an excellent aſtronomical clock; a chronometer, made by Arnold; a portable circle, of one foot diameter; and, it was reported, had ordered one of three feet diameter from the ſucceſſor of Mr. Ramſden: Thus M. Goldbach will be provided with every inſtrument neceſſary and uſeful to aſtronomy, at the obſervatory of Moſkow.

M. Goldbach has taken the opportunity of his journey, to determine the poſition of ſome towns; among others that of Riga, $1^h\ 27'.0$, and $56°\ 57'.8$.

At the ſame time that M. Goldbach is engaged with the aſtronomical eſtabliſhment at Moſkow, MM. Schubert and Wiſniewſki are employed at the obſervatory of Peterſburgh; and there is reaſon to expect a ſeries of obſervations from that part of the world.

Solar Tables.

A ſet of tables of the ſun, compoſed by M. Delambre, have been printed at Paris, in which there are many new equations, Solar Tables.

tions, and of which all the elementary parts have been verified by new obſervations. A ſet of tables of the moon's motion are alſo to be printed, and when they are completed, thoſe of the planets will follow.

Bequeſt of Erneſt the Second relative to his Obſervatory.

Erneſt the Second's bequeſt to his obſervatory.

Erneſt the Second, late Duke of Saxe-Gotha, was remarkably attached to aſtronomical ſtudies. He made obſervations and calculations himſelf, aſſiſted in compoſing books on the ſubject, and furniſhed the funds for their publication. He enabled M. Zach to meaſure a degree of the meridian in Germany, and defrayed the expences from his private purſe; ſo that he united to the merit of a connoiſſeur in the ſcience, that of an author, a patron, a man of ſcience, and of a generous prince.

He left in his will a ſum equal to about 1330*l.* to form a fund for the maintenance of the obſervatory of Seeberg, near Gotha, which was built out of his own private eſtate; and ordered his ſucceſſor to erect no other monument to his fame, but the careful ſupport of this eſtabliſhment.

Baron de Zach, who has given a copy of the will in his Journal, adds, "That he can aſſure the lovers of ſcience, that the will of the father will not only be fulfilled, but ſurpaſſed by his ſucceſſor, the preſent Duke Emilius Leopold Auguſtus, who has already ſhewn the moſt marked proofs of his attachment to the ſciences.

"In a codicil to the will the Duke repeated, 'I forbid expreſsly the elevation of any monument to my memory, or even an epitaph, or any monument at or near my tomb."

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL, 1806.

ARTICLE I.

Letter from T. YOUNG, *M.D. F.R.S. &c. claiming the Lamp described in our last Number, and demanding an Explanation from the anonymous Communicator.*

To Mr. NICHOLSON.

SIR,

I WAS much surprized on seeing, a few days ago, the figure of a lamp contained in the fourth plate of your Number for February last. I trust you will be convinced, upon inspection of the figure which I now send you, and which was engraved before Christmas, that your correspondent A. F. must have copied his lamp from that which is here represented; and I am sure you will think I have a right to demand a public explanation of the manner in which he procured a sight of a plate not yet published, and of the motives which induced him to make so unjustifiable a use of it. I shall reserve the complete explanation of this lamp for the work to which the plate belongs, which has been long in the press, and which will soon be ready for publication; I shall only observe that it

Concerning the invention of a Lamp.

it is in a great meaſure free from the inconvenience which A. F. has attributed to it, (p. 168) and that the "ſmall ſhaded circle" is not a "perforation," but a weight attached to the counterpoiſe,

I am, Sir,

Your very obedient Servant,

THOMAS YOUNG.

Welbec Street,
March 15, 1806.

II.

On the Tendency of Elaſtic Fluids to Diffuſion through each other. By JOHN DALTON *.

Mixed elaſtic fluids of different denſities do not ſeparate;

IN an early period of pneumatic chemiſtry it was diſcovered that elaſtic fluids of different ſpecific gravities being once diffuſed through each other, do not of themſelves ſeparate, by long ſtanding, in ſuch manner as that the heavieſt is found in the loweſt place; but on the contrary, remain in a ſtate of uniform and equal diffuſion.

but will they mix without agitation.

Dr. Prieſtley has given us a ſection on this ſubject (vid. Experiments and Obſervations, &c. abridged. Vol. II. p. 441) in which he has proved the fact above-mentioned in a ſatisfactory manner; and every one's experience ſince, as far as I know, has coincided with his concluſions. He has not offered any conjecture concerning the cauſe of this deviation from the law obſerved by inelaſtic fluids; but he ſuggeſts that

Dr. Prieſtley thinks not.

"if two kinds of air of very different ſpecific gravities, were put into the ſame veſſel, with very great care, without the leaſt agitation that might mix or blend them together, they might continue ſeparate, as with the ſame care *wine* and *water* may be made to do."

The determination of this point, which ſeems at firſt view but a trivial one, is of conſiderable importance; as from it we may obtain a ſtriking trait, either of the agreement or diſagreement of elaſtic and inelaſtic fluids in their mutual action on each other.

* Manchester Memoirs, Vol. I. New Series.

It

It is, therefore, the subject of the following experiments to ascertain whether two elastic fluids brought into contact, could intermix with each other, independently of agitation. The result seems to give it in the affirmative beyond a doubt, contrary to the suggestion of Dr. Priestley; and establishes this remarkable fact, *that a lighter elastic fluid cannot rest upon a heavier*, as is the case with liquids; but, they are constantly active in diffusing themselves through each other till an equilibrium is effected, and that without any regard to their specific gravity, except so far as it accelerates or retards the effect, according to circumstances. Inquiry by experiment which shews the contrary.

The only apparatus found necessary was a few phials, and tubes with perforated corks; the tube mostly used was one ten inches long, and of $\frac{1}{20}$ inch bore; in some cases a tube of 30 inches in length and $\frac{1}{3}$ inch bore was used; the phials held the gases that were subjects of experiment and the tube formed the connection. In all cases, the heavier gas was in the *under* phial, and the two were placed in a perpendicular position, and suffered to remain so during the experiment in a state of rest; thus circumstanced it is evident that the effect of agitation was sufficiently guarded against; for, a tube almost capillary and ten inches long, could not be instrumental in progagating an intermixture from a momentary commotion at the commencement of each experiment. Apparatus.

FIRST CLASS.

Carbonic Acid Gas, with Atmospheric Air, Hydrogenous, Azotic and Nitrous Gases.

1. A pint phial filled with carbonic acid gas, the 30 inch tube and an ounce phial, the tube and small vial being filled with common air, were used at first. In one hour the small phial was removed, and had acquired no sensible quantity of acid gas, as appeared from agitating lime water in it. In three hours it had the acid gas in great plenty, instantly making lime water milky. After this it was repeatedly removed in the space of half an hour, and never failed to exhibit signs of the acid gas. Things remaining just the same, the upper phial was filled with the different gases mentioned above repeatedly, and in half an hour there was always found acid sufficient to make the phial $\frac{1}{4}$ filled with lime water quite milky. Carbonic acid gas with lighter gases.

milky. There was not any perceptible difference whatever gas was in the upper phial *.

SECOND CLASS.

Hydrogenous Gas with Atmospheric Air and Oxigenous Gas.

Hydrogen, with atmospheric air and oxigen.

1. Two six ounce phials were connected by the tube of a tobacco pipe, three inches long, the upper containing hydrogenous gas, the lower atmospheric air: after standing two hours, the lower phial was examined; the mixed gases it contained made six explosions in a small phial. The gas in the upper also exploded.

2. Two four ounce phials connected with the ten inch small tube stood two days, having common air and hydrogen gas. Upon examination the upper was found to be $\frac{1}{3}$ common air by the test of nitrous gas. The gas in the under exploded smartly; that in the upper moderately with a lambent flame.

3. Two one ounce phials were connected by the ten inch tube, containing common air and hydrogenous gas; in three hours and a half the upper was about $\frac{1}{3}$ common air and the under $\frac{2}{3}$; the former exploded faintly; the latter smartly.

4. Two one ounce phials were connected as above; the under containing gas about $\frac{3}{4}$ oxygenous, the upper hydrogenous: In three hours the latter was $\frac{1}{3}$ oxygenous, and the former about $\frac{1}{2}$; the upper exploded violently, the under, moderately.

5. Two one ounce phials were again connected, the lower having atmospheric air, the upper hydrogenous gas; they stood fifteen hours, and were then examined; the upper gave 1.67 with nitrous gas, the under 1.66.—Hence it is evident that an equilibrium had taken place, or the two gases were uniformly diffused through each other in both phials.

THIRD CLASS.

Nitrous Gas, with Oxigenous Gas, Atmospheric Air, Hydrogenous and Azotic Gases.

Nitrous gas with oxigenous, atmospheric hydrogenous, and azote.

The results of the preceding experiments upon gases that have no known affinity for each other, were conformable to

* The small tube of ten inches was then used and a phial of common air; in one hour much acid gas had come through, as appeared by lime water.

what

what *à priori*, I had conceived; for, according to my hypothefis, every gas diffufes itfelf equably through any given fpace that may be affigned to it, and no other gas being in its way can *prevent*, though it may confiderably *retard* this diffufion. But in fome of the following experiments, in which the two gafes are known to have a chemical affinity for each other, I expected different refults from what are found; perhaps without fufficient reafon. For, chemical union cannot take place till the particles are brought into contiguity; and the elaftic force which fets them in motion appears, from the above experiments, to be a principle diametrically oppofite to affinity. That circulation of elaftic fluids, therefore, which we have now before us, cannot be *accelerated* by their having a chemical affinity for each other. Another circumftance deferves explanation;—when nitrous and oxygenous gas are in the two phials, the refiduary gafes after the experiment are nearly as pure as before; becaufe thofe portions of them that meet in the tube, form nitrous acid vapour, which is abforbed by the moifture in the phials, and therefore does not contaminate either gas.

Nitrous gas with oxygenc atmofpheric l drogenous, ar azote.

1. Two one ounce phials were connected with the fmall tube, the under containing nitrous gas, the upper atmofpheric air; after three hours, the upper phial was taken off when a quantity of air was perceived to enter, as was expected; the air in the upper phial was fcarcely diftinguifhable from what it was at firft; that in the under phial was ftill fo much nitrous as to require its own bulk of common air to faturate it.

2. The above experiment was repeated, and the upper phial drawn off when the whole was under water, in order to prevent communication with the atmofphere: about $\frac{1}{6}$ of an ounce of water entered the phials, to compenfate the diminution. Remaining air in the upper phial was a very little worfe than common air, it being of the ftandard 1,47 when the former was 1,44. The gas in the under phial was ftill nitrous and nearly of the fame purity as at firft; for three parts of it required four of atmofpheric air to faturate them.

3. Nitrous gas and one $\frac{2}{3}$ oxygenous were tried in the fame way: after four hours, the apparatus was taken down under water. The upper phial was $\frac{2}{3}$ filled with water, and the gas

gas in it was partly driven down the tube into the other phial, by which, and the previous proceſs, the nitrous gas was completely ſaturated and nothing but azotic with a ſmall portion of oxigenous were found in the under phial: the remaining gas in the upper phial was ſtill $\frac{1}{4}$ oxygenous.

4. Nitrous gas and hydrogenous: in three hours the upper phial was $\frac{1}{4}$ nitrous, and of courſe the under muſt have a like part of hydrogen.

5. Nitrous gas and azotic: after three hours the upper phial was $\frac{1}{4}$ nitrous.

In the two laſt experiments, the quantity of nitrous gas in the upper phial was leſs than might be expected; but the tube was at firſt filled with common air, and ſome muſt enter on connecting the apparatus, which is ſufficient to account for the reſults.

FOURTH CLASS.

Azotic Gas, with Mixtures containing Oxigenous Gas.

Azote with oxygenous compounds.

1. Azotic gas and one $\frac{2}{7}$ oxygenous: after ſtanding three hours the upper phial was of the ſtandard 1.78, or about $\frac{1}{14}$ oxygenous.

2. Azotic gas with atmoſpheric air: after ſtanding three hours: the upper phial was not ſenſibly diminiſhed by nitrous gas; the under phial, however, had loſt two per cent, or $\frac{1}{10}$ of its oxigen. The reaſon of this was, that the azotic gas in this experiment having been juſt made for it from nitrous gas, this laſt had not been completely ſaturated with atmoſpheric air, and hence had ſeized upon all the oxygen aſcending into the upper phial.

Having now related all the experiments I made of any importance to the ſubject, it will be proper to add, for the ſake of thoſe that may wiſh to repeat ſome of them, that great care muſt be taken to keep the inſide of the tube dry; for if a drop of water interpoſe between the two gaſes, I have found that it effectually prevents the intercourſe: glaſs tubes ſhould therefore be uſed, that one may be ſatisfied on this head, as the obſtruction will then be viſible.

I ſhall make no further comments on the above experiments, by way of explanation: becauſe to thoſe who underſtand my hypotheſis of elaſtic fluids, they need none: and I think it would be in vain to attempt an explanation any other way.

I can

I cannot however, on this occasion, avoid adverting to some experiments of Dr. Priestley, which few modern philosophers can be unacquainted with: I mean those relating to the seeming conversion of water into air. (Vid. Philos. Transact. vol. 73, page 414,—or his Expts. abridged, vol. 2, page 407.) He found that unglazed earthern retorts containing a little moisture, when heated, admitted the external air to pass through their pores at the same time that aqueous vapour passed through the pores the contrary way or outward; and that this last circumstance was *necessary* to the air's entrance. The retorts are air-tight, so far as that blowing into them discovers no pores; but when subjected to a greater pressure, as that of the atmosphere, or even one much short of it, they are not able to prevent the passage of elastic fluids. The fact of air passing into the retort through its pores, and vapour out of them at the same time, are elegantly and most convincingly shewn by Dr. Priestley's experiments, in which he used the apparatus represented in plate 7, fig. 1, of the edition above referred to. The Doctor confesses his explanation of these remarkable facts is very inadequate; and no wonder, for it is impossible for him or any other to explain them on the commonly received principles of elastic fluids. But we will hear what he says on the subject:—"At present it is my opinion, that the agent in this case is that principle which we call *attraction* of *cohesion*, or that power by which water is raised in capillary tubes. But in what manner it acts in this case I am far from being able to explain. Much less can I imagine how *air* should pass one way and *vapour* the other, in the same pores, and how the transmission of the one should be necessary to the transmission of the other.—I am satisfied, however, that it is by means of such pores as air may be forced through, that this curious process is performed; because the experiment never succeeds but in such vessels as, by the air-pump at least, appear to be porous, though in all such."

The remarkable experiment of Priestley, of air entering earthen retorts while water passed out in a vapor.

Dr. Priestley's explanation or conjectures.

The truth is, these facts so difficult to explain are exactly similar to those which are the subject of this memoir: only instead of a *great number* of pores we have *one* of sensible magnitude, (the bore of the tube.) Let the porous retort have the same elastic fluid within and without, in the one case; and the two phials contain the same elastic fluid in the other, then

The fact is, that the steam and air mix by means of the pores.

then no transmission is observable in either; but if the retort have common air, or any other gas, without, and aqueous vapour, or any other elastic fluid, except the outside one, within; then the motion in and out commences, just as with the phials in similar circumstances. In fact this last observation has since been verified by Dr. Priestley himself, of which an account is given in No. 2, of the American Philosophical Transactions, vol. 5. After alluding to his experiments above-mentioned, he observes, "Since that time I have extended and diversified the experiments, and have observed, that what was done by *air* and *water*, will be done by *any two kinds of air*, and whether they have affinity to one another or not, that this takes place in circumstances of which I was not at all apprized before, and such as experimenters ought to be acquainted with, in order to prevent mistakes of considerable consequence."

—and the same happens in any two gases.

The facts stated above, taken altogether, appear to me to form as decisive evidence *for* that of elastic fluids which I maintain, and *against* the one commonly received, as any physical principle which has ever been deemed a subject of dispute, can adduce.

III.

On the Horizontal Moon. By Dr. OKELY. *In a Letter from Mr.* H. STEINHAUER.

To Mr. NICHOLSON.

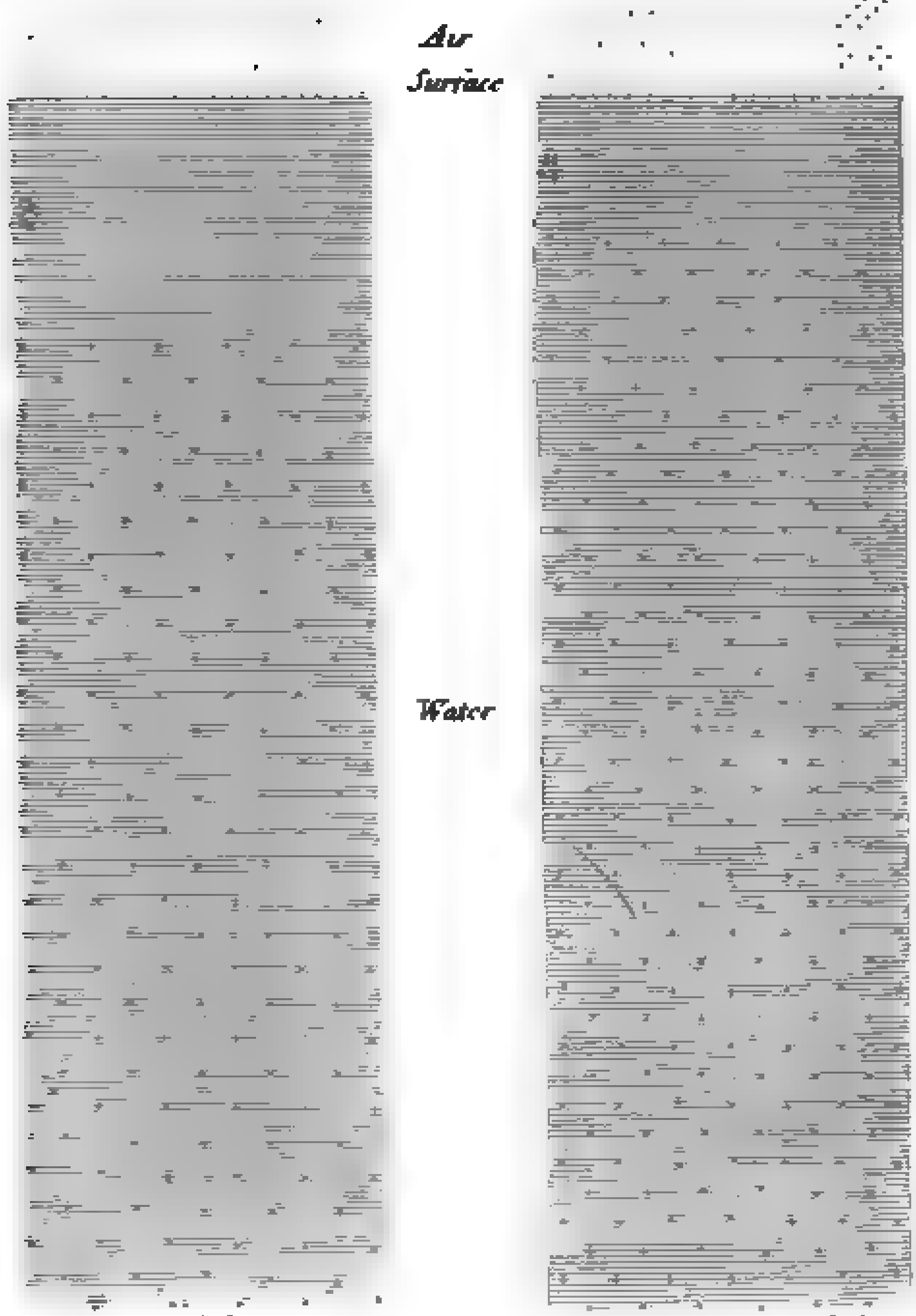
SIR, *Fulnuk, March* 1, 1806.

ABOUT the beginning of last year, I had the pleasure, in compliance with your obliging letter to send you impressions of the Egyptian Scarabacus, which I hope came safe to hand. Your kindness in inserting my trivial remarks upon the same in your valuable Journal, encourage me to submit the following short essay, upon a subject which has employed the ingenuity of several of your correspondents, which I received from my friend Dr. Okely, of Wyke, near Hallifax, in consequence of some conversations occasioned by the perusal of your work.

If

Profile View of Air in Water

by Mr. Dalton



Azotic and Hydrogenous gas $\frac{1}{64}$ Density.

Oxygenous Nitrous & Carburetted Hydrogen gas $\frac{1}{27}$ Density.

View of a square Pile of Shot &c

The lower globes are to represent particles of water the top globe represents a particle of air resting on 4 particles of water

Fig 1

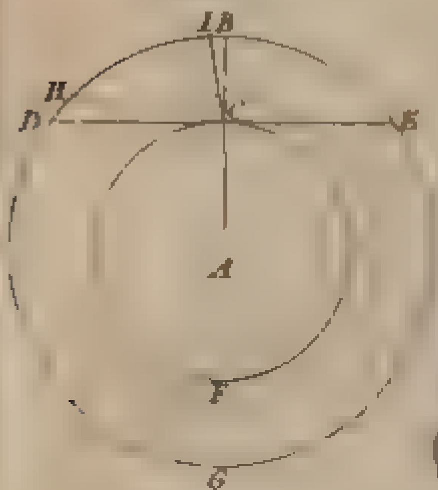


Fig 2

Horizontal View of particles of Air in Water by Mr Dalton

Incumbent particles are marked

Absorbed particles

Fig 3

Azotic & Hydrogenous gas
Distance of Particles 4 to 1

Oxygenous Nitrous & carburetted Hydrogen Gas
Distance of Particles 3 to 1

If you think it worthy a place in your collection, it will be considered as an additional obligation conferred on,

SIR,

Your obedient servant,

H. STEINHAUER.

Observations on the seemingly enlarged apparent Diameters of the Sun and Moon, when viewed in or near the Horizon.

Every one who views the sun or moon, when they are in the horizon, thinks that they appear larger than when they are seen in any more elevated part of the heavens. And astronomers know, that the distance of the same fixed stars is apparently greater when seen near the horizon, than when they are more elevated. But it is likewise well known to astronomers, that the apparent magnitudes of the sun and moon, as well as the apparent distances of any given fixed stars, as measured by the micrometer, are the same in that part of the heavens which is near the horizon, as, in the same circumstances, they are found to be in any other part, except that the moon, being really perceptibly farther from an observer, placed on the earth's surface, when she appears in the horizon, than when she appears in the zenith, is found to have a smaller apparent magnitude, agreeing with the causes to which it is known to be owing. The first mentioned phenomena must therefore belong to the head of *optical deceptions*. Let us enquire from what source this deception arises.

General fact stated that the heaven'y bodies seem larger at low altitudes.

I am not the first by whom the source was sought for in the apparent flatness of the sky; but I differ as far as I know, from all others in my manner of connecting one appearance with the other.

In order to explain my idea of the matter, I shall first attempt to shew that the flattened appearance of the visible heavens is not an illusion, but a reality; or in other words, that an observer placed on the earth is really at a greater distance from a point of the sky, situate in the horizon, than from a point situate in the zenith.

Explanation of the flattened appearance of the heavens.

This will appear in the clearest manner if we endeavour to give an answer to the two following questions: *What is the sky?* and *Where is the sky?*

The sky is a real object, and offers a flattened concavity.

By the sky, I mean that blue concave superficies, within which every observer on the surface of the earth finds himself placed

placed. What is this? It is certainly ſomething real and material, or elſe it would not appear coloured. For bodies, to appear coloured, muſt have parts of ſome determinate magnitude.

Where doth it exiſt? Not in thoſe immenſely diſtant parts of ſpace, where the heavenly bodies revolve. For if thoſe ſpaces contained any bodies of a determinate magnitude, and conſequently of a determinate denſity, the heavenly bodies could not continue through ages to revolve in the ſame periodic times; their momentum would be diminiſhed by reſiſtance, and the periodic times of their revolution would change. The blue ſky therefore cannot be placed beyond the atmoſphere of our earth. The ſmalleſt parts of bodies, that are coloured are blue, and the blue ſky is therefore either the atmoſphere itſelf or the ſmalleſt and moſt elevated vapours aſcending in it, or both together. The heavenly bodies ſhine through it, and therefore it cannot be opaque; it is itſelf of a blue colour, and therefore is not perfectly tranſparent.

Though we are ignorant of the exact height of the atmoſphere, yet we may take it for granted, that it does not extend as far as the moon, and therefore that the diſtance of its fartheſt points from the centre of the earth has a finite ratio to the ſemi-diameter of the earth. That the ratio is probably leſs than 2 : 1.

If therefore the blue heavens which ſurround the earth, and are concentric with it, have a ſemidiameter not double that of the earth, their horizontal points as viewed from the earth, muſt be farther from us than any that are nearer the zenith.

For let A C F (*Plate* VII. *Fig.* 1.) repreſent a great circle of the earth, and A C be its radius, and let the circle D B E G repreſent a great circle of the atmoſphere drawn with a radius A B not $=$ 2. A C, the line C D $>$ B C; H C $>$ B C.

—which is not always alike.

I was led to this ſolution of the flattened appearance of the heavens, by obſerving that, when the ſky is uniformly overcaſt with clouds, the concave ſuperficies appears conſiderably flatter than when the ſky is ſerene. In the former caſe, the two concentric circles in the figure approach nearer to each other, the clouds being nearer to the earth than the ſky is, and the ratio of D E to C B muſt of courſe increaſe.

Explanation from a diagram of the ſky, that

But to proceed. When any bodies ſituated behind a ſemitranſparent ſcreen are ſeen through it, they will appear to be fixed

fixed in the ſcreen at the points of interſection, which lines drawn from every point of the bodies to the eye of the obſerver, make with the ſcreen. Now ſuch a ſemi-tranſparent ſcreen, the blue ſkies interpoſe between the heavenly bodies and our eyes. They will therefore appear to be fixed in the ſky, at the above-mentioned points of interſection.

the heavenly bodies muſt appear largeſt in the horizon.

But if lines DC, HC, IC, BC, be drawn ſo that the angles at C are equal, they may be conſidered as coming from the extreme points of bodies which ſubtend equal angles of viſion, or which have the ſame apparent magnitude. The angle DCH may be conſidered as repreſenting the angle of viſion which the ſun ſubtends at the horizon. The equal angle HCI, the angle ſubtended by the ſame body in a more elevated ſituation. But DH $>$ HI. Thus the heavenly bodies muſt appear enlarged in their vertical diameters, when in the horizon; and the ſame may be ſhewn of any other diameter They will therefore appear uniformly enlarged; which was the thing to be explained.

W. OKELY.

IV.

Account of ſome Specimens of Baſaltes from the northern Coaſt of Antrim. By the Rev. Dr. William Richardson.

(Concluded from Page 273.)

Remarks on the baſaltes of the coaſt of Antrim.

THE peninſula of Portruſh lies about ſix miles to the weſt of the Giant's Cauſeway, and on its eaſtern ſurface alone preſents theſe ſtrata.

In the ſpace of about 700 yards, it exhibits in miniature thoſe changes and interruptions of the ſtrata, which occur on the large ſcale along the northern baſaltic coaſt of Ireland. At the place where it emerges from the ſtrand, there firſt occurs a maſs compoſed of ſtrata of the coarſe and ſiliceous baſalt, placed over each other alternately; this is ſucceeded by an accumulation of regular ſtrata of the coarſe baſalt alone. A ſecond alternation, and a ſecond accumulation of the coarſe-grained ſtrata, come in order, and extend to the well called Tubber Wherry. Here commences an accumulation of many ſtrata of the ſiliceous

Remarks on the basaltes of the coast of Antrim.

ceous basalt alone, which stretches along the shore for about 100 yards, and then changes into a third alternation, which continues to the little boat-harbour, called *Port-in-too*, near which the siliceous basalt disappears. Over this stretch, notwithstanding the frequent change in the arrangement of the strata, the thickness of each stratum, of both species, remains pretty nearly the same, and the position of them all steadily so, viz. with a considerable dip to E. N. E.

The west side of the peninsula, though only about 400 yards distant, consists entirely of coarse basalt. It shows a bolder face, and is formed of rude massive pillars, from 60 to 80 feet long.

"I am aware," says Dr. Richardson, "that several mineralogists deny the shell-bearing stone to be basalt, while others contend strenuously that it is. I will not venture to decide on the question, but must remark, that I have never met with it but contiguous to basalt, and so solidly united to this last, that the continuity of the whole mass was uninterrupted. The grain of the stone graduates, as has been already remarked, into that of the common basaltes; and the arrangement of it and that of the basalt, with which it is so much mixed at Portrush and the Skerry island, is exactly the same; the strata of each scarcely differing in thickness, and not at all in inclination. The strata of both kinds break into prisms, and the surfaces, where accessible, exhibit the appearance of causeways, differing only in this, that in the siliceous basalt, the pentagon is the prevalent figure, and in the coarse basalt, the quadrangle. The fusibility of both stones is also nearly the same; the shells in the siliceous basalt are calcined in the fire, and many more are then discovered which had before escaped the eye *."

Whinstone

* Dr. Richardson observes, that some mineralogists deny that this fossil is basalt. Several of the members present when this paper was read, some of whom had examined the stone in its native place, were of that number. It was remarked, that though certain portions of the strata of this fossil bore much resemblance to some species of basalt, by far the greater part of the mass bore no resemblance whatever to any.

It was also stated, that the substance of the coarse-grained, undisputed basalt, which lies between the strata of this stone, does not contain any vestiges of marine animals; That veins often issue from

Whinstone Dikes on the Coast of Antrim.

Dr. Richardson describes some particulars in the construction of the whinstone dikes on the coast of Antrim, which appear singular, and deserving of attention. These dikes, he says, are uniformly formed of large massive prisms laid horizontally, which are always divisible into smaller prisms that are likewise horizontal. To prevent confusion, he calls the first of these *component prisms*, and the second, or smaller ones into which the others break, *constituent prisms*. Remarks on the basaltes of the coast of Antrim.

The component prisms are sometimes of enormous size, and in the same dike are nearly equal; the constituent prisms are small, (the sides about an inch long), and neatly formed.

The dike which traverses the Giant's Causeway, differs from those on other parts of the coast, by having no component prisms. It resembles a plain wall, of which the parts shiver under the hammer into very neat constituent prisms, In the dike at Seaport the same thing is observed; the prismatic structure does not penetrate two inches from its edge; the whole interior seems an amorphous mass.

The specimens of this latter dike, sent to Dr. Hope, exhibit its continuity with the adjacent basaltic rock which it traverses, and also the continuity of the fine basalt of its edge with the *granular* stone which composes the middle of the dike.

The dike of *Port-coan* is a very solid mass, composed of stones apparently round, and imbedded in a basaltic paste, or indurated mortar. The round stones are formed of concentric spheres, like the coats of an onion; they exceed a foot in diameter, and, together with the mortar by which they are united, they form a very compact and highly indurated rock.

Besides these large dikes, Dr. Richardson remarks, that veins from half an inch to an inch and a half thick, often cut the basaltic strata on that coast in all directions. The materials of these veins are never the same with the contiguous basalt,

from the beds of this real basalt, and pervade the supposed siliceous species; some of them connecting together the separate beds of the real basalt; others dying away in slender ramifications; as they rise through the interposed stratum. In no instance is this reversed: The veins never proceed from what is called the Siliceous Basalt. It was farther observed, that both the fracture and external surface of this stone exhibit a stratified structure, in many instances, which never happens in the true basaltes.

but

Remarks on the basaltes of the coast of Antrim.

but are generally finer. At Portrufh is a large vein, and near it a fmaller vein, not an inch thick, which, proceeding from below, terminates in the folid rock before it reaches the furface.

Mifcellaneous Obfervations.

Some of the fpecimens in Dr. Richardfon's catalogue are from a quarry in a mafs of bafalt at Ballylugan, two miles fouth of Portrufh. This bafalt contains fmall cavities in its interior; many of them full of frefh water, which gufhes out when the ftone is broken by the hammer, as if it had been in a ftate of compreffion. The ftone is fo hard, and flies fo in pieces, that Dr. Richardfon has not been able to collect any of the water for the purpofe of analyfis.

The face of the quarry in which this variety of the bafalt is found is about 15 feet high, and is cut into a ftratum, the thicknefs of which is not yet afcertained. The rock is entirely columnar, the pillars fomewhat fmaller than thofe of the Giant's Caufeway, lefs perfect, not articulated, fometimes bent, and varioufly inclined. The fides and the interior of the pillars are full of cavities. In confequence of the obfervations of Dr. Hamilton and Mr. Whitehurft refpecting the porous texture of the air or bladder holes of the bafaltes of the Caufeway and its vicinity, Dr. Richardfon has examined a great variety; but in no inftance, except this of Ballylugan, has he found cavities, in the interior of the bafaltic rocks on this coaft, though they are frequent on the furface expofed to the air.

The laft variety of whinftone enumerated by Dr. Richardfon is the Ochrous, which makes, as he fays, a confpicuous figure in the ftupenduous precipices along the coaft of Antrim. It is difpofed in extenfive ftrata of every thicknefs, from an inch to twenty-four feet, and varies in colour, from a bright minium to a dull ferruginous brown.

Three remarks are made by Dr. Richardfon, that are undoubtedly of importance, and fhow that this ftone is merely bafalt in a certain ftate of decompofition.

1. The ochrous ftrata are extenfive; they remain always parallel to the bafalt ftrata which they feparate; they unite to the bafalt without interrupting its folidity; the change from the

one to the other is ſudden, and the lines of demarkation are diſtinct. The ochrous ſtone is never found but contiguous to other baſalt.

2. The ſubſtances imbedded in the ochrous rock, and in baſalts, are exactly the ſame; calcareous ſpar, zeolite, chalcedony, &c.

3. Among the varieties which this rock preſents, there may be found every intermediate ſtage between ſound baſalt and perfect ochre. The change is often partial, beginning with veins and ſlender ramifications.

V.

On the Abſorption of Gaſes by Water and other Liquids. By JOHN DALTON.*

1. IF a quantity of pure water be boiled rapidly for a ſhort time in a veſſel with a narrow aperture, or if it be ſubjected to the air-pump, the air exhauſted from the receiver containing the water, and then be briſkly agitated for ſome time, very nearly the whole of any gas the water may contain, will be extricated from it.

Air or gas is extricated from water by boiling and agitation in vacuo.

2. If a quantity of water thus freed from air be agitated in any kind of gas, not chemically uniting with water, it will abſorb its bulk of the gas, or otherwiſe a part of it equal to ſome one of the following fractions, namely, $\frac{1}{8}$, $\frac{1}{27}$, $\frac{1}{64}$, $\frac{1}{125}$, &c. theſe being the cubes of the reciprocals of the natural numbers 1, 2, 3, &c. or $\frac{1}{1^3}$, $\frac{1}{2^3}$, $\frac{1}{3^3}$, $\frac{1}{4^3}$, &c. the ſame gas always being abſorbed in the ſame proportion, as exibited in the following table:—It muſt be underſtood that the quantity of gas is to be meaſured *at the preſſure and temperature with which the impregnation is effected.*

The volume of every gas abſorbed by water is conſtant, and is either equal to the bulk or to the cube of a reciprocal of that bulk,

—equal preſſures and temperatures being ſuppoſed.

* Manchester Mem. N. S. Vol. I.

Bulk

Table of quantities.

Bulk absorbed, the bulk of water being unity. $\frac{1}{1^3} = 1$	Carbonic acid gas, sulphuretted hydrogen, nitrous oxide.*
$\frac{1}{2^3} = \frac{1}{8}$	Olefiant gas, of the Dutch chemists.
$\frac{1}{3^3} = \frac{1}{27}$	Oxygenous gas, nitrous gas,† carburretted hydrogen gas, from stagnant water.
$\frac{1}{4^3} = \frac{1}{64}$	Azotic gas, hydrogenous gas, carbonic oxide.
$\frac{1}{5^3} = \frac{1}{125}$	None discovered.

3. The gas thus absorbed may be recovered from the water the same in quantity and quality as it entered, by the means pointed out in the first article.

Water absorbs any gas in the same quantity, whether it contain another gas or not.

4. If a quantity of water free from air be agitated with a mixture of two or more gases (such as atmospheaic air) the water will absorb portions of each gas the same as if they were presented to it separately in their proper density.

Ex. gr. Atmospheric air, consisting of 79 parts azotic gas, and 21 parts oxygenous gas, *per cent.*

Water absorbs $\frac{1}{64}$ of $\frac{79}{100}$, azotic gas = 1.234
———— $\frac{1}{27}$ of $\frac{21}{100}$, oxygen gas = .778

Sum, *per cent.* 2.012

* According to Mr. William Henry's experiments, water does not imbibe quite its bulk of nitrous oxide; in one or two instances with me it has come very near it: The apparent deviation of this gas, may be owing to the difficulty of ascertaining the exact degree of its impurity.

† About $\frac{1}{20}$ of nitrous gas is usually absorbed; and $\frac{1}{27}$ is recoverable: This difference is owing to the residuum of oxygen in the water, each measure of which takes $3\frac{1}{2}$ of nitrous gas to saturate it, when in water. Perhaps it may be found that nitrous gas usually contains a small portion of nitrous oxide.

5. If

5. If water impregnated with any one gas (as hydrogenous) be agitated with another gas *equally* abſorbable (as azotic) there will *apparently* be no abſorption of the latter gas; just as much gas being found after agitation as was introduced to the water; but upon examination the refiduary gas will be found a *mixture* of the two, and the parts of each, in the water, will be exactly proportional to thoſe out of the water.

If water and gaſes be agitated in confinement, a mixture will take place of the gaſes in and out of the water, &c.

6. If water impregnated with any one gas be agitated with another gas leſs or more abſorbable; there will *apparently* be an increaſe or diminution of the latter; but upon examination the refiduary gas will be found a *mixture* of the two, and the proportions agreeable to article 4.

7. If a quantity of water in a phial having a ground ſtopper very accurately adapted, be agitated with any gas, or mixture of gaſes, till the due ſhare has entered the water; then, if the ſtopper be ſecured, the phial may be expoſed to any variation of *temperature*, without diſturbing the equilibrium: That is, the quantity of gas in the water will remain the ſame whether it be expoſed to heat or cold, if the ſtopper be air-tight.

Temperature does not affect ſuch incloſed fluids.

N. B. The phial ought not to be near full of water, and the temperature ſhould be between 32° and 212°.

8. If water be impregnated with one gas (as oxygenous), and another gas, having an affinity for the former (as nitrous), be agitated along with it; the abſorption of the latter gas will be greater, by the quantity neceſſary to ſaturate the former, than it would have been if the water had been free from gas.*

Gaſes which are diſpoſed to combine.

9. Moſt liquids free from viſcidity, ſuch as acids, alcohol, liquid ſulphurets, and ſaline ſolutions in water, abſorb the ſame quantity of gaſes as pure water; except they have an affinity for the gas, ſuch as ſulphurets for oxygen, &c.

The abſorption by other liquids is the ſame as by water.

The preceding articles contain the principal facts neceſſary to eſtabliſh the theory of abſorption: Thoſe that follow are of a ſubordinate nature, and partly deducible as corrollaries to them.

* One part of oxygenous gas requires 3.4 of nitrous gas to ſaturate it in water. It is agreeable to this that the rapid mixture of oxygenous and nitrous gas over a broad ſurface of water, occaſions a greater diminution than otherwiſe. In fact, the *nitrous* acid is formed this way; whereas, when water is not preſent, the *nitric* acid is formed, which requires juſt half the quantity of nitrous gas, as I have lately aſcertained.

Natural waters or rain contain the due share of atmos. air; but corrupt water has less or no oxygen.

10. Pure distilled water, rain and spring water usually contain nearly their due share of atmospheric air: if not, they quickly acquire that share by agitation in it, and lose any other gas they may be impregnated with. It is remarkable, however, that water by stagnation, in certain circumstances, loses part or all of its oxygen, notwithstanding its constant exposition to the atmosphere. This I have uniformly found to be the case in my large wooden pneumatic trough, containing about eight gallons, or $1\frac{1}{3}$ cubic foot of water. Whenever this is replenished with tolerably pure rain water, it contains its share of atmospheric air; but in process of time it becomes deficient of oxygen: In three months the whole surface has been covered with a pellicle, and no oxygenous gas whatever was found in the water. It was grown offensive, but not extremely so; it had not been contaminated with any material portion of metallic or sulphureous mixtures, or any other article to which the effect could be ascribed.* The quantity of azotic gas is not materially diminished by stagnation, if at all.—These circumstances, not being duly noticed, have been the source of great diversity in the results of different philosophers upon the quantity and quality of atmospheric air in water. By article 4, it appears that atmospheric air expelled from water ought to have 38 per cent. oxygen; whereas by this article air may be expelled from water that shall contain from 38 to 0 per cent. of oxygen. The disappearance of oxygenous gas in water, I presume, must be owing to some impurities in the water which combine with the oxygen. Pure rain water that had stood more than a year in an earthenware bottle had lost none of its oxygen.

Why water by agitation absorbs most oxygen from air.

11. If water free from air be agitated with a small portion of atmospheric air (as $\frac{1}{13}$ of its bulk) the residuum of such air will have proportionally less oxygen that the original: If we take $\frac{1}{13}$, as above, then the residuum will have only 17 per cent. oxygen; agreeably to the principle established in article 4. This circumstance accounts for the observations made by Dr. Priestley, and Mr. William Henry, that water absorbs oxygen in preference to azote.

Disappearance of gas by agitation under a jar.

12. If a tall glass vessel, containing a small portion of gas be inverted into a deep trough of water, and the gas thus confined by the glass and the water be briskly agitated, it will gradually disappear.

* It was drawn from a leaden cistern.

It

It is a wonder that Dr. Prieftley, who feems to have been the firft to notice this fact, fhould have made any difficulty of it;—the lofs of gas has evidently a mechanical caufe; the agitation divides the air into an infinite number of minute bubbles, which may be feen pervading the whole water; thefe are fucceffively driven out from under the margin of the glafs into the trough, and fo efcape.

13. If old ftagnant water be in the trough, in the laft experiment, and atmofpheric air be the fubject, the oxygenous gas will very foon be almoft wholly extracted, and leave a refiduum of azotic gas; but if the water be fully impregnated with atmofpheric air at the beginning, the refiduary gas examined at any time will be pure atmofpheric air. Old ftagnant water.

14. If any gas not containing either azotic or oxygenous gas, be agitated over water containing atmofpheric air, the refiduum will be found to contain both azotic and oxygenous gas. Agitation of gas over common water gives out oxygen and azote by mixture.

15. Let a quantity of water contain equal portions of any two or more unequally abforbable gafes: For inftance, azotic gas, oxygenous gas, and carbonic acid gas; then, let the water be boiled or fubjected to the air-pump, and it will be found that unequal portions of the gafes will be expelled. The azotic will be the greateft part, the oxygenous next, and the carbonic acid will be the leaft. For, the previous impregnation being fuch as is due to atmofpheres of the following relative forces nearly: The efcape of any gas from water by removing the preffure will be greater the lefs abforbable.

Azotic - - -	21 inch. of mercury.
Oxygenous - -	9
Carbonic acid -	$\frac{1}{7}$

confequently, when thofe forces are removed, the refiliency of the azotic gas will be the greateft, and that of the carbonic acid the leaft; the laft will even be fo fmall as not to overcome the cohefion of the water without violent agitation.

Remarks on the Authority of the preceding Facts.

In order to give the chain of facts as diftinct as poffible, I have not hitherto mentioned by whom or in what manner they were afcertained. Remarks on the laws of abforption of gafes by denfe fluids, &c.

The fact mentioned in the firft article has been long known; a doubt, however, remained refpecting the quantity of air ftill

Remarks on the laws of absorption of gases by dense fluids, &c.

left in water after ebullition and the operation of the air-pump. The subsequent articles will, I apprehend, have placed this in a clearer point of view.

In determining the quantity of gases absorbed, I had the result of Mr. William Henry's experience on the subject before me, an account of which has been published in the Philosophical Transactions for 1803. By the reciprocal communications since, we have been enabled to bring the results of our experiments to a near agreement; as the quantities he has given in his appendix to that paper nearly accord with those I have stated in the second article. In my experiments with the less absorbable gases, or those of the 2d, 3d, and 4th classes, I used a phial holding 2700 grains of water, having a very accurately ground stopper; in those with the more absorbable of the first class, I used an eudiometer tube, properly graduated, and of aperture so as to be covered with the end of a finger. This was filled with the gas and a small portion expelled by introducing a solid body under water; the quantity being noticed by the quantity of water that entered on withdrawing the solid body, the finger was applied to the end and the water within agitated; then removing the finger for a moment under water, an additional quantity of water entered, and the agitation was repeated till no more water would enter, when the quantity and quality of the residuary gas was examined. In fact, water could never be made to take its bulk of any gas by this procedure; but if it took $\frac{9}{10}$, or any other part, and the residuary gas was $\frac{9}{10}$ pure, then it was inferred that water would take its bulk of that gas. The principle was the same in using the phial; only a small quantity of the gas was admitted, and the agitation was longer.

There are two very important facts contained in the second article. The first is, that the quantity of gas absorbed is as the density or pressure. This was discovered by Mr. William Henry, before either he or I had formed any theory on the subject.

The other is, that the density of the gas in the water has a special relation to that out of the water, the distance of the particles within being always some multiple of that without: Thus, in the case of carbonic acid, &c. the distance within and without is the same, or the gas within the water is of the same density as without; in olefiant gas the distance of the particles

particles in the water is twice that without; in oxygenous gas, &c. the distance is just three times as great within as without; and in azotic, &c. it is four times. This fact was the result of my own enquiry. The former of these, I think, decides the effect to be mechanical; and the latter seems to point to the principle on which the equilibrium is adjusted.

Remarks on the laws of absorption of gases by dense fluids, &c.

The facts noticed in the 4th, 5th and 6th articles, were investigated *à priori* from the mechanical hypothesis, and the notion of the distinct agency of elastic fluids when mixed together. The results were found entirely to agree with both, or as nearly as could be expected from experiments of such nature.

The facts mentioned in the 7th article are of great importance in a theoretic view; for, if the quantity of gas absorbed depend upon mechanical principles, it cannot be affected by temperature in confined air, as the mechanical effect of the external and internal air are alike increased by heat, and the density not at all affected in those circumstances. I have tried the experiments in a considerable variety of temperature without perceiving any deviation from the principle. It deserves further attention.

If water be, as pointed out by this essay, a mere receptacle of gases, it cannot affect their affinities: hence what is observed in the 8th article is too obvious to need explanation.— And if we find the absorption of gases to arise not from a chemical but a mechanical cause, it may be expected that all liquids having an equal fluidity with water, will absorb like portions of gas. In several liquids I have tried, no perceptible difference has been found; but this deserves further investigation.

After what has been observed, it seems unnecessary to add any explanation of the 10th and following articles.

Theory of the Absorption of Gases by Water, &c.

From the facts developed in the preceding articles, the following theory of the absorption of gases by water seems deducible.

1. All gases that enter into water and other liquids by means of pressure, and are wholly disengaged again by the removal of that pressure, are *mechanically* mixed with the liquid, and not *chemically* combined with it.

2. Gases

Remarks on the laws of absorption of gases by dense fluids, &c.

2. Gases so mixed with water, &c. retain their elasticity or repulsive power amongst their own particles, just the same in the water as out of it, the intervening water having no other influence in this respect than a mere vacuum.

3. Each gas is retained in water by the pressure of gas of its own kind incumbent on its surface abstractedly considered, no other gas with which it may be mixed having any permanent influence in this respect.

4. When water has absorbed its bulk of carbonic acid gas, &c. the gas does not press on the water at all, but presses on the containing vessel just as if no water were in. When water has absorbed its proper quantity of oxygenous gas, &c. that is, $\frac{1}{27}$ of its bulk, the exterior gas presses on the surface of the water with $\frac{26}{27}$ of its force, and on the internal gas with $\frac{1}{27}$ of its force, which force presses upon the containing vessel, and not on the water. With azotic and hydrogenous gas the proportions are $\frac{63}{64}$ and $\frac{1}{64}$ respectively. When water contains no gas, its surface must support the whole pressure of any gas admitted to it, till the gas has, in part, forced its way into the water.

5. A particle of gas pressing on the surface of water is analogous to a single shot pressing upon the summit of a square pile of them. As the shot distributes its pressure equally amongst all the individuals forming the lowest stratum of the pile, so the particle of gas distributes its pressure equally amongst every successive horizontal stratum of particles of water downwards till it reaches the sphere of influence of another particle of gas. For instance; let any gas press with a given force on the surface of water, and let the distance of the particles of gas from each other be to those of water as 10 to 1; then each particle of gas must divide its force equally amongst 100 particles of water, as follows:—It exerts its immediate force upon 4 particles of water; those 4 press upon 9, the 9 upon 16, and so on according to the order of square numbers, till 100 particles of water have the force distributed amongst them; and in the same stratum each square of 100, having its incumbent particle of gas, the water below this stratum is uniformly pressed by the gas, and consequently has not its equilibrium disturbed by that pressure.

6. When water has absorbed $\frac{1}{27}$ of its bulk of any gas, the stratum of gas on the surface of the water presses with $\frac{26}{27}$ of its

its force on the water, in the manner pointed out in the last article, and with $\frac{1}{27}$ of its force on the uppermost stratum of gas in the water: The distance of the two strata of gas must be nearly 27 times the distance of the particles in the incumbent atmosphere, and 9 times the distance of the particles in the water. This comparatively great distance of the inner and outer atmosphere arises from the great repulsive power of the latter, on account of its superior density, or its presenting 9 particles of surface to the other 1. When $\frac{1}{64}$ is absorbed, the distance of the atmospheres becomes 64 times the distance of two particles in the outer, or 16 times that of the inner. The annexed views of perpendicular and horizontal strata of gas in and out of water, will sufficiently illustrate these positions.

Remarks on the laws of absorption of gases by dense fluids, &c.

7. An equilibrium between the outer and inner atmospheres can be established in no other circumstance than that of the distance of the particles of one atmosphere being the same or some multiple of that of the other; and it is probable the multiple cannot be more than 4. For in this case the distance of the inner and outer atmospheres is such as to make the perpendicular force of each particle of the former on those particles of the latter that are immediately subject to its influence, physically speaking, equal; and the same may be observed of the small lateral force.

8. The greatest difficulty attending the mechanical hypothesis, arises from different gases observing different laws.— Why does water not admit its bulk of every kind of gas alike? This question I have duly considered, and though I am not yet able to satisfy myself completely, I am nearly persuaded that the circumstance depends upon the weight and number of the ultimate particles of the several gases: those whose particles are lightest and single being least absorbable, and the others more, according as they increase in weight and complexity.* An enquiry into the relative weights of the ultimate particles of bodies, is a subject, as far as I know, entirely new: I have lately been prosecuting this enquiry with remarkable success. The principle cannot be entered upon in this paper; but I shall just subjoin the results, as far as they appear to be ascertained by my experiments.

* Subsequent experience renders this conjecture less probable.

Table

Weights of the particles of bodies.

Table of the relative weights of the ultimate particles of ga[illegible]
and other bodies.

Hydrogen	1
Azote	4.2
Carbon	4.3
Ammonia	5.2
Oxygen	5.5
Water	6.5
Phoſphorus	7.2
Phoſphuretted hydrogen	8.2
Nitrous gas	9.3
Ether	9.6
Gaſeous oxide of carbon	9.8
Nitrous oxide	13.7
Sulphur	14.4
Nitric acid	15.2
Sulphuretted hydrogen	15.4
Carbonic acid	15.3
Alcohol	15.1
Sulphureous acid	19.9
Sulphuric acid	25.4
Carburetted hydrogen from ſtagnated water	6.3
Oleſiant gas	5.3

VI.

On the ſuppoſed faſcinating Power of the Rattle-ſnake. With a remarkable Indian Tradition upon which it is probable the early European Settlers founded their popular Tales. From the Philadelphia Medical and Phyſical Journal, by BENJAMIN SMITH BARTON, *M. D.*

Faſcinating power of the rattle-ſnake deſcribed by Fabricius.

ALMOST all amphibious animals (ſays Profeſſor Fabricius,) the tortoiſe excepted, live by preying upon other animals. But being deſtitute of ſtrength and ſwiftneſs, nature has given, at leaſt to ſome of them (according to the teſtimony of many and creditable writers,) the peculiar faculty of forcing other animals to throw themſelves into their open jaws. Kalm, the Swede, and the American Smith Barton, aſſert of the American

American ſerpents, that if they fix their fiery, glaring eyes upon any animal, ſuch as a ſquirrel, or a bird, within a certain diſtance, they entirely loſe the power of eſcaping, but throw themſelves, ſlowly, irreſiſtibly, into the extended jaws of the ſnake. And if any thing diſturbs the ſnake, ſo that it withdraws its eyes but for one moment, they eſcape with the utmoſt precipitation.

We obſerve (continues this learned naturaliſt) ſomething ſimilar to this in our common, tardy, thick, and fat toads, which frequently ſit under little ſtones and buſhes, having their mouths wide open, into which flies, bees, and other inſects, are drawn in the ſame manner. All the theories that have hitherto been offered to explain theſe appearances appear to me both unnatural and improbable. Indeed, I cannot but doubt the reality of the fact itſelf, until we ſhall receive further obſervations and diſcoveries relative to it.

J. C. Fabricii, &c.
Reſultate Natur-Hiſtoriſcher Vorleſungen, p. 267, 268. *Kiel:* 1804.

It will be evident to any one, who has peruſed, with attention, my two publications * on the ſuppoſed faſcinating faculty of the rattle-ſnake, and other American ſerpents, that Mr. Fabricius has by no means fully comprehended my peculiar theory. I have not adopted the hypotheſis of the very reſpectable Kalm, with whoſe name mine is mentioned by the Daniſh Profeſſor. On the contrary, I have endeavoured to ſhow, and I flatter myſelf that I have very ſatisfactorily ſhown, that there is no ſolid foundation for the vulgar, and very generally-received opinion, that ſerpents are endued with the faculty of faſcinating, or charming, other animals. Annotation by Dr. Barton.

B. S. B.

The following very curious tradition of ſome of our Indians, relative to ſerpents, is worthy of publication in this place. A *part* of the tradition has already been publiſhed in my Narrative.

* A Memoir concerning the Faſcinating Faculty which has been aſcribed to the Rattle-ſnake, and other American Serpents. Philadelphia: 1796.—Supplement to a Memoir, &c. Philadelphia, 1800.—Or ſee Philoſ. Journal, Vols. VII. and VIII.

Supplement

Supplement to a Memoir concerning the Faſcinating Faculty which has been aſcribed to the Rattle-ſnake, and other American Serpents.

'Having queſtioned Indians, a number of times, with reſpect to ſnakes having the power of charming, and always being anſwered in the negative, I was at length deſired (ſays my friend, Mr. John Heckewelder) to give the reaſon the white people had for believing ſuch a thing, which not being ſatisfactory, Pemaholend* declared: "The rattle-ſnake obtains its food merely by ſlyneſs, and a perſevering patience. It knoweth as well where to watch for its prey as a cat does, and ſucceeds as well. It has, and retains its hunting grounds. In ſpring, when the warm weather ſets in, and the woods ſeem alive with the ſmaller animals, it leaves its den. It will croſs a river, and go a mile and further from its den, to the place it intends to ſpend the ſummer; and in fall, when all the young animals bred this ſeaſon are become ſtrong and active, ſo that they are no more ſo eaſily overtaken or caught, it directs its courſe back again, to its den, the ſame as a hunter does to his camp.

The rattle-ſnake catches its prey by craft and addreſs.

"The white-people," continued Pemaholend, "probably have taken the idea of this ſnake having the power of charming from a tradition of ours (the Indians) which our forefathers have handed down to us, from many hundred years back, and long before ever the white people came into this country. Then (they tell us) there *was* ſuch a ſnake, and a rattle-ſnake too, but then there was only *this one* ſnake which had this power, and he was afterwards deſtroyed; and ſince that time it hath never been ſaid that any other of the kind had made its appearance."

Indian tradition.

'At my requeſt, Pemaholend related the tradition, and in the following words. "Our forefathers have told us, that at a ſmall lake, or large pond, not a great diſtance from where, as is believed, now the great city *Quequenáku* (Philadelphia) is built, there dwelt a rattle-ſnake, whoſe length and thickneſs exceeded that of the thickeſt and longeſt tree in the woods. This ſnake was very deſtructive, not only in deſtroying ſo much game, but in devouring ſo many Indians: for when he was hungry, he only looked round, and whatever he

American native tradition about a rattle-ſnake.

* An aged and much reſpected Delaware-Indian.

American native tradition about a rattle-snake.

ſ, whether Indian, deer, turkey, or even geeſe flying, he ly held his head that way, opening his mouth wide, and awing breath in the manner we do, and nothing could prevent ſuch living creature entering his jaws. It is even ſaid, that a whole flock of geeſe, flying at a great diſtance, have been drawn into his mouth, at one time; * and it was well known among the Indians, that of all the hunters or travellers, who paſſed that way, very few eſcaped him.

"The Indians well knew when he was hungry, for then he grew angry, and blew with his mouth, which ſounded like thunder: for his breath was ſo powerful, that all the trees, however large, would bend, and even ſometimes break down before him. There being no proſpect of ever killing him with arrows, on account of the barrenneſs of the land far round the lake, into which he would always retire, after ſatisfying his hunger, a great council of the nation was called together, and the queſtion put, *Where are the Mannittoes of the nation? Are they no more? Shall the whole of the nation be deſtroyed by a Mannitto-Snake?* At length, two young men, endowed with *Mannittoic powers*, offered their ſervices, and declared, that unleſs the Mannittoie power of the ſnake exceeded theirs, they ſhould ſucceed; but they would, at all events, make an attempt. They then bid farewell to the aſſembly and their friends, dived into the river, from whence they proceeded under the water to a place oppoſite the Mennſippeek (lake, or large pond) where this ſnake dwelt. They made an opening under ground, from the river to the centre of the pond, by which the pond was drained, and became perfectly dry. After returning again, the ſame way they had come, they found the ſnake in great uneaſineſs, and on dry ground. Taking then the advantage of the dry weather, and the graſs far around the ſnake being dry, they ſet fire to the graſs, at a diſtance, and around the ſnake, by which means he was burnt

* It is curious, at leaſt, to compare this part of the Indian tradition with what Metrodorus, as cited by Pliny, relates of certain Aſiatic ſerpents. Theſe, he ſays, by means of their breath, attracted birds, however high they were, or however quick their flight. "Metrodorus, circa Rhyndacum amnem in Ponto, ut ſupervolantes quamvis alte perniciterque, alites hauſtu raptas abſorbeant." Plin. Hiſt. Nat. lib. viii. cap. 14.

to

American native tradition about a rattle-snake.

to death.* Thus (continued Pemaholend) was the monster killed by two mannitto men of the nation: for, you must know, in those days, we had such men among us, who could live as well in the water as on land."

'Conversing one day with a Monsy (advanced in years) on ancient times, on the migration of the Indians, &c. he, in order to convince me (says Mr. Heckewelder) what the Indians once were, mentioned the killing of the big snake, the history of which, according to his relation, differing only in the following points:

"*a.* He did not think it had been a rattle-snake, but understood the old men, from whom he had heard it so often related (when he was young), that it was a snake of a peculiar kind, and had feet; and that never since had a snake of this kind appeared:

"*b.* That he was not sure as to the place where this snake kept; believed it had been higher up the country, and kept in a wide and deep place of the river, and in the country of the Munsees (or Minsy) and was killed by a Mannitto Munsee:

"*c.* That after the nation had met in council, and the above questions put, a Munsee man of no character, nor seemingly of any consequence to the nation, said and declared, that he had *Mannittoic Powers;* could and would destroy the monster, prescribing the ceremonies the assembly were to observe during the expedition. That he then made a very strong arrow, or spear, sharp at both ends; and being equipped, took leave of the assembly—plunged into the river, and dived under water, until he arrived within a small distance of the place where the snake lay, or floated, basking in the sun. Here he ascended to the surface, and calling out to the snake to receive him, he opened his mouth wide, and drew him in, when, however, in an instant, the snake was stabbed by him through both his sides, with the spear, which wounded him so deadly, that he gave a whirl, and being under great pain, discharged his excrements, and with the same this hero, who

* Even this part of the Indian tradition seems to be borrowed from the old world. See a curious relation of the capture of an enormous serpent in *The Life of Sethos, as taken from private memoirs of the Egyptians.* Vol. i. p. 125—147. London, 1737.

then

then swam again to shore, announcing his victory, and congratulating the assembly on the deliverance of the nation.

"Thus (continued the old Munsee) were the Indians of those days *Mannittoes*. Nothing could resist them. They knew nothing of drowning. Our first Parents have sprung from the bottom of a lake."

VII.

A Description of a Property of Caoutchouc, or Indian Rubber; With some Reflections on the Cause of the Elasticity of this Substance. In a Letter to Dr. HOLME.*

SIR, *Middleshaw, near Kendal, Nov.* 26, 1802.

THE substance called Caoutchouc, or Indian Rubber, possesses a singular property; which, I believe, has never been taken notice of in print, at least by any English writer; the present letter contains my experiments and reflections on the subject; and should they appear to deserve the attention of your philosophical friends, I am certain you will take the trouble of communicating the paper to the Literary and Philosophical Society of Manchester.

The property I am about to describe depends on the temperature of the Caoutchouc, which is used in the experiment; for heat increases the pliancy of the substance, and cold, on the contrary, renders it more rigid: so that when a slip of this resin has been sufficiently warmed, it may be extended to more than twice its natural length, by a moderate force applied to its extremities, after which it will recover its original dimensions in a moment, provided one of the ends of it be let go as soon as it has been stretched. This disposition of the substance may be produced by a degree of temperature less than the heat of the blood; it is therefore necessary to prepare a slip of it, by steeping it for a few minutes in warm water, or by holding it somewhat longer in the fist; either of these precautions makes the resin pliant, and fits it for the experiment; which is performed in the following manner.

Caoutchouc more pliant by heat,

* Manchester Mem. N. S. Vol. I.

I made

and less dense. I made a piece of Caoutchouc a little heavier than an equal bulk of water, the temperature of which was 45 degrees: the vessel containing the resin and water was then placed on the fire; and when the contents of it were heated to 130 degrees, the Caoutchouc floated on the surface.

It becomes cold by sudden drawing out, and hot by contraction. *Exp.* 1. Hold one end of the slip, thus prepared, between the thumb and fore-finger of each hand; bring the middle of the piece into slight contact with the edges of the lips;* taking care to keep it straight at the time, but not to stretch it much beyond its natural length: after taking these preparatory steps, extend the slip suddenly; and you will immediately perceive a sensation of warmth in that part of the mouth which touches it, arising from an augmentation of temperature in the Caoutchouc: for this resin evidently grows warmer the further it is extended; and the edges of the lips possess a high degree of sensibility, which enables them to discover these changes with greater facility than other parts of the body. The increase of temperature, which is perceived upon extending a piece of Caoutchouc, may be destroyed in an instant, by permitting the slip to contract again; which it will do quickly by virtue of its own spring, as oft as the stretching forth ceases to act as soon as it has been fully exerted. Perhaps it will be said, that the preceding experiment is conducted in a negligent manner; that a person, who wishes for accuracy, will not trust his own sense of feeling in inquiries of this description, but will contrive to employ a thermometer in the business. Should the objection be started, the answer to it is obvious; for the experiment in its present state demonstrates the reality of a singular fact; by convincing that sense, which is the only direct judge in the case, that the temperature of a piece of Caoutchouc may be changed, by compelling it to change its dimensions. The use of a thermometer determines the relative magnitudes of these variations, by referring the question of temperature to the eye; experiments of this sort are therefore of a mathematical nature, and afford a kind of knowledge with which we have nothing to do at present; for we are not inquiring after proportions, but endeavouring to estab-

* This effect was first noticed in 1784, at Mr. Kirwan's meetings in Newman street, and Dr. Crawford ascribed it to change of capacity similar to what he supposed to take place in a nail by hammering.—N.

blish

blish the certainty of a fact, which may assist in discovering the reason of the uncommon elasticity observable in Caoutchouc. My essay or letter appears to be running into a long digression; the subject must therefore be resumed, and it will not be improper to premise the following simple experiment, in the present state of the inquiry; because it seems capable of affording no inconsiderable degree of insight into the plan which nature pursues in producing the phenomenon in question.

Caoutchouc when stretched expands by heat and contracts by cold.

Exp. 2. If one end of a slip of Caoutchouc be fastened to a rod of metal or wood, and a weight be fixed to the other extremity, in order to keep it in a vertical position; the thong will be found to become shorter with heat and longer with cold. The processes of heating, cooling, and measuring bodies are so well known, that I need not enter into the minuter parts of the experiment; it will be proper, however, to add, that an increase of temperature diminishes the specific gravity of the Indian Rubber, and a loss of heat occasions a contrary effect in it; as I have proved experimentally. The knowledge of the latter fact leads me to conclude, apparently on reasonable grounds, that the pores or interstices of Caoutchouc are enlarged by heat, and diminished by cold; consequently when a slip of this substance which remains extended by a weight, or the application of force, happens to contract from an accession of temperature, the capacity of its pores, taken separately or collectively, is augmented by the change that takes place in the figure of the thong. Now if the existence of caloric be admitted, it will follow from the preceding arguments, that the phenomenon under consideration is occasioned by the alternate absorption and emission of the calorific fluid, in the same manner that ropes, the blades of Fuci, as well as many more bodies, are obliged to contract and extend themselves, by the alternate absorption and emission of water.—You will perceive by the tenour of the foregoing observations, that my theory of this case of elasticity is perfectly mechanical; in fact, the explanation of it depends upon the mutual attraction of Caloric and Caoutchouc; the former of which penetrates the latter, and pervades every part of it with the greatest ease and expedition; by which the resin is compelled to accommodate its pores to that portion of the Calorific fluid which is due to its whole mass, at

Theory. That this substance is affected by caloric, as ropes are by water;

at any particular degree of temperature. In order to apply the last remark to the phenomenon under consideration I may observe, that if a force be exerted on a piece of Caoutchouc to alter the dimensions of its pores, the mutual attraction mentioned above will resist the effort. But the ease with which this substance may be made to change its figure, and the retractile power which it possesses on these occasions, shew that its constituent particles move freely amongst themselves: but where there is motion, there is void space; consequently Caoutchouc abounds with innumerable pores or interstices, the magnitudes of which are variable, because the specific gravity of the resin becomes less with heat, and greater with cold. Now if the dimensions of the pores in a piece of Caoutchouc can be lessened, without taking away part of the matter of heat, which it contains at the time; this new arrangement in the internal structure of the slip will lessen its capacity for the matter of heat, and consequently augment its temperature. But the warmth of such a slip is increased by stretching it, according to the first experiment; the pores of it are therefore diminished: and the effort, which it exerts at the time, arises from the mutual attraction of the Caoutchouc and Caloric; which attraction causes an endeavour to enlarge the interstices of the former for the reception of the latter; hence it happens that the thong contracts longitudinally, according to the second experiment, and the redundant caloric is absorbed in the course of this operation, which again reduces the temperature. The preceding explanation agrees very well with the phenomenon, as it is stated in the beginning of this letter; and the theory receives additional confirmation from the following facts.

that its capacity may be mechanically altered and the caloric extruded, &c.

Exp. 3. If a thong of Caoutchouc be stretched in water warmer than itself, it retains its elasticity unimpaired; on the contrary, if the experiment be made in water colder than itself, it loses part of its retractile power, being unable to recover its former figure; but let the thong be placed in hot water, while it remains extended for want of spring, and the heat will immediately make it contract briskly. The foregoing circumstances may be considered as proving, that the elasticity of Caoutchouc is not a constitutional quality of the substance, but a contingent effect, arising from the loss of equilibrium between the portion of caloric, which the resin

Overstretched Caoutchouc does not completely recover itself in the cold; but heat restores its elasticity:

whence the nature of its elasticity is deduced, &c.

fessin happens to contain at any moment, and its capacity to receive that fluid at the same instant. The object of the present letter is to demonstrate, that the faculty of this body to absorb the calorific principle, may be lessened, by forcibly diminishing the magnitudes of its pores; and this essential point of the theory may be confirmed by experiment: for the specific gravity of a slip of Caoutchouc is increased, by keeping it extended, while it is weighed in water.

JOHN GOUGH.

VIII.

Observations on the training of Pugilists, Wrestlers, Jockies, and others, who give themselves up to Athletic Exercises; with some Queries for discovering the Principles thereof, and the Process of training Running Horses, &c. with a View of ascertaining whether the same can furnish any Hints serviceable to the Human Species. *

PROFESSIONAL men are ready to acknowledge, that prevention is better than cure; and the best informed ingenuously admit, that organic diseases, once confirmed, are beyond the reach of their art. As organic diseases generally proceed from slow and gradual changes, they may certainly be prevented by temperance and labour; by activity of body, and contentment of mind. In regard to the common metaphysical expressions, "of the exhausting of the excitability; of the wearing of the parts; of the attrition of our fluids, in circulation, against the solids; of the abrasion of the solids by friction;

General considerations on organic diseases.

* The subsequent queries and observations have been circulated by Sir John Sinclair, with a view to obtain information concerning the effects of diet and exercise on the human frame, from a class of practical experimentalists, whom the pride of science has hitherto overlooked. The philosophical manner in which this branch of diætatic medicine is here considered, appears to render it a fit object for insertion in a Journal conducted on the plan of the present. In promoting the circulation of this paper, we have no doubt that we are coinciding with the plan of the author, by extending his means of information: Any communications tending to throw further light on the subject, will be acceptable. W. N.

tion; of the debility produced by the most natural powers supporting life, namely, the waste of substance created by that exercise and labour, for which we seem peculiarly destined,"—all these expressions are extremely suspicious. The speculator is always to be suspected, when, forsaking plain direct facts, he involves his want of meaning, and conscious ignorance, in learned words, or metaphor.

It is usually supposed that bones, and other solid parts are permanent; but they are successively replaced, like the fluids.

These metaphorical expressions have originated in a persuasion, that the bones, cartilages, muscles, and other solid parts, being once formed, *are permanent,* because the identity of the individual is permanent; and that being once formed, and always retaining one shape, their actual component parts must continue the same. Nothing in philosophy is farther from the truth. There are experiments to demonstrate, that every part and particle of the firmest bones, is successively absorbed and deposited again*. The solids of the body, whatever their form or texture, are incessantly renewed. The whole body is a perpetual secretion, and the bones and their ligaments, the muscles and their tendons, all the finer and all the more flexible parts of the body, are as continually renewed, and as properly a secretion, as the saliva that flows from the mouth, or the moisture that bedews the surface. The health of all the parts, and their soundness of structure, depends on this perpetual absorption, and perpetual renovation; and exercise, by promoting at once absorption and secretion, promotes life, without hurrying it; renovates all the parts and organs, and preserves them apt and fit for every office.

Nutrition is a general process.

Nutrition belongs not to the stomach alone, which but prepares the food, and converts it into chyle, but to the vessels by which it is circulated, and appropriated to the nutrition of parts, which of course is performed by every petty artery of the body.

Many general rules are rash and dangerous.

In nothing should we be more anxiously careful, than, in laying down rules, which must affect the health of thousands; and whenever we proceed on doctrines, unsupported by fact,

* This has been ascertained by giving madder to growing animals, especially pigs and fowls, among their food. It is found that the madder tinges the bones, layer after layer, with a red colour; and by the deepness of the tinge, demonstrates the succession in which the particles of the bone are absorbed and deposited. This is, I believe, the conclusion which physiologists have formed.

wherever

wherever we divert mankind from those amusements and labours to which nature excites us, we should proceed with particular caution. We read in books, that life and the body are but as a given quantity of living energy and living materials, to be expended and used with discretion and economy; and that the sum of excitability, which is born with the child, is expended towards the close of life. The doctrine of abrasion also intimates, that our solids are perpetually wasting, and that it is by the diminution of moisture,—the aridity of solids, the scantiness of fluids, and the slow induration of the solid parts; that the body becomes shrunk, emaciated, stiff, and motionless, before it sinks into the grave. And, rash as the doctrine seems, it has been boldly asserted, that "to live with as little food, and as little exercise as possible, is the surest means to preserve the body, and to live long." To live with as little food, and as little exercise as possible, would make a man little better than a mere grasshopper. A man living thus, would be a voluntary prisoner, wan, colourless, fleshless, bloodless, having no speculation in his eyes, no marrow in his bones; his complexion would declare him what he was. This system practised, either in infancy, in the prime of manhood, or in the decline of life, would abridge it. Ascetics are a proof, not of the length of life, which temperance insures, but of the premature old age which abstinence brings upon us. The squalid look, the hollow cheek, the matted hair, the emaciated body, only prove how much, by such criminal self-denial, the body suffers, with but little profit to the powers of the mind. Let us then take care that our philosophy be not too severe; for men may run into real danger, if we take from them every fair indulgence, or divert them from following the dictates of nature. The fairest livers, who have not abused, but have enjoyed their strength and health, have in general enjoyed them longest.

The doctrine of abrasion or wearing out has been absurdly applied.

There are habits which seem to be natural to, and congenial with, the several periods of life. The child should merely suck, sleep, and vegetate. The boy should ramble wild and unconstrained, little oppressed with tasks or studies, and nourished with abundance of simple food. The youth should be temperate, sober, active. The old man quiet, sedate, self-indulgent; should have long sleep, delicate food, rich wines, and agreeable temperature; little labour, and a cheerful mind.

Natural habits of infancy, youth, &c. &c.

Nature affigns us vigour, fpirit, enterprife, and forefight the early part of life, to treafure up the needful indulgences for age. Parents are careful of our firft infancy; we ourfelves ought to provide for our latter childhood.

Confiderations refpecting the functions of the fkin;— The moft intelligent profeffional men have an opinion concerning the functions of the fkin, confonant with that of the vulgar; and more refined, only from their affigning a general caufe for thofe effects, of which all of us are confcious. The fkin is not regarded merely as an organ of fecretion, deftined for draining off fuperfluous moifture, or faline particles, from the general mafs of fluids, but as a furface of more active circulation, which folicits the blood to the very extremities of the veffels, and thus contributes to fupport and complete the circulation of the blood, and to nourifh the parts within. The fkin is regarded as connected, in a peculiar manner, with all the parts of the cellular fubftance, interpofed betwixt the mufcles, and involving the blood veffels. The ftate of the fkin indicates the condition of that cellular fubftance, whofe office it is to conduct the blood-veffels to all parts, efpecially to the mufcular flefh, and to nourifh the parts; and while the circulation of the fkin is lively and active, that of the involved parts can never flag. The condition of the bowels, and of the fkin, are the firft and moft natural points for the phyfician to attend to. It is by regulating thefe, that he regulates the pulfe; by ftimulating or foothing them, that he raifes or depreffes the vital actions; and it is matter of common obfervation, that in animals, a good fkin is the criterion of health, and the drynefs of the fkin, the forming of fcabs or eruptions upon it, and the clapping of the hair, (as it is called by thofe who have the care of ftock), are the firft and fureft figns of approaching difeafe.

—and the inteftines.

The lungs and their office.

Next to the free circulation of the blood through all the body, terminating in the furface, that of the free tranfit of the blood through the lungs, is effential to health.

The oxydation or chemical change produced by air upon the blood, is effential to its vital properties. A free and powerful refpiration is moft effential to a frefh colour of the face, to lively fpirits, and cheerful feelings, and to the healthy and vigorous actions of the body. "It is my breathing hour of the day," fays Hamlet to Ofric. It is a princely thing to fet apart hours for exercifes; and there is little doubt, that if all

thofe

thoſe, who linger away their hours in luxurious and indolent relaxations, were to aſſign a regular portion of their time to the hardy and manly exerciſes of walking, riding, fencing, &c. and would take *their breathing hour*, they would breathe long and well.

The art of training men to athletic exerciſes is wonderfully effective.

Theſe reflections naturally ariſe upon conſidering the almoſt incredible perfection, to which thoſe, whoſe profeſſion it is to train men to athletic exerciſes, have brought their reſpective arts. By certain proceſſes, they improve the breath, the ſtrength, and the courage of thoſe they take in hand, ſo as to enable them to run thirty, or walk a hundred miles, in a given ſpace of time; to excel in wreſtling; or to challenge a profeſſed boxer. Would it not then be a moſt important addition to the facts we already know concerning the means of improving ſtrength, and enſuring long life, if authentic information could be procured from thoſe diſtricts where athletic exerciſes prevail, what are eſteemed the beſt and ſureſt proceſſes for training men for foot-races, trials of ſtrength in wreſtling or boxing matches, or for raiſing the ſtrength and courage of game-cocks, or improving the wind, ſtrength, and ſpeed of running horſes to their higheſt pitch.*

Some account of the methods.

Thoſe who give themſelves out as ſkilful in this art, attend to the ſtate of the bowels, the ſkin, and the lungs. They uſe ſuch means as reduce the cellular or fatty ſubſtance, and invigorate the muſcular fibres. When they take a man in training for any feat of this kind, he is not oiled and ſuppled as the ancient athletics were; for as their common modes of life were hardy and active, they needed no other preparation: but he is ſweated, purged, and dieted, and then put upon trial. He is purged with very draſtic purges, to reduce his groſſneſs. He is made to walk out under a load of clothes; his walks are regularly increaſed, and a certain number of times a-week; he is laid between two feather-beds; ſweat promoted by drinks; his limbs taken from between the feather-beds, ſucceſſively, and rubbed very roughly. After enduring for many

* Though not immediately connected with the object of this paper, it may not be improper to ſuggeſt, that it would be of great importance, if medical gentlemen, whether in the army or navy, who have been on ſervice, were alſo to point out the various circumſtances which tended to ſupport, or to abate, the ſtrength and courage of the ſoldier or the ſailor.

hours

hours this ſtate of ſuffocation, he is comforted with a draught of ale or wine. The purges and ſweatings are repeated, according to the groſſneſs of his habit, and from time to time his trainer, (regarding him no otherwiſe than he would a running horſe, under the like diſcipline,) takes him out, and makes trial of his wind and ſtrength, and does not ceaſe till he has made him as lank as a greyhound, and almoſt as fleet.

and the great increaſe of force acquired by the human frame.

A man, even in the beſt of ordinary health, becomes giddy and breathleſs when he ſtrikes; and ſick and pale on receiving a few blows. He is thence unable to bear any unuſual exertion, and by inference prone to diſeaſe. If, by extenuating the fat, emptying in the cellular ſubſtance, hardening the muſcular fibres, and improving the breath, a man of the ordinary frame may be made to fight for one hour, with the utmoſt exertion of ſtrength and courage; the inquiry which I have already ſuggeſted muſt be of the higheſt uſe. For were this new train of facts regularly laid before profeſſional men, and were they enabled thus to judge of the influence which the methods of theſe practical philoſophers have on regulating the functions of breathing, perſpiration and digeſtion; it would be drawing into the province of ſcience, an art connected moſt particularly with the means of prolonging life, and hitherto known and practiſed only by a few inſulated individuals, of courſe imperfectly known, and of too limited uſe.

Theſe facts are probably of great value to the ſcience of prolonging life.

The art ſeems to be modern.

I queſtion whether the athletics of old uſed ſimilar means; whether they were equally ſucceſsful; whether there ever were, in any climate, age, or country, more hardy or powerful frames than thoſe of our Engliſh pugiliſts. In Cooke's voyage, we are told of the marked inferiority of the Engliſh ſailors, in wreſtling or boxing, to the naked ſun-burnt heroes of the South Sea Iſlands. But an Engliſh ſailor, though full of ſpirit and vigour, is as clumſy as a clown, and could not even row againſt an inhabitant of the Sandwich Iſlands. An Engliſh bricklayer, blackſmith, or drayman, however, who liked the ſport, and was practiſed in balancing and ſtriking, might have challenged the whole of the tawny nation.

Queries.

With a view of collecting ſuch important information, I am very anxious that the following queries ſhould be propoſed to thoſe who profeſs the art of training pugiliſts, wreſtlers, and runners of foot-races, by ſuch intelligent men as have the opportunity of converſing with them.

1. By

1. By what criterions or tefts, they judge of the mufcular ftrength, or wind, or other qualities of thofe who feek to put themfelves under training. What is the earlieft, and what is the lateft age they would attempt to train? Tefts of ftrengt &c.?

2. How they judge of the length of time that may be required for bringing a man into good plight, vigorous health, and free breathing; and what period of preparation is ufually required for running a match? Time required to train?

3. What purges they ufe; and in what fucceffion; and by what rules do they adminifter them; and how do they judge of their effects? Is the purging only preparatory, or is it regularly continued? Is it meant, by this procefs to reduce the plethoric ftate of the fyftem, (on the idea that there is too great a quantity of blood,) or is it fimply defigned to put the bowels in the moft favourable condition, for eafy and good digeftion? Is the reducing the actual fize of the belly, neceffary to more free and perfect breathing *? Purges, treatment, their object, &c.?

4. Is the diet rich or fimple; of animal food, or of vegetable; in great quantity, or fparing; is it increafed gradually, or diminifhed gradually? What meals have they in the day; and at what hours; one or more; frequent feeding, in fmall and fixed portions, or full and fubftantial meals? What kinds of flefh or meat is reckoned the beft; whether beef, mutton, veal, pork, lamb, or fowl? Are any kinds of fifh allowed? What quality of food is moft conducive to ftrength? What quantity is neceffary for maintaining the fyftem in its moft perfect ftate of vigour? Do they feed much in the intermediate days of the purges? Is abftinence required when they take their phyfic? Diet?

5. What kinds of liquors are reckoned beft? Whether wine, ale, water, fpirits, &c.? Whether given hot or cold; in what quantities; and when ought they to be given? Liquors?

* The effects of taking up a running horfe from idlenefs and foft pafture, to hard food and regular exercife, is attended with this peculiar effect, that while the animal becomes lank, fleek, and gloffy, while he gets fire in his eye, and a new vigour in his limbs, and wind and fpeed, his belly, (fwollen with coarfe indigeftible food, eaten in great profufion,) is drawn into half its fize. May we not then prefume from this analogy, that the ftate of the belly has a remarkable effect upon the wind.

Intention of the perſpirations, how excited, &c. ?

6. Are the very violent perſpirations into which they throw their patients, deſigned to reduce the ſyſtem, to extenuate the fat, to leſſen that quantity of blood, the exceſs of which makes us giddy or ſhort breathed; or is it merely deſigned to produce a new condition of the ſkin, more favourable to health and muſcular vigour; to produce a ſharper appetite; a greater demand for food; and a quicker nouriſhment, or a greater nutrition from a more ſlender diet? Is the ſweat at firſt produced by exerciſe, and only continued by the perſon, when trained, being put between feather beds, and encouraged by drinks; or is it produced by force of ſweating drugs, or violent heats, or by continued friction? At what hours are the perſpirations brought on? How is the pupil treated when the ſweat is over? What becomes of the ſkin of a fat man, when, by the proceſs, he is reduced in ſize, and rendered lean? Does it hang looſe, or is it tight? Has it any effect upon the bones?

Exerciſe and treatment?

7. What hours of exerciſe do they require of their pupils during the day? At what hours do they ſend them out in the morning? How long do they continue abroad? Are they loaded with clothes after the body is reduced, and becomes limber, and thin and muſcular; or only while the ſweating proceſs continues? Are they fed before they go abroad, or when they return? What trials are made of their ſtrength? When is a man known to be up to his full ſtrength and breath in training? At what hours do they go to bed? What ſleep are they allowed? What indiſpoſitions are they ſubject to during training? Are there any circumſtances by which the proceſs may be interrupted; or any circumſtances, in conſequence of which, it muſt ſometimes be abandoned?

Subſequent effects of training?

8. What is the ſtate of the health, after they give up training? Are they ſubject to any complaints; and what are they? How long does the acquired exceſs of ſtrength continue?

What part of training moſt effectual? Whether it be permanent, temporary, curative, &c.

9. It is moſt intereſting to learn, on which part of this proceſs, the purging, the ſweating, the exerciſe, or the feeding, they moſt depend; and whether it procures a permanent increaſe of vigour, eaſily maintained by ſuitable diet and exerciſes, or only a temporary excitement, calculated for the particular occaſion? Alſo, whether perſons have ever thought of undergoing this proceſs, not for the purpoſe of running matches, but to recover health; with what ſucceſs

this

this has been done, and whether it is to be recommended for gout, corpulency, asthma, nervous disorder, or other maladies, as likely to be of service?

These are questions, of the importance of which, those who are best able to answer, may not be fully aware. But nothing which so suddenly changes the powers, and the very form and character of the body, from gross to lean, from weakness to vigorous health, from a breathless and bloated carcase, to one active and untiring, can ever be unimportant, either to the art of physic in general, or to that branch of it more immediately connected with inquiries regarding health and longevity. The art must be of importance.

The queries to be put regarding jockies, running-horses, or game-cocks, may be to the following effect:

1. *Jockies.*

1. What is the process used in training them, and reducing their weight? Queries respecting Jockies.

2. What effect has it upon their health and strength?

3. What effect has it upon their mind, in regard to courage, quickness, &c.

4. How long do these effects continue?

5. After being reduced, do they quickly get fat again, or do they continue long in the state to which they were brought?

6. Are jockies, accustomed to be thus treated, healthy and long lived?

2. *Running Horses.*

1. What are the principal objects to be attended to in regard to running-horses? Do their perfections depend upon parentage, and whether most upon the male or the female? Is it necessary that the mare should have gone her full time, to bring a perfect foal? Is the gradual growth of the foal essential? Is there a great difference, in regard to natural constitution, between horses of the same parentage? What kind of form is in general preferred? Do you prefer great or small bones? Which sex is preferable for speed, and which for strength? Running horses

2. What is the best age for beginning to train horses for the turf? Are they first put upon grass? What is the effect of soft

soft meat? When should they be put on hard meat? What are the effects thereof? Is it necessary to purge them frequently? Have the purges any tendency to weaken them? What food is reckoned the most nourishing? How often are they fed? What drinks are given them, and how often? Whether hot or cold? Is it necessary to keep their skin perfectly clean, and how? Is it necessary to make them perspire much? What exercise is given them? How is the training completed?

3. After the training is completed, can the perfections thereby obtained be easily kept up? Does the process effect merely a temporary change, or does it last during life? Are running horses as long lived as others, or do they soon wear out?

3. *Game-Cocks.*

Game-cocks. 1. Does the superiority of game-cocks depend upon parentage? Which is of most importance, the male or the female? Is it of any consequence that the cock should arrive rather gradually at maturity? Is there a great difference, in point of strength and constitution, in game cocks of the same parentage? Do you prefer great or small bones?

2. When do you begin to feed the young cocks? What diet and drink do you give them, and what is the process by which they are brought to the greatest possible height of strength and spirit?

3. When the game-cocks are thus trained, how long do the effects thereof last? Are they temporary or permanent? Do game-cocks thus trained live shorter or longer than others of the same species?

4. What drugs are given to fighting-cocks immediately before the main begins? Is it not usual, by giving them saffron, (or some drug which has the same effect with opium, as used among the Janisaries, or brandy among the French soldiery,) to excite an unnatural and short-lived courage? What are the effects of such drugs? and how do they manage the feeding up to this point, so as to take advantage of this momentary excitement?

IX. On

IX.

On the Dangers encountered in travelling over Downs, occasioned by Quicksands, which are frequently found on the Sea Coast; with an Indication of the Means of avoiding them. By M. BIEMONTIER, *Inspector-General of Bridges and Roads.* *

AFTER heavy and continued rains, there are formed at the edge of the sea-downs, small pools, or collections of water, frequently of several feet in depth. Strong winds dislodge portions of sand from the general mass, and transport them to a distance; which falling in showers on the clayey and sheltered surface of these pools, descend gradually, and remain as it were in equilibrium in the midst of the water, so as to form an infinity of little vaulted cavities. These arches sustain others, which are again surmounted in a similar manner, till at length the mass rises, sometimes to several feet above the level of the water; the surface becomes white and dry, and the snare lies perfectly concealed. Whoever walks over this structure destroys the whole, the arches give way, and the intruder is immersed sometimes to his waist; but his alarm is usually greater than the real danger; for if he were buried even up to the neck, he might easily extricate himself, only by retaining sufficient presence of mind not to struggle, but to move slowly and deliberately; want of attention to this might hazard his destruction.

Quicksands formed by bodies of sand transported by the wind into pools of water, where they form vaulted cavities till the hollow of the pool is filled up.

The surface is dry and scarcely differs in appearance from the contiguous ground, but it gives way when trod upon.

When the equilibrium of the masses of sand is destroyed, they naturally fall into heaps, and it is only necessary that time should be allowed for this to take place. When this has happened, the person immersed should gently lift up one leg, and remain in that position till the sand has formed a sufficient bottom to support his raised foot; the other leg should then be lifted up with the same precautions; and thus successively, till he rises to the surface. In the mean time, the water which had been confined in the hollows of the sand will have also risen, forming a pond (three or four inches deep) through which the adventurer may pass in perfect safety.

Management to avoid danger.

* Bibliotheque Physico-Economique, &c. de Sonini, November, 1805, page 186.

Cows,

Animals when immerfed ufe the fame method;

Cows, dogs, and other animals who frequent downs, and chance to fall into thefe quickfands, either through inftinct or experience, make ufe of this method to regain their freedom; provided, however, they be not too deeply immerfed to retain the free ufe of their fhoulder joints, otherwife they cannot be extricated without affiftance. I experienced this twice in one day; my horfe fank to above the breaft-leather, and although he was very ftrong, his efforts to extricate himfelf were unavailing, till we had removed fo much of the fand as impeded the action of thofe joints.

but they are feldom caught in quickfands.

It rarely happens that animals accuftomed to live on downs are caught in thefe fnares, which they are aware of, and know how to avoid.

Inftance,

I attempted, but in vain, to force another horfe with the whip and fpur into a quickfand; his owner, who acted as guide, affured me, that I fhould not fucceed, although there was no other indication of the fpot than a flat furface, flightly wrinkled. By thefe marks the traveller may generally detect the concealed pitfall; but he may always avoid them by tracing the footfteps of the cattle, when vifible, or by walking a few fathoms above the bottom of the declivity, or on the fummit of the down.

Another kind of quickfand,

Another kind of quickfand is fometimes met with on the fea-fhore, between high and low water mark, which it is proper fhould be here taken notice of. This is fometimes the effect of rain, but more commonly of the fea, when forced by wind and tempeft beyond its ufual limits, which being generally more elevated than the diftant land, the waters thus impelled forward are prevented from returning to their ancient bed; they therefore after forming in a body, drain away through the earth they have inundated, or brought with them, and form excavations beneath, large or fmall, deep or fhallow, according to circumftances.

formed by the waters drained through the earth.

I ought not to omit a fingular fact which paffed under my own obfervation, and which feems to prove, (as I have already ftated) that animals frequenting thefe plains, and living near the borders of the fea, employ combined means, acquired undoubtedly by experience, to extricate themfelves from thefe cavities, wherein they muft inevitably perifh, did they, as it appears natural they fhould, attempt to efcape by recoiling or by flight.

2 Traverfing

Traverfing the plain of Arcachon, after a violent tempeft, which had been accompanied with heavy rains, we thought it prudent to get off our horfes and lead them by the bridle. One of the horfes who was left to himfelf, immediately quitted the company; and was retiring from the fhore, but being compelled to return by the application of the whip, he went upon the quickfand, which probably he had attempted to avoid by defertion; but the moment he felt the earth giving way, he crouched down, or rather threw himfelf precipitately on his fide. The ground quickly fank beneath and round about him; the water furmounted the fand; the horfe was only wetted to the crupper, and we efcaped with no other damage than the lofs of our ftock of bread, which being foaked in the falt water was rendered unfit to be eaten. Singular incident.

It may be received as matter of fact, that a man who fhould experience a fimilar misfortune, could not do better than to extend himfelf in the like manner, nearly in the attitude of a fwimmer, when he throws himfelf into the water. It is fcarcely neceffary to explain the fuperior advantage of this method; a plate of lead, of fome thoufands of weight, and feveral feet in breadth, if caft flat into any liquid body, would reach the bottom no quicker than the fluid could efcape to make way for it; if a fimilar body were to fall upon a quickfand, it would fhake every part of it, but would prevent the fand or earth from rifing, while the firm furrounding earth would confine it laterally: the ruins of the arched vaults would replace the waters which had been liberated from their fubterraneous confinement; folid heaps would then neceffarily be formed towards the centre, and the incumbent body would remain at the furface, or at leaft it would not be fwallowed up. Travellers when caught in this fnare fhould throw themfelves down.

Thefe quickfands are generally denoted by fmall ftreams, below which, when practicable, there is no danger in paffing. Quickfands are denoted by rills of water.

X. Extract

X.

Extract from a Memoir by Messrs. FOURCROY *and* VAUQUELIN, *on the Guano, or Natural Manure, of the small Islands of the South Sea, near the Coast of Peru. Read at the French National Institute, by* A. LAUGIER.*

M. Humboldt the first who gave an account of the Guano.

AMONG the multitude of subjects worthy the attention of the naturalist, which the philosophical Humboldt observed and collected during his travels, the Guano is not the least considerable, from the interest which it excites. This celebrated naturalist, by making us acquainted with this singular matter, one of the principal resources of agriculture in the countries he visited, has given confirmation to a discovery made by the authors of this memoir, about the time of his return. Reading their memoir on the existence of uric acid in the excrements of birds, it occurred to him that the Guano of the islets on the coast of Peru, which are frequented by great numbers of birds, might possibly be of the same nature. It remained for chemical investigation to examine how far this conjecture was well founded; and Messrs. Fourcroy and Vauquelin undertook the analysis of this matter. The following is the result of their labours, with this view, extracted from the Memoirs of the National Institute.

Memoir by Messrs. Fourcroy and Vauquelin on the excrements of birds, suggested the notion that the Guano was derived from the same origin.

Before I enter upon a detail of the experiments made upon Guano, in order to ascertain its nature, it may not be irrelevant to the subject to transcribe what M. Humboldt himself says of this substance in a note sent to the authors of this memoir.

Extract from M. Humboldt's note.

Guano found on certain small islands,

"The Guano is found in abundance in the South Sea, in the Chinche islands, near Pisco; and also on the more southern coasts and islets of Ilo, Iza, and Arica. The inhabitants of Chancay, who make Guano an object of their commerce, go to and return from the Chinche islands once in 20 days.—Each vessel contains from 1500 to 2000 cubic feet. A vanega sells at Chancay for 14 livres, and at Arica for 15 livres, Tournois.

—in beds 50 or 60 feet thick.

"Guano is dug from beds 50 or 60 feet thick; where it is worked like the bog-ore of iron. The islets where it is found

* Annales de Chimie, Vol. LVI. p. 258.

are

are frequented by a multitude of birds, particularly of the species of Ardea and Phœnicopterus, who roost there every night: but the excrements of these birds have hardly formed, in three centuries, a layer of four or five lines in depth. Is then the Guano the effect of some convulsion of the globe, like pit-coal and fossil wood? The fertility of the naturally sterile soil of Peru is derived from the Guano, which has become a material article of commerce. Fifty little vessels, called *Guaneras*, are constantly employed in fetching this manure, for the supply of the coast. Its effluvium may be smelled at the distance of a quarter of a league. The sailors, accustomed to this smell of ammonia, feel no inconvenience from it; but we could not approach it without being affected with continued fits of sneezing.

The place frequented by vast numbers of birds.

Sterile soil of Peru made fruitful by the Guano.

Vessels employed to collect it.

It has a strong odour of ammonia.

"Maize is the particular vegetable for which Guano forms an excellent manure. The Spaniards learned its use of the Indians: If too much be thrown upon the maize, the root is burned and destroyed. Guano is too acidifiable, and is therefore a manure containing hydruret of azote; whilst all other manures are rather hydrurets of carbon."

Maize particularly benefited by Guano as a manure.

Guano is of a dirty yellow colour, rather insipid to the taste, but possessing a powerful odour, partaking of castor and of valerian. It turns black in the fire, and exhales a white smoke of an ammoniacal smell.

Its appearance.

Its solubility in water, particularly with potash, determined the operators as to the method they should pursue in its analysis. They treated it successively with water, with potash, and with muriatic acid; each of which methods presented many phenomena, as related in the following part of this paper, divested of the particular details of process, which are too extensive for an extract.

Partly soluble in water.

Ten grammes of this matter, after being repeatedly washed with large quantities of boiling water, were reduced to $5\frac{7}{10}$ grammes. The water had obtained a red colour, which it communicated to paper stained with turnsole.

The solution is acid.

In distillation, the water yielded ammonia during the whole operation. Twenty-four hours afterwards, it had deposited a dirty yellow powder, possessing very little flavour, but with an odour of castor: On the surface was a crystalline pellicle, of the same colour with the deposition.

The water yielded ammonia by distillation, and deposited a yellow powder with a smell of castor.

The

The liquor, filtered and again evaporated, till reduced to 3 grammes, on cooling again, depofited a fawn-coloured powder, fimilar to the former, but in lefs quantity.

The powder, and the mother-water, which had held it in folution, were feparately examined.

Examination of the powder.

The powder offered the following properties:—It is a concrete and pulverulent fubftance, of a brilliant cryftalline afpect, and of a dull yellow colour. Before the blow-pipe it is confumed entirely away, yielding a flight empyreumatic odour of ammonia and pruffic acid. It is very little foluble in cold water; but abundantly fo in warm water, to which it communicates its yellowifh colour. This folution, though taftelefs, ftrongly reddens the tincture of turnfole, precipitates folutions of acetate of lead, and of nitrate of filver and mercury, in coloured flakes, which are readily and completely rediffolved by nitric acid.

This matter inftantaneoufly diffolves in an alkaline ley, which it tinges of a deep brown colour, exhaling a pungent fmell of ammonia. Sulphuric acid poured into the concentrated alkaline folution, throws down a very thick whitifh precipitate, and difengages a brifk odour, refembling that of weak acetic acid.

It is an acidulous falt, compofed of animal acid, ammonia, and lime.

The learned authors of this memoir conclude from their experiments, that this powder is an acidulous falt, compofed of animal acid, ammonia, and a little lime. In fact, very weak nitric acid, wherein this falt had been macerated in order to difengage the acid it contained, from its bafes, yielded, on evaporation, copious ammoniacal vapours, by the addition of potafh, and unequivocal figns of the prefence of lime, by the addition of oxalic acid.

Analyfis of the powder when deprived of its ammonia and lime.

When thus deprived of its ammonia and lime, this matter is lefs coloured and lefs foluble than before. Its folution in boiling water depofits pretty hard and brilliant cryftals, and more deeply reddens turnfole paper. It combines readily, and without any ammoniacal vapour, with potafh, from which all the acids again feparate it. Heat turns it black; and it burns, without leaving any refiduum, with an odour of ammonia and of pruffic acid. A neutral combination of it with ammonia will not precipitate the folution of fulphate of alumine, as is done by boniftic acid.

From

From these facts it appears evident, 1, that the matter taken up by the boiling water from Guano is an acid, partly saturated with ammonia and a little lime; 2, that this acid is an animal product, because it yields ammonia and prussic acid, when decomposed by fire; 3, that the same acid, according to all the known properties, must be uric acid, similar to that contained in the excrements of aquatic birds; 4, that it forms about one fourth part of the Guano.

The acid of Guano is uric acid, forming about ¼ of the whole.

The mother-water which deposited the powder, whose qualities have been just examined, is very acid; potash causes a copious disengagement of ammonia: It contains, therefore, an ammoniacal salt. Nitrate of barytes and of silver announce the presence of muriatic and sulphuric salts; which are precipitated in white flakes by lime-water, and are re-dissolved, though with difficulty, in muriatic acid.

Analysis of the mother-water which deposited the powder. It contains ammoniacal

This precipitate caused by lime water, is evidently formed of two salts, both soluble in acids without effervescence; one easily, and without the assistance of heat, the other with difficulty, even with the aid of heat; the former resists calcination, the latter is decomposed by fire, and afterwards dissolves in acids with effervescence. The first is phosphate of lime, the second oxalate of lime.

—and phosphate and oxalate of lime.

Messrs. Fourcroy and Vauquelin wished to separate these two salts, without their undergoing any alteration; and with this view they made use of weak nitric acid, which dissolved the phosphate of lime, and left the oxalate untouched. The latter salt, on being treated with a solution of carbonate of potash, yielded a precipitate that dissolved with effervescence in nitric acid: This solution displayed all the properties of nitrate of lime. The acid separated from the lime was taken up by the potash: in fact, the liquor possessed the characters of oxalate of potash; it precipitated with lime-water, a very divided powder, with sulphate of lime, in flakes, which would not readily unite; and with all the metallic solutions capable of precipitation by oxalic acid. Sulphate of alumine caused no precipitate, as it would have done with honistate of potash.

The potash found in the mother-water, after its precipitation by lime-water, and the disengagement of ammonia, caused by the addition of potash to the mother-water, prior to its decomposition by lime-water, sufficiently shew that these two alkalis saturate the acids contained in the mother-water of Guano;

The mother-water contains oxalates, phosphates, sulphates, and muriates of potash and of ammonia.

Guano; and that the mother-water certainly contains oxalates, phosphates, sulphates, and muriates of potash, and of ammonia.

The Guano left from the first washing,

The five grammes and seven-tenths, left after the washing of the ten grammes originally taken for analysis, were treated with caustic potash, which took up eight-tenths. This alkaline solution contained only uric acid, and a small portion of fat matter.

—contains uric acid.

Phosphate of lime, iron, and carbonate of lime;

The 4.9 grammes left by the caustic potash, were treated with muriatic acid: the product was phosphate of lime, iron and an atom of carbonate of lime.

—and left quartzose and ferruginous sand.

After these applications of water, of caustic potash, and of muriatic acid, there remained of the 10 grammes of Guano only 3.1 grammes of matter, composed of quartzose and ferruginous sands.

Recapitulation of component parts.

From the foregoing interesting analysis, it appears that the manure of the islets of the South Sea is formed of,—

1. Uric acid to the amount of $\frac{1}{4}$ of the whole compound; partly saturated with ammonia and lime:

2. Oxalic acid, partly saturated with ammonia and potash:

3. Phosphoric acid, combined with the same bases and with lime:

4. Small quantities of sulphate and muriates of potash and ammonia:

5. A small portion of fat matter:

6. Sand, partly quartzose and partly ferruginous.

Remarks.

The existence of Guano in places frequented by vast numbers of birds, and the identity of its nature with that of the excrements of aquatic birds, necessarily throw considerable light on the origin of this matter.

The analysis proves how well founded was the ingenious comparison of the learned naturalist, to whom we are indebted for our knowledge of this substance, no less interesting to us than useful to the inhabitants of Peru. It confirms the important discovery made by the researches of Messrs. Fourcroy and Vauquelin. In a word, this analysis possesses the advantage of proving a well-known maxim, that the sciences mutually enrich and enlighten each other with the light they possess; and it affords a new occasion to remark that among the sciences, there are perhaps none which have so immediate and so necessary a connection as Chemistry and Natural History.

XL. New

XI.

Note on a Varnish for glazing Cups. By M. PARMENTIER *.

M. BOMPOIX, chief apothecary to the French Military Hospital at Genoa, having sent me some coffee-cups of a remarkable lightness, and glazed with a varnish which is held in great repute, perhaps only because its preparation is kept a secret in that country; I requested him to use his utmost endeavours to procure me the receipt. He obtained it through the medium of one of his pupils, who learned the secret from the artificer at the manufactory, and had made from his prescription a varnish in every respect equal to that in question: Account of very light coffee-cups finely varnished.

It consisted of lintseed oil 1 ½ lbs.; amber 1lb.; litharge in powder, minium in powder, ceruse in powder, each, 5 oz. Receipt for the varnish.

Boil the lintseed oil in an unglazed earthen vessel, and tie the litharge, minium, and ceruse in a linen bag, which is to be suspended in the oil whilst boiling, so that it may not touch the bottom of the vessel. When the oil begins to turn brown, take out the bag, and put in a clove of garlic, cleared of the skin; continue the boiling; and when the garlic is dried away, put in another and another, to the amount of six or seven. In the mean time, the amber is to be melted in another unglazed vessel, according to the method hereafter prescribed; and when the oil has been sufficiently boiled, the fused amber is to be poured into it.

To melt the Amber.

Take two ounces of lintseed oil, to soften the amber and to assist its fusion by a very brisk fire, and when the amber is melted, add the lintseed oil, and boil the whole about two minutes. The fluid must then be strained through a coarse cloth, and when cold put into a bottle well corked, to prevent it from drying. Fusion of the amber.

Method of using the Varnish.

Let the piece intended to be varnished be first well polished, and then apply the varnish in the following manner: Application of the varnish.

* Annales de Chimie, Vol. LVI. p. 254.

Mix lamp black with varnish and a little turpentine, with a hair pencil, and lay one coat on the piece; when this is dry, lay on another, and repeat the process till four coats have been laid on, taking care to let each dry before the application of the next. When the last is dry, put the piece into a stove or oven to complete the drying, and then polish it with pumice and Tripoli powder.

Method of preparing the Piece intended to be varnished.

Manner of making the wooden cups.

Make the cups of hazel, alder, or cherry-tree, which are preferable to other woods for this use, because they are porous when perfectly dry, and do not warp. Form them according to fancy, and dry them in an oven. The work must be polished as if it were complete; and afterwards lay on the varnish as already prescribed.

Red varnish.

If it should be wished to give a red ground to the article, mix a little minium, or rather cinabar, with the varnish. Any other colour may in like manner be mixed with it, as may best please the fancy of the operator.

XII.

Account of a Series of Experiments, shewing the Effects of Compression in modifying the Action of Heat. By* Sir James Hall, *Bart. F. R. S. Edinburgh.*

Section I.

Ancient Revolutions of the Mineral Kingdom.—Vain Attempts to explain them.—Dependance of Geology on Chemistry.—Importance of the Carbonate of Lime.—Dr Black's *Discovery of Carbonic Acid subverted the former theories depending on Fire, but gave Birth to that of Dr.* Hutton.*—Progress of the Author's Ideas with Regard to that Theory.—Experiments with Heat and Compression, suggested to Dr.* Hutton *in 1790.—Undertaken by the Author in 1798.—Speculations on which his Hopes of Success were founded.*

Violent revolutions of the surface of the globe.

WHOEVER has attended to the structure of rocks and mountains, must be convinced, that our globe has not always existed

* The highly interesting experiments of Sir James Hall upon the effects of heat modified by compression, were communicated to the

existed in its present state; but that every part of its mass, so far at least as our observations reach, has been agitated and subverted by the most violent revolutions.

Facts leading to such striking conclusions, however imperfectly observed, could not fail to awaken curiosity, and give rise to a desire of tracing the history, and of investigating the causes, of such stupendous events; and various attempts were made in this way, but with little success; for while discoveries of the utmost importance and accuracy were made in astronomy and natural philosophy, the systems produced by the Geologists were so fanciful and puerile, as scarcely to deserve a serious refutation. Geologic[al sys]tems im[perfect]

One principal cause of this failure seems to have lain in the very imperfect state of chemistry, which has only of late years begun to deserve the name of a science. While chemistry was in its infancy, it was impossible that geology should make any progress; since several of the most important circumstances to be accounted for by this latter science, are admitted on all hands to depend upon principles of the former. The consolidation of loose sand into strata of solid rock; the crystalline arrangement of substances accompanying those strata, and blended with them in various modes, are circumstances of a because [chemi]cal know[ledge] was in i[n]fancy.

the Royal Society of Edinburgh in August 1804, and were transmitted to our Journal by the author in the following month. They appear in Vol. IX. page 98. That concise narrative could not but strongly excite the curiosity of philosophers and geologists, and direct their earnest expectations to a fuller detail. In the last session, June 3, 1805, a very ample communication was made, which has been printed with five quarto plates, very beautifully engraved by Lizars, from designs by Sir James. I cannot but consider it as one of those high marks of approbation, with which the Philosophical Journal has been honoured from time to time, that the author has again directed his attention to this periodical work, as the vehicle through which his discoveries should be more extensively circulated. With this view he has not only favoured me with the memoir as soon as completed, but has liberally taken upon himself the expence of engraving the plates for the Journal in the same superior style. By this means the numbers containing his memoir will be enriched with ten additional plates besides those usually given:—for I shall with great satisfaction follow the steps of the worthy baronet by presenting the additional expences of paper and print to the reader without charge. W. N.

chemical

chemical nature, which all those who have attempted to frame theories of the earth have endeavoured by chemical reasonings to reconcile to their hypotheses.

Fire and water adduced as the agents in two theories.

Fire and *water*, the only agents in nature by which stony substances are produced, under our observation, were employed by contending sects of geologists, to explain all the phenomena of the mineral kingdom.

Water has little agency on minerals.

But the known properties of water are quite repugnant to the belief of its universal influence, since a very great proportion of the substances under consideration are insoluble, or nearly so, in that fluid; and since, if they were all extremely soluble, the quantity of water which is known to exist, or that could possibly exist in our planet, would be far too small to accomplish the office assigned to it in the Neptunian theory*. On the other hand, the known properties of fire are no less inadequate to the purpose; for, various substances which frequently occur in the mineral kingdom, seem, by their presence, to preclude its supposed agency; since experiment shews, that, in our fires, they are totally changed or destroyed.

Common fire does not explain the facts.

Hence both theories were doubtful.

Under such circumstances, the advocates of either element were enabled, very successfully, to refute the opinions of their adversaries, though they could but feebly defend their own: and, owing, perhaps to this mutual power of attack, and for want of any alternative to which the opinions of men could lean, both systems maintained a certain degree of credit; and writers on geology indulged themselves, with a sort of impunity, in a style of unphilosophical reasoning, which would not have been tolerated in other sciences.

Carbonate of lime is of extensive importance,

Of all mineral substances, the *carbonate of lime* is unquestionably the most important in a general view. As limestone or marble, it constitutes a very considerable part of the solid mass of many countries; and, in the form of veins and nodules of spar, pervades every species of stone. Its history is thus interwoven in such a manner with that of the mineral kingdom at large, that the fate of any geological theory must very much depend upon its successful application to the various conditions of this substance. But, till Dr. Black, by his

* *Illustrations of the Huttonian Theory*, by Mr. Professor Playfair, § 430.

discovery of carbonic acid, explained the chemical nature of the carbonate, no rational theory could be formed, of the chemical revolutions which it has undoubtedly undergone.

This discovery was, in the first instance, hostile to the supposed action of fire; for the decomposition of limestone by fire in every common kiln being thus proved, it seemed absurd to ascribe to that same agent the formation of limestone, or of any mass containing it. seems not producible by heat.

The contemplation of this difficulty led Dr. Hutton to view the action of fire in a manner peculiar to himself, and thus to form a geological theory, by which, in my opinion, he has furnished the world with the true solution of one of the most interesting problems that has ever engaged the attention of men of science. Dr. Hutton's theory.

He supposed,

I. That heat has acted, at some remote period, on all rocks. That rocks have undergone heat under strong pressure,

II. That during the action of heat, all these rocks (even such as now appear at the surface) lay covered by a superincumbent mass, of great weight and strength.

III. That in consequence of the combined action of heat and pressure, effects were produced different from those of heat on common occasions; in particular, that the carbonate of lime was reduced to a state of fusion, more or less complete, without any calcination.

The essential and characteristic principle of his theory is thus comprised in the word *compression*; and by one bold hypothesis, founded on this principle, he undertook to meet all the objections to the action of fire, and to account for those circumstances in which minerals are found to differ from the usual products of our furnaces.

This system, however, involves so many suppositions, apparently in contradiction to common experience, which meet us on the very threshold, that most men have hitherto been deterred from the investigation of its principles, and only a few individuals have justly appreciated its merits. It was long before I belonged to the latter class; for I must own, that, on reading Dr. Hutton's first geological publication, I was induced to reject his system entirely, and should probably have continued still to do so, with the great majority of the world, but for my habits of intimacy with the author; the vivacity and Singular contrast of the perspicuity of Dr. Hutton's conversation, and the obscurity of his writings.

and perſpicuity of whoſe converſation formed a ſtriking contraſt to the obſcurity of his writings. I was induced by that charm, and by the numerous original facts which his ſyſtem had led him to obſerve, to liſten to his arguments, in favour of opinions which I then looked upon as viſionary. I thus derived from his converſation the ſame advantage which the world has lately done from the publication of Mr. Playfair's *Illuſtrations*; and, experienced the ſame influence which is now exerted by that work, on the minds of our moſt eminent men of ſcience.

The author's progreſs in the Doctor's theory.

After three years of almoſt daily warfare with Dr. Hutton, on the ſubject of his theory, I began to view his fundamental principles with leſs and leſs repugnance. There is a period, I believe, in all ſcientific inveſtigations, when the conjectures of genius ceaſe to appear extravagant; and when we balance the fertility of a principle, in explaining the phenomena of nature, againſt its improbability as an hypotheſis: The partial view which we then obtain of truth, is perhaps the moſt attractive of any, and moſt powerfully ſtimulates the exertions of an active mind. The miſt which obſcured ſome objects diſſipates by degrees, and allows them to appear in their true colours, at the ſame time, a diſtant proſpect opens to our view, of ſcenes unſuſpected before.

He propoſes experimental confirmation,

Entering now ſeriouſly into the train of reaſoning followed by Dr. Hutton, I conceived that the chemical effects aſcribed by him to compreſſion, ought, in the firſt place, to be inveſtigated; for, unleſs ſome good reaſon were given us for believing that heat would be modified by preſſure, in the manner alledged, it would avail us little to know that they had acted together. He reſted his belief of this influence on analogy; and on the ſatisfactory ſolution of all the phenomena furniſhed by this ſuppoſition. It occurred to me, however, that this principle was ſuſceptible of being eſtabliſhed in a direct manner by experiment, and I urged him to make the attempt; but he always rejected this propoſal, on account of the immenſity of the natural agents, whoſe operations he ſuppoſed to lie far beyond the reach of our imitation; and he ſeemed to imagine, that any ſuch attempt muſt undoubtedly fail, and thus throw diſcredit on opinions already ſufficiently eſtabliſhed, as he conceived, on other principles. I was far, however,

rejected by the Doctor.

from

from being convinced by these arguments; for, without being able to prove that any artificial compression to which we could expose the carbonate, would effectually prevent its calcination in our fires, I maintained, that we had as little proof of the contrary, and that the application of a moderate force might possibly perform all that was hypothetically assumed in the Huttonian theory. On the other hand, I considered myself as bound, in practice, to pay deference to his opinion, in a field which he had already so nobly occupied; and abstained, during the remainder of his life, from the prosecution of some experiments with compression, which I had begun in 1790.

In 1798, I resumed the subject with eagerness, being still of opinion that the chemical law which forms the basis of the Huttonian theory, ought, in the first place, to be investigated experimentally; all my subsequent reflections and observations having tended to confirm my idea of the importance of this pursuit, without in any degree rendering me more apprehensive as to the result. Experimental investigation undertaken.

In the arrangement of the following paper, I shall first confine myself to the investigation of the chemical effects of heat and compression, reserving to the concluding part the application of my results to Geology. I shall then appeal to the volcanoes, and shall endeavour to vindicate the laws of action assumed in the Huttonian theory, by shewing, that lavas, previous to their eruptions, are subject to similar laws; and that the volcanoes, by their subterranean and submarine exertions, must produce, in our times, results similar to those ascribed, in that theory, to the former action of fire. Order of the present treatise.

In comparing the Huttonian operations with those of the volcanoes, I shall avail myself of some facts, brought to light in the course of the following investigations, by which a precise limit is assigned to the intensity of the heat, and to the force of compression, required to fulfil the conditions of Dr. Hutton's hypothesis: For, according to him, the power of those agents was very great, but quite indefinite; it was therefore impossible to compare their supposed effects in any precise manner with the phenomena of nature.

My attention was almost exclusively confined to the carbonate of lime, about which I reasoned as follows: The carbonic acid, when uncombined with any other substance, exists natu- Argument respecting carbonate of lime

rally in a gaseous form, at the common temperature of our atmosphere; but when in union with lime, its volatility is repressed, in that same temperature, by the chemical force of the earthy substance, which retains it in a solid form. When the temperature is raised to a full red-heat, the acid acquires a volatility by which that force is overcome, it escapes from the lime, and assumes its gaseous form. It is evident, that were the attractive force of the lime increased, or the volatility of the acid diminished by any means, the compound would be enabled to bear a higher heat without decomposition, than it can in the present state of things. Now, pressure must produce an effect of this kind; for when a mechanical force opposes the expansion of the acid, its volatility must, to a certain degree, be diminished. Under pressure, then, the carbonate may be expected to remain unchanged in a heat, by which, in the open air, it would have been calcined. But experiment alone can teach us what compressing force is requisite to enable it to resist any given elevation of temperature; and what is to be the result of such an operation. Some of the compounds of lime with acids are fusible, others refractory; the carbonate, when constrained by pressure to endure a proper heat, may be as fusible as the muriate.

Pressure must oppose the expansion and escape of the acid, and enable it to support a stronger heat.

One circumstance, derived from the Huttonian Theory, induced me to hope, that the carbonate was easily fusible, and indicated a precise point, under which that fusion ought to be expected. Nothing is more common than to meet with nodules of calcareous spar inclosed in whinstone; and we suppose, according to the Huttonian theory, that the whin and the spar had been liquid together; the two fluids keeping separate, like oil and water. It is natural, at the junction of these two, to look for indications of their relative fusibilities; and we find, accordingly, that the termination of the spar is generally globular and smooth; which seems to prove, that, when the whin became solid, the spar was still in a liquid state; for had the spar congealed first, the tendency which it shews, on all occasions of freedom, to shoot out into prominent crystals, would have made it dart into the liquid whin, according to the peculiar forms of its crystallization; as has happened with the various substances contained in whin, much more refractory than itself, namely, augite, felspar, &c.; all of which having congealed in the liquid whin, have assumed their peculiar

Probability that the carbonate might not be of difficult fusion.

Facts which indicate its melting heat.

forms

forms with perfect regularity. From this I concluded, that when the whin congealed, which must have happened about 28° or 30° of Wedgwood, the spar was still liquid. I therefore expected, if I could compel the carbonate to bear a heat of 28° without decomposition, that it would enter into fusion. The sequel will shew that this conjecture was not without foundation.

I shall now enter upon the description of those experiments, the result of which I had the honour to lay before this Society on the 30th of August last (1804); fully aware how difficult it is, in giving an account of above five hundred experiments, all tending to one point, but differing much from each other in various particulars, to steer between the opposite faults of prolixity and barrenness. My object shall be to describe, as shortly as possible, all the methods followed, so as to enable any chemist to repeat the experiments; and to dwell particularly on such circumstances only as seem to lead to conclusions of importance. The experiments introduced.

The result being already known, I consider the account I am about to give of the execution of these experiments, as addressed to those who take a particular interest in the progress of chemical operations: in the eyes of such gentlemen, I trust, that none of the details into which I must enter, will appear superfluous.

Section II.

Principle of Execution upon which the following Experiments were conducted.—Experiments with Gun-Barrels filled with baked Clay, and welded at the Muzzle.—Method with the fusible Metal.—Remarkable Effects of its Expansion.—Necessity of introducing Air.—Results obtained.

When I first undertook to make experiments with heat acting under compression, I employed myself in contriving various devices of screws, of bolts, and of lids, so adjusted, I hoped, as to confine all elastic substances; and perhaps some of them might have answered. But I laid aside all such devices, in favour of one which occurred to me in January 1798; which, by its simplicity, was of easy application in all cases, and accomplished all that could be done by any device, since it secured perfect strength and tightness to the utmost that the vessels employed could bear, whether formed of metallic or earthy substance. The device depends upon the following general view; If we take a hollow tube or barrel (AD Pl. ix. The author's early contrivances for confining elastic substances at high temperatures.

fig.

The method adopted ultimately was to include the ſubject in an iron barrel, and cloſe the aperture by fuſion.

fig. 1.)* cloſed at one end, and open at the other, of one foot or more in length; it is evident, that by introducing one end into a furnace, we can apply to it as great heat as art can produce, while the other end is kept cool, or, if neceſſary, expoſed to extreme cold. If, then, the ſubſtance which we mean to ſubject to the combined action of heat and preſſure be introduced into the breech or cloſed end of the barrel (CD), and if the middle part be filled with ſome refractory ſubſtance, leaving a ſmall empty ſpace at the muzzle (AB), we can apply heat to the muzzle, while the breech containing the ſubject of experiment, is kept cool, and thus cloſe the barrel by any of the numerous modes which heat affords, from the welding of iron to the melting of ſealing-wax. Things being then reverſed, and the breech put into the furnace, a heat of any required intenſity may be applied to the ſubject of experiment, now in a ſtate of conſtraint.

First experiment with the muzzle plugged and welded,

My firſt application of this ſcheme was carried on with a common gun-barrel, cut off at the touch-hole, and welded very ſtrongly at the breech by means of a plug of iron. Into it I introduced the carbonate, previouſly rammed into a cartridge of paper or paſteboard, in order to protect it from the iron, by which, in ſome former trials, the ſubject of experiment had been contaminated throughout during the action of heat. I then rammed the reſt of the barrel full of pounded clay previouſly baked in a ſtrong heat, and I had the muzzle cloſed like the breech, by a plug of iron welded upon it in a common forge; the reſt of the barrel being kept cold during this operation, by means of wet cloths. The breech of the barrel was then put horizontally into a common muffle, heated to about 25° of Wedgwood. To the muzzle a rope was fixed, in ſuch a manner, that the barrel could be withdrawn without danger from an exploſion †. I likewiſe, about this time cloſed the muzzle of

in another inſtance ſoldered.

* This plate will be given in No. 54, being the ſupplement to the preſent volume.

† On one occaſion, the importance of this precaution was ſtrongly felt. Having inadvertently introduced a conſiderable quantity of moiſture into a welded barrel, an exploſion took place, before the heat had riſen to redneſs, by which, part of the barrel was ſpread out to a flat plate, and the furnace was blown to pieces. Dr. Kennedy, who happened to be preſent on this occaſion, obſerved, that notwithſtanding this accident, the time might come when we ſhould employ water in theſe experiments to aſſiſt the force

of the barrel, by means of a plug, fixed by folder only; which method had this peculiar advantage, that I could fhut and open the barrel without having recourfe to a workman. In thefe trials, though many barrels yielded to the expanfive force, others refifted it, and afforded fome refults that were in the higheft degree encouraging, and even fatisfactory, could they have been obtained with certainty on repetition of the procefs. In many of them, chalk, or common limeftone previoufly pulverifed, was agglutinated into a ftony mafs, which required a fmart blow of a hammer to break it, and felt under the knife like a common limeftone; at the fame time, the fubftance, when thrown into nitric acid, diffolved entirely with violent effervefcence. Satisfactory refults.

In one of thefe experiments, owing to the action of heat on the cartridge of paper, the baked clay, which had been ufed to fill the barrel, was ftained black throughout, to the diftance of two-thirds of the length of the barrel from its breech. This circumftance is of importance, by fhewing, that though all is tight at the muzzle, a protrufion may take place along the barrel, greatly to the detriment of complete compreffion: and, at the fame time, it illuftrates what has happened occafionally in nature, where the bituminous matter feems to have been driven by fuperior local heat, from one part of a coaly bed, though retained in others, under the fame compreffion. The bitumen fo driven off being found, in other cafes, to pervade and tinge beds of flate and of fandftone. Volatile matter may be driven from one to another part of a clofed barrel.

I was employed in this purfuit in fpring 1800, when an event of importance interrupted my experiments for about a year. But I refumed them in March 1801, with many new plans of execution, and with confiderable addition to my apparatus.

In the courfe of my firft trials, the following mode of execution had occurred to me, which I now began to put in practice. It is well known to chemifts, that a certain compofition of Experiments in which the fufible metal was ufed as the plug.

force of compreffion. I have fince made great ufe of this valuable fuggeftion: but he fcarcely lived, alas! to fee its application; for my firft fuccefs in this way took place during his laft illnefs.—I have been expofed to no rifk in any other experiment with iron barrels; matters being fo arranged, that the ftrain againft them has only commenced in a red heat, in which the metal has been fo far foftened, as to yield by laceration like a piece of leather.

different metals*, produces a substance so fusible, as to melt in the heat of boiling-water. I conceived that great advantage, both in point of accuracy and dispatch, might be gained in these experiments, by substituting this metal for the baked clay above mentioned: That after introducing the carbonate into the breech of the barrel, the fusible metal, in a liquid state, might be poured in, so as to fill the barrel to its brim: That when the metal had cooled and become solid, the breech might, as before, be introduced into a muffle, and exposed to any required heat, while the muzzle was carefully kept cold. In this manner, no part of the fusible metal being melted but what lay at the breech, the rest, continuing in a solid state, would effectually confine the carbonic acid: That after the action of strong heat had ceased, and after all had been allowed to cool completely, the fusible metal might be removed entirely from the barrel, by means of a heat little above that of boiling water, and far too low to occasion any decomposition of the carbonate by calcination, though acting upon it in freedom; and then, that the subject of experiment might, as before, be taken out of the barrel.

Advantages of this method.

This scheme, with various modifications and additions, which practice has suggested, forms the basis of most of the following methods.

A striking phenomenon. When the barrel was completely filled with fusible metal only, and the closed end of the iron exposed to heat in a muffle, the greater expansion of the fluid forced it through the texture of the iron in very fine wire resembling wool.

In the first trial, a striking phenomenon occurred, which gave rise to the most important of these modifications. Having filled a gun-barrel with the fusible metal, without any carbonate; and having placed the breech in a muffle, I was surprised to see, as the heat approached to redness, the liquid metal exuding through the iron in innumerable minute drops, dispersed all round the barrel. As the heat advanced, this exudation increased, till at last the metal flowed out in continued streams, and the barrel was quite destroyed. On several occasions of the same kind, the fusible metal, being forced through some very minute aperture in the barrel, spouted from it to the distance of several yards, depositing upon any substance opposed to the stream, a beautiful assemblage of fine wire, exactly in the form of wool. I immediately understood that the phenomenon was produced by the superior expansion of the liquid over the solid metal, in con-

* Eight parts of bismuth, five of lead, and three of tin.

sequence

fequence of which, the fufible metal was driven through the iron as water was driven through filver * by mechanical percuffion in the Florentine experiment. It occurred to me, that this might be prevented by confining along with the fufible metal a fmall quantity of air, which, by yielding a little to the expanfion of the liquid, would fave the barrel. This remedy was found to anfwer completely, and was applied, in all the experiments made at this time †.

Remedy. A fmall quantity of air was left in the barrel.

I now propofed, in order to keep the carbonate clean, to inclofe it in a fmall veffel; and to obviate the difficulty of removing the refult at the conclufion of the experiment, I further propofed to connect that veffel with an iron ramrod, longer than the barrel, by which it could be introduced or withdrawn at pleafure.

The carbonate was inclofed in a fmall feparate veffel.

* *Effays of Natural Experiments made in the Academie del Cimento*, tranflated by Waller, London, 1684, page 117. The fame in Muffchenbroek's Latin Tranflation, Ludg. Bat. 1731, p. 63.

† I found it a matter of much difficulty to afcertain the proper quantity of air which ought to be thus inclofed. When the quantity was too great, the refult was injured by diminution of elafticity, as I fhall have occafion fully to fhew hereafter. When too fmall, or when, by any accident, the whole of this included air was allowed to efcape, the barrel was deftroyed.

I hoped to afcertain the bulk of air neceffary to give liberty to the expanfion of the liquid metal, by meafuring the actual quantity expelled by known heats from an open barrel filled with it. But I was furprifed to find, that the quantity thus difcharged, exceeded in bulk that of the air which, in the fame heats, I had confined along with the carbonate and fufible metal in many fuccefsful experiments. As the expanfion of the liquid does not feem capable of fenfible diminution by an oppofing force, this fact can only be accounted for by a diftenfion of the barrel. In thefe experiments, then, the expanfive force of the carbonic acid, of the included air, and of the fufible metal, acted in combination againft the barrel, and were yielded to in part by the diftenfion of the barrel, and by the condenfation of the included air. My object was to increafe the force of this mutual action, by diminifhing the quantity of air, and by other devices to be mentioned hereafter. Where fo many forces were concerned, the laws of whofe variations were unknown, much precifion could not be expected, nor is it wonderful, that in attempting to carry the compreffing force to the utmoft, I fhould have deftroyed barrels innumerable.

A fmall

Description of this apparatus.

A small tube of glafs,* or of Reaumur's porcelain, about a quarter of an inch in diameter, and one or two inches in length, (fig. 2, A) was half filled with pounded carbonate of lime, rammed as hard as poffible; the other half of the tube being filled with pounded filex, or with whatever occurred as moft likely to prevent the intrufion of the fufible metal in its liquid and penetrating ftate. This tube fo filled, was placed in a frame or cradle of iron (*d f k h*, figs. 3, 4, 5, and 6), fixed to the end (*m*) of a ram-rod (*m n*). The cradle was from fix to three inches in length, and as much in diameter as a gun-barrel would admit with eafe. It was compofed of two circular plates of iron, (*d e f g* and *h i k l*, feen edgewife in the figures), placed at right-angles to the ram-rod, one of thefe plates (*d e f g*) being fixed to it by the centre (*m*). Thefe plates were connected together by four ribs or flattened wires of iron (*d h*, *e i*, *f k*, and *g l*,) which formed the cradle into which the tube (A), containing the carbonate, was introduced by thrufting the adjacent ribs afunder. Along with the tube juft mentioned, was introduced another tube (B), of iron or porcelain, filled only with air. Likewife, in the cradle, a pyrometer † piece (C) was placed in contact with (A) the tube con-

* I have fince conftantly ufed tubes of common porcelain, finding glafs much too fufible for this purpofe.

† The pyrometer-pieces ufed in thefe experiments were made under my own eye. Neceffity compelled me to undertake this laborious and difficult work, in which I have already fo far fucceeded as to obtain a fet of pieces, which, though far from complete, anfwer my purpofe tolerably well. I had lately an opportunity of comparing my fet with that of Mr. Wedgwood, at various temperatures, in furnaces of great fize and fteadinefs. The refult has proved, that my pieces agree as well with each other as his, though with my fet each temperature is indicated by a different degree of the fcale. I have thus been enabled to conftruct a table, by which my obfervations have been corrected, fo that the temperatures mentioned in this paper are fuch as would have been indicated by Mr. Wedgwood's pieces. By Mr. Wedgwood's pieces, I mean thofe of the only fet which has been fold to the public, and by which the melting heat of pure filver is indicated at the 22d degree. I am well aware, that the late Mr. Wedgwood, in his Table of Fufibilities, has ftated that fufion as taking place

containing the carbonate. These articles generally occupied the whole cradle; when any space remained, it was filled up by a piece of chalk dressed for the purpose. (Fig. 4, represents the cradle filled, as just described).

Method of using the same,

Things being thus prepared, the gun-barrel, placed erect with its muzzle upwards, was half filled with the liquid fusible metal. The cradle was then introduced into the barrel, and plunged to the bottom of the liquid, so that the carbonate was placed very near the breech, (as represented in fig. 5, the fusible metal standing at *o*). The air-tube (B) being placed so as to enter the liquid with its muzzle downwards, retained great part of the air it originally contained, though some of it might be driven off by the heat, so as to escape through the liquid. The metal being now allowed to cool, and to fix round the cradle and ramrod, the air remaining in the air-tube was effectually confined, and all was held fast. The barrel being then filled to the brim with fusible metal, the apparatus was ready for the application of heat to the breech, (as shewn in fig. 6.) Plate X.

the furnace and muffle arrangement, &c.

In the experiments made at this time, I used a square brick furnace, (figs. 7 and 8, having a muffle (*r s*) traversing it horizontally and open at both ends. This muffle being supported in the middle by a very slender prop, was exposed to fire from below, as well as all round. The barrel was placed in the muffle, with its breech in the hottest part, and the end next the muzzle projecting beyond the furnace, and surrounded with cloths which were drenched with water from time to time. (This arrangement is shewn in fig. 7.) In this situation, the fusible metal surrounding the cradle being melted, the air contained in the air-tube would of course seek the highest position, and its first place in the air-tube would be occupied by fusible metal. (In fig. 6, the new position of the air is shewn at *p q*).

Method of disengaging the contents after experiment.

At the conclusion of the experiment, the metal was generally removed by placing the barrel in the transverse muffle, with its muzzle pointing a little downwards, and so that the heat was applied first to the muzzle, and then to the rest of the barrel in succession. (This operation is shewn in fig. 8.) In

place at the 28th degree; but I am convinced that his observations must have been made with some set different from that which was afterwards sold.

ſome of the firſt of theſe experiments, I looſened the cradle, by plunging the barrel into heated brine, or a ſtrong ſolution of muriate of lime; which laſt bears a temperature of 250° of Fahrenheit before it boils. For this purpoſe, I uſed a pan three inches in diameter, and three feet deep, having a flat baſon at top to receive the liquid when it boiled over. The method anſwered, but was troubleſome, and I laid it aſide. I have had occaſion, lately, however, to reſume it in ſome experiments in which it was of conſequence to open the barrel with the leaſt poſſible heat *.

By theſe methods I made a great number of experiments, with reſults that were highly intereſting in that ſtage of the buſineſs, though their importance is ſo much diminiſhed by the ſubſequent progreſs of the inveſtigation, that I think it proper to mention but very few of them.

Calcareous ſpar converted into hard denſe marble by heat of 33° Wedgwood.

On the 31ſt of March, 1801, I rammed forty grains of pounded chalk into a tube of green bottle-glaſs, and placed it in the cradle as above deſcribed. A pyrometer in the muffle along with the barrel indicated 33°. The barrel was expoſed to heat during ſeventeen or eighteen minutes. On withdrawing the cradle, the carbonate was found in one ſolid maſs, which had viſibly ſhrunk in bulk, the ſpace thus left within the tube being accurately filled with metal, which plated the carbonate all over without penetrating it in the leaſt, ſo that the metal was eaſily removed. The weight was reduced from forty to thirty-ſix grains. The ſubſtance was very hard, and reſiſted the knife better than any reſult of the kind previouſly obtained; its fracture was cryſtalline, bearing a reſemblance to white ſaline marble; and its thin edges had a decided ſemitranſparency, a circumſtance firſt obſerved in this reſult.

Calcareous ſpar rendered cryſtalline with rhomboidal fracture by heat 23°.

On the 3d of March of the ſame year, I made a ſimilar experiment, in which a pyrometer-piece was placed within

* In many of the following experiments, lead was uſed in place of the fuſible metal, and often with ſucceſs; but I loſt many good reſults in this way: for the heat required to liquefy the lead approaches ſo near to redneſs, that it is difficult to diſengage the cradle without applying a temperature by which the carbonate is injured. I have found it anſwer well, to ſurround the cradle and a few inches of the rod with fuſible metal, and to fill the reſt of the barrel with lead.

the

the barrel, and another in the muffle; they agreed in indicating 23°. The inner tube, which was of Reaumur's porcelain, contained eighty grains of pounded chalk. The carbonate was found, after the experiment, to have loft $3\frac{1}{2}$ grains. A thin rim, lefs than the 20th of an inch in thicknefs, of whitifh matter, appeared on the outfide of the mafs. In other refpects, the carbonate was in a very perfect ftate; it was of a yellowifh colour, and had a decided femitranfparency and faline fracture. But what renders this refult of the greateft value, is, that on breaking the mafs, a fpace of more than the tenth of an inch fquare, was found to be completely cryftallized, having acquired the rhomboidal fracture of calcareous fpar. It was white and opaque, and prefented to the view three fets of parallel plates which are feen under three different angles. This fubftance, owing to partial calcination and fubfequent abforption of moifture, had loft all appearance of its remarkable properties in fome weeks after its production; but this appearance has fince been reftored, by a frefh fracture, and the fpecimen is now well preferved by being hermetically inclofed,

(To be continued.)

XIII.

On the Ufe of the Sutures in the Skulls of Animals. By Mr. B. Gibson *.

THE full ufe of the fingular junction of the bones of the fkull, which is called future, has, from the earlieft periods of anatomy and furgery, attracted the attention and eluded the refearches of the phyfiologift. To this remarkable feature in ofteogny, in a great meafure peculiar to a certain period of life, many ufes have been attributed. Some of thefe are totally erroneous; fuch as that for allowing the tranfpiration of moifture, to keep the brain cool and fit for thinking; for giving a more ftrict adhefion of the *dura mater* to the inner furface of the fkull; for admitting a more free communication by blood-veffels between the external and internal parts of the head; or for affording interftices, that the bones may be

Conjectures on the ufe of the futures in the fkulls of animals.

* Manchefter Memoirs, N. S. Vol. I. 39.

pufhed

pushed asunder by the growth of the brain, lest that organ should be cramped in its growth, in consequence of the comparatively slow growth of the bones of the skull.

Other supposed uses.

Other uses attributed to the sutures are merely slight advantages derived from their structure, which are enjoyed in early infancy, or till adult life, but gradually cease after that period. Thus at the time of birth the loose union of the bones of the skull accommodates the shape of the head to the figure of the different parts of the cavity through which it passes. At adult age, when the sutures are fully formed, they may occasionally check the progress (if I may be allowed the expression) of a fracture nearly spent;—or vibrations, communicated to the bones of the skull, will be propagated with less force to the brain, in consequence of the bones being separated at the sutures. It is, however, abundantly evident, that these are not the main purposes for which the sutures are formed; otherwise they would not begin to be obliterated at a period of life when they would perform these offices more usefully than ever. Consistent with this remark we shall find, that the true purpose for which they are formed, and the particular process with which they are connected, is fully completed before their obliteration takes place.

The cartilage between bones destined to be united, disappears at last.

When we take a view of the mode of junction between many bones, and parts of bones in the human body, which do not admit of motion, we find that with little exception they all agree in this particular; that sooner or later the cartilage or periosteum which once was interposed is obliterated, and these different portions, or entire bones, coalesce.

Instances in the ribs and other bones.

The separate portions, which originally compose the vertebræ, are early in thus uniting: after these the sides of the lower jaw; at a later period the epiphysis of a cylindrical bone is united to its body: and still later the bones of the skull usually coalesce, and the sutures are obliterated. Other bones, as those of the face, which have no motion and sustain little weight, are irregular in this respect; sometimes uniting, but generally remaining distinct, to the end of a long life.

Manner in which the osseous system is completed, &c.

The original formation of the osseous system in several distinct pieces, respects principally its speedy ossification at an early period of life, and its future convenient extension, till it has arrived at its full growth; and we may consider it as

general principle, that where two parts of *one* bone are separated from each other by an intervening cartilage, or *two* distinct bones merely by periosteum, at that part osseous materials are added to increase their length or extend their superficies. This we shall find takes place, whether the junction be effected by comparatively smooth surfaces, as between the body of a bone and its epiphysis; or between the bones of the skull by jagged sutures. Hence it appears that the bones of the body generally are increased in length or extent, not by a uniform extension of the whole substance, but by an addition of bony matter in some particular part.

Thus the body of a cylindrical bone is lengthened by addition to each end. This we might conclude would be the case, from considering the part in which its ossification commences: as this commences in a middle point and proceeds to each extremity, it is natural to suppose that its growth still goes on in the same direction, or continues at the extremities. That this is the case we know, not by reasoning alone, but by a direct experiment. Mr. Hunter sunk two small pieces of lead in the middle of the tibia, or shin bone of a pig, and measured accurately the distance between them: on examining the animal some time afterwards, it appeared, that though the bone had increased considerably in length, the pieces of lead still remained at the same distance from each other that they were before. From this experiment we learn, that a cylindrical bone is not extended in its middle, but is lengthened by addition to its extremities, where the body of the bone is joined to its epiphysis; the chief intention of the epiphysis being to allow the intervention of a vascular organ, which may conveniently deposit bony materials, without interfering with the joint itself.

Cylindrical bones are lengthened by additions at each end.

As cylindrical bones are lengthened at their extreme parts, we are led by analogy to conclude, that the same general plan is pursued in the extension of the flat bones of the body: and although we have no direct experiment by which this has been proved, there are circumstances which leave little doubt but they are extended by addition to their edges. Thus to take the parietal bone as an example; as ossification begins in a central point and extends towards the circumference, it is probable that to the completion of the process, it continues to go on in the same direction; and the same circumstance taking place

The same process appears to take place at the edges of flat bones.

place in every bone of the cranium, it is probable that even after the whole of the brain is incased in bone, the addition is still made at the edge of each, and that the general enlargement originates where they are all mutually joined by the sutures. Of this process I had a very striking illustration some years ago. In a young subject, from what cause I know not, the deposition of osseous matter had been suddenly increased a short time before death. It was in different stages of progress, but had taken place in all the bones of the body which I preserved; in some partially, in others generally. In all, the new osseous matter was elevated above the level of the bone upon which it was placed. In some parts of the parietal bones it was only in its commencement, and put on the appearance of a net-work, similar to that which may be observed in the same bones at an early period of their formation. In other parts the meshes of the net-work were more or less filled up; in others again completely, so as to put on the uniform appearance of solid bone. The same reticulated appearance was evident on the edges of all the bones of the skull, where they form the sutures, and at the extremities of the cylindrical bones, between the body and epiphysis. The same appearance of increased deposition was seen on the surface of the cylindrical bones, with this difference, that the meshes were not circular, but oblong squares; so as to put on more of the striated appearance. In some parts, the newly secreted bone was easily separable from the general mass, and formed a thin layer externally, affording one of the best proofs I have met with, of the increase of cylindrical bones in thickness by deposition externally, whilst a corresponding internal absorption goes on. From the striking similarity of appearance on the surfaces and edges of the bones, we may safely conclude, that the same process of deposition was going on in both, and may thence infer, that the bones of the skull are increased in extent bythe deposition of osseous matter at their edges, or where they are joined to each other by suture. This fact points out to us, in a great measure, the real use of this peculiar mode of junction.

Instance in the skull.

In order that the bones of the skull may be increased in extent, it is necessary that they should be retained at a certain distance from each other; that the periosteum with its vessels

The serrated edges give firmness, &c.

2 may

may pass down between them, free from compression and secrete the osseous matter. At the same time, the thin bones composing the upper part of the skull, resting as an arch upon its basis, must be united together so firmly, as not to be separated by common degrees of violence. For this purpose, projecting points from the external surface of each bone, are reciprocally received into corresponding nitches; which only penetrate through one half of the thickness of the skull, and form an irregular kind of dovetailing.

Two advantages arise from this structure, being superficial, and confined to the external table of the skull. The projecting points from each side, resting upon the solid surface of the internal table of the opposite bone, can resist more effectually any violence, which might tend to force the bones inwards; and the internal part of the skull presents, by this means, a smooth surface to the coverings of the brain; for internally no appearance of a jagged suture is seen.

Thus the sutures unite the bones, and admit the vascular organ requisite for their growth.

From this view of the subject we see, that the sutures of the human skull, by their peculiar formation, at once unite the bones together, and so far separate them, as to allow the interposition of a vascular organ by which their superficies is gradually increased to its greatest extent *. This explanation of

* Since this paper was written in the year 1800, I have found, that a similar opinion was published by Professor Soemmerring in 1794, in his valuable work, "De corporis humani fabrica." To him, therefore, any credit which may belong to the primary suggestion of this use of the sutures is due. As his opinion, however, has been little noticed by anatomists generally, and is placed in a clearer point of view by the facts which suggested this further explanation of it to me, it has not been thought improper to give this essay a place in these Memoirs. But whilst the reader will see, by the following quotation, the near resemblance between the opinion of Professor Soemmerring and that which I have brought forward, I hope the character of plagiarist or compiler will not be attributed to me.

"Usus horum sic sese habentium terminorum ossa cranii inter bene liquet.

"Incrementum ambitus calvariæ levant, ni enim inter ossa capitis mox post partum suturæ interponerentur, hæc crescere non possent, nisi aliâ ratione natura rem institueret. Tali igitur modo incrementum calvariæ cum incremento reliquorum ossium convenit; initio enim suturis, vel potius lineis cartilaginosis ossa iis

of the use of sutures comprehends and accounts for those concomitant circumstances, which were considered by older anatomists as their real use; and, as far as I can see, is not contradicted by any fact connected with them.

Other remarks and inferences.

If it be asked, for instance, why at the sutures there is a stronger adhesion of the *dura mater* internally and *periosteum* externally than in other parts of the skull? the answer is, that these membranes with their vessels are continued into the sutures, to form conjointly the secretory organ, by which the bones are extended.

If it be asked, why there is a greater vascularity or an appearance of blood-vessels passing through the sutures? it is perfectly consistent with this opinion to answer, that the increase of blood goes to this secretory organ, for the purpose of the extension of the bones.

Why the sutures are obliterated, &c.

The explanation here offered accounts also for the general obliteration of the sutures after a certain period of life; for the bones having then arrived at their full size, the organ for the secretion of osseous matter is no longer needed; it shrinks and is absorbed, and the bones gradually coalesce; by which a further advantage is derived, that of an accession of strength to the cranium at large.

iis locis conglutinantur, verum tamen non nisi in embrionibus ad fonticulos, ut aiunt, hæc linea notabili latitudine, observatur. Ossibus enim capitis hic locorum cerebro crescente, placide quasi deductus, cartilago augetur, latior evasura, nisi pristina pars simul in os mutaretur, inde ossa calvariæ, eodem modo, quo ossa longa deductis epiphysibus, vel quod unum idemque est, marginibus crescere, liquet, etsi in ossibus, longis sutura epiphyses inter et diaphysin non crispetur.

"Quo junior igitur infans, eo minus crispa et implexa sutura, vel ut rectius loquar, linea cartilaginosa augusta, ossa jungens, observatur. Quum vero aucta ætate ossa, cresente cerebro, deducuntur, eorumque, crassitudo adposita cum internæ, tum externæ potissimum tabulæ, (internæ enim incrementum citius absolutum videtur) massa ossea, augetur, non potest non esse, quin hæc crispa suturæ forma, quum quidem nasci cœpit, externâ in superficie tamdiu, augeatur, donec tandem ipsa ea quam maxime impediat, quo minus cerebrum calvariam ulterius deducere possit, quod pubertatis tempore accidit. Rarissime hæc ossificatio ad ætatem virilem usque detinetur."—Soemmerring de corporis Humani Fabrica, page 212.

If

If any additional argument be neceſſary in ſupport of this opinion, I may alſo notice the ſtriking analogy which ſubſiſts between the ſeparation of one bone of the ſkull from another by a ſuture; and that ſeparation which exiſts between the body of a cylindrical bone and its epiphyſis. They each remain only for a certain length of time; each allows the interpoſition of a ſecretory organ; and both begin to be obliterated when the bones with which they are connected have completed their growth, and their continuance is no longer neceſſary.

XIV.

On the Reproduction of Buds. By THOMAS ANDREW KNIGHT, *Eſq. F. R. S.**

MY DEAR SIR,

EVERY tree in the ordinary courſe of its growth generates, in each ſeaſon, thoſe buds which expand in the ſucceeding ſpring; and the buds thus generated, contain, in many inſtances, the whole of the leaves which appear in the following ſummer. But if theſe buds be deſtroyed during the winter or early part of the ſpring, other buds, in many ſpecies of trees, are generated, which in every reſpect perform the office of thoſe which previouſly exiſted, except that they never afford fruit or bloſſoms. This reproduction of buds has not eſcaped the notice of naturaliſts; but it does not appear to have been aſcertained by them from which, amongſt the various ſubſtances of the tree, the buds derive their origin.

If the generated buds of a ſeaſon be deſtroyed, others are produced.

Du Hamel conceived that reproduced buds ſprang from pre-organized germs; but the exiſtence of ſuch germs has not, in any inſtance, been proved, and it is well known that the roots, and trunk, and branches of many ſpecies of trees will, under proper management, afford buds from every part of their ſurfaces; and therefore, if this hypotheſis be well founded, many millions of ſuch germs muſt be annually generated in every large tree; not one of which in the ordinary courſe of nature will come into action: and as nature, amidſt all its exu-

Du Hamel's opinion that theſe laſt are from pre-organized germs.

Objection.

* Phil. Tranſ. 1805.

berance,

berance, does not abound in ufelefs productions, the opinions of this illuftrious phyfiologift are, in this cafe, probably erroneous.

Suppofition that they are afforded by the bark.

Other naturalifts have fuppofed the buds, when reproduced, to fpring from the plexus of veffels which conftitutes the internal bark; and this opinion is, I believe, much entertained by modern botanifts: it neverthelefs appears to be unfounded, as the facts I fhall proceed to ftate will evince.

Inftance to the contrary in fea cale. Internal buds.

If the fruit-ftalks of the fea cale *(crambe maritima)* be cut off near the ground in the fpring, the medullary fubftance, within that part of the ftalk which remains attached to the root, decays; and a cup is thus formed in which water collects in the fucceeding winter. The fides of this cup confift of a woody fubftance, which in its texture and office, and mode of generation, agrees perfectly with the alburnum of trees; and I conceive it to be as perfect alburnum, as the white wood of the oak or elm: and from the interior part of this fubftance, within the cup, I have frequently obferved new buds to be generated in the enfuing fpring. It is fufficiently obvious that the buds in this cafe do not fpring from the bark; but it is not equally evident that they might not have fprung from fome remains of the medulla.

Potatoes afford buds at the cut furface,

In the autumn of 1802, I difcovered that the potatoe poffeffed a fimilar power of reproducing its buds. Some plants of this fpecies had been fet, rather late in the preceding fpring, in very dry ground, where, through want of moifture, they vegetated very feebly; and the portions of the old roots remained found and entire till the fucceeding autumn. Being then moiftened by rain, many fmall tubers were generated on the furfaces made by the knife in dividing the roots into cuttings; and the buds of thefe, in many inftances, elongated into runners, which gave exiftence to other tubers, fome of which I had the pleafure to fend to you.

—and therefore not from the bark.

I have in a former paper remarked, that the potatoe confifts of four diftinct fubftances, the epidermis, the true fkin, the bark, and its internal fubftance, which, from its mode of formation, and fubfequent office, I have fuppofed to be alburnous: there is alfo in the young tubes a tranfparent line through the centre, which is probably its medulla. The buds and runners fprang from the fubftance which I conceive to be the alburnum of the root, and neither from the central part of it, nor from the

the ſurface in contact with the bark. It muſt, however, be admitted, that the internal ſubſtance of the potatoe correſponds more nearly with our ideas of a medullary than of an alburnous ſubſtance, and therefore this, with the preceding facts, is adduced to prove only that the reproduced buds of theſe plants are not generated by the cortical ſubſtance of the root: and I ſhall proceed to relate ſome experiments on the apple, and pear, and plumb-tree, which I conceive to prove that the reproduced buds of thoſe plants do not ſpring from the medulla.

Other inſtances of fruit trees in which reproduced buds appeared to ſpring from the alburnum.

Having raiſed from ſeeds a very conſiderable number of plants of each of theſe ſpecies in 1802, I partly diſengaged them from the ſoil in the autumn, by digging round each plant, which was then raiſed about two inches above its former level. A part of the mould was then removed, and the plants were cut off about an inch below the points where the ſeed-leaves formerly grew; and a portion of the root, about an inch long, without any bud upon it, remained expoſed to the air and light. In the beginning of April, I obſerved many ſmall elevated points on the bark of theſe roots, and, removing the whole of the cortical ſubſtance, I found that the elevations were occaſioned by ſmall protuberances on the ſurface of the alburnum. As the ſpring advanced, many minute red points appeared to perforate the bark: theſe ſoon aſſumed the character of buds, and produced ſhoots, in every reſpect ſimilar to thoſe which would have ſprung from the organized buds of the preceding year. Whether the buds thus reproduced derived any portion of their component parts from the bark or not, I ſhall not venture to decide; but I am much diſpoſed to believe that, like thoſe of the potatoe, they ſprang from the alburnous ſubſtance ſolely.

They do not originate from the medulla.

The ſpace, however, in the annual root, between the medulla and the bark is very ſmall; and therefore it may be contended that the buds in theſe inſtances may have originated from the medulla. I therefore thought it neceſſary to repeat ſimilar experiments on the roots and trunks of old trees, and by theſe the buds were reproduced preciſely in the ſame manner as the annual roots: and therefore, conceiving myſelf to have proved in a former Memoir,* that the ſubſtance which has

* Phil. Tranſ. of 1803.

been

been called the medullary procefs does not originate from the medulla, I muft conclude that reproduced buds do not fpring from that fubftance.

Remarks on the manner in which this procefs of nature is probably effected.

I have remarked, in a paper which you did me the honour to lay before the Royal Society in the commencement of the prefent year, that the alburnous tubes at their termination upwards invariably join the central veffels, and that thefe veffels, which appear to derive their origin from the alburnous tubes, convey nutriment, and probably give exiftence to new buds and leaves. It is alfo evident, from the facility with which the rifing fap is transferred from one fide of a wounded tree to the other, that the alburnous tubes poffefs lateral as well as terminal orifices: and it does not appear improbable that the lateral as well as the terminal orifices of the alburnous tubes may poffefs the power to generate central veffels; which veffels evidently feed, if they do not give exiftence to, the reproduced buds and leaves. And therefore, as the preceding experiments appear to prove that the buds neither fpring from the medulla nor the bark, I am much inclined to believe that they are generated by central veffels which fpring from the lateral orifices of the alburnous tubes. The practicability of propagating fome plants from their leaves may feem to ftand in oppofition to this hypothefis; but the central veffel is always a component part of the leaf, and from it the bud and young plant probably originate.

Attempt to difcover the fame power in feeds.

I expected to difcover in feeds a fimilar power to regenerate their buds; for the cotyledons of thefe, though diffimilar in organization, execute the office of the alburnum, and contain a fimilar refervoir of nutriment, and at once fupply the place of the alburnum and the leaf. But no experiments, which I have yet been able to make, have been decifive, owing to the difficulty of afcertaining the number of buds previoufly exifting within the feed. Few, if any, feeds, I have reafon to believe, contain lefs than three buds, one only of which, except in cafes of accident, germinates; and fome feeds appear to contain a much greater number. The feed of the peach appears to be provided with ten or twelve leaves, each of which probably covers the rudiment of a bud, and the feeds, like the buds of the horfe-cheftnut, contain all the leaves and apparently all the buds of the fucceeding year: and I have never been able to fatisfy myfelf that all the buds were eradicated without

having

having destroyed the base of the plumule, in which the power of reproducing buds probably resides, if such power exists.

Nature appears to have denied to annual and biennial plants (at least to those which have been the subjects of my experiments) the power which it has given to perennial plants to reproduce their buds; but nevertheless some biennials possess, under peculiar circumstances, a very singular resource, when all their buds have been destroyed. A turnip, bred between the English and Swedish variety, from which I had cut off the greater part of its fruit-stalks, and of which all the buds had been destroyed, remained some weeks in an apparently dormant state; after which the first seed in each pod germinated, and bursting the seed-vessel, seemed to execute the office of a bud and leaves to the parent plant, during the short remaining term of its existence, when its preternatual foliage perished with it. Whether this property be possessed by other biennial plants in common with the turnip, or not, I am not at present in possession of facts to decide, not having made precisely the same experiment on any other plant.

Annual and biennial pl[ants] have not [this] power.

I will take this opportunity to correct an inference that I have drawn in a former paper,* which the facts (though quite correctly stated) do not, on subsequent repetition of the experiment, appear to justify. I have stated, that when a perpendicular shoot of the vine was inverted to a depending position, and a portion of its bark between two circular incisions round the stem removed, much more new wood was generated on the lower lip of the wound become uppermost by the inverted position of the branch, than on the opposite lip, which would not have happened had the branch continued to grow erect; and I have inferred that this effect was produced by sap which had descended by gravitation from the leaves above. But the branch was, as I have there stated, employed as a layer, and the matter which would have accumulated on the opposite lip of the wound had been employed in the formation of roots, a circumstance which at that time escaped my attention. The effects of gravitation on the motion of the descending sap, and consequent growth of plants, are, I am well satisfied, from a great variety of experiments, very great; but it will be very difficult to discover any method by which the extent

Correctio[n of a] former in[fer]ence.

* Phil. Transf. of 1803.

of

of its operation can be accurately afcertained. For the veffels which convey and impel * the true fap, or fluid from which the new wood appears to be generated, pafs immediately from the leaf-ftalk towards the root; and though the motion of this fluid may be impeded by gravitation, and it be even again returned into the leaf, no portion of it, unlefs it had been extravafated, could have defcended to the part from which the bark was taken off in the experiment I have defcribed. I am not fenfible that in the different papers which I have had the honour to addrefs to you, I have drawn any other inference which the facts, on repetition of the experiments, do not appear capable of fupporting.

I am, &c.

THO[s]. ANDREW KNIGHT.

Elton, May 12, 1805.

XV.

Experiments on the Gafeous Oxide of Azote, by a Society of Amateurs at Touloufe. Publifhed by M. P. DISPAN, *Profeffor of Chemiftry in the College of that City.*†

Difagreement of former experiments on the oxide of azote.

THE motive for the following experiments was the very different, and even contradictory refults, which have been publifhed of former effects. The experiments were tried upon more than a dozen perfons, and in fome cafes repeated two or three times; the fenfations which each experienced were written down at the moment, by the reporter, from whofe memorandums the fubfequent obfervations are drawn.

Preparation of the nitrate of ammonia.

The nitrate of ammonia ufed for the experiment was indiftinctly cryftallized, but was quite neutral. Its tafte was very pungent, with a flight odour. It had been formed by the faturating very pure nitric acid with ammoniacal gas obtained by diftilling fal ammoniac with the common potafh of commerce.

Procefs for obtaining the gafeous oxide of azote.

About one hectogramme (1545 grains) of this falt was put into a fmall retort, and placed on a fand-bath, where the falt

* Phil. Tranf. of 1804.

† Annales de Chimie, Vol. LVI. p. 243.

melted

melted and boiled for a ſhort time without yielding any gas; at length, the retort became filled with a white vapour, which quickly diſappeared; the gas was then rapidly diſengaged, and was caught in bladders. By degrees the diſengagement became more and more ſlow, and when the operation was ended, ſcarcely any thing remained in the retort.

Another experiment was made with a larger retort, and three hectogrammes (10 oz. troy) of the ſalt, from which was obtained gas ſufficient to fill eight bladders. This operation proceeded in a ſimilar manner with the former; except that as the retort cooled, a red vapour aroſe within it, which it was aſcertained by experiment, contained no nitrous gas. The ſame pr ceſs on a larg ſcale.

Effects of Gaſeous Oxide of Azote when breathed into the Lungs.

All who have taſted or inhaled this gas, agree in deſcribing its flavour as ſtrongly ſaccharine, and remaining upon the organs of ſome perſons during the whole day after receiving it. M. Diſpan obſerved in it an after-taſte of nitre; but acknowledges that it was the laſt collected gas which he taſted.—M. de M * * *, perhaps under a ſimilar impreſſion, ſays he perceived in it a ſtyptic quality. The gas has ſaccharine t

The method of reſpiring this gas was by means of a bladder with a ſtop-cock in it, applied to the mouth; the noſtrils being cloſed, and the lungs as much as poſſible emptied. The gas was ſpired.

No. 1. The firſt perſon upon whom the experiment was tried, ſwooned at the third inſpiration, and remained ſenſeleſs about five minutes, when he recovered, but with a ſenſation of great fatigue. He recollected to have experienced only a ſudden faintneſs, attended with a tingling at the temples.

No. 2. M. de M * * * obſerved a ſaccharine and ſtyptic taſte, and experienced a ſenſe of great dilatation, accompanied with heat in the breaſt; his veins ſwelled, and his pulſe was quickened: ſurrounding objects ſeemed to revolve round him. But he thought he could have borne a ſtronger doſe; the bladder not being large enough for his lungs.

No. 3 experienced a ſaccharine taſte on the firſt inſpiration; but became inſenſible to thoſe which ſucceeded. His lungs were forcibly dilated with great heat. When the bladder was removed, he appeared very comfortable, but could not refrain from violent burſts of involuntary laughter.

No.

No. 4 had the fame faccharine tafte with the preceding, and retained the impreffion from ten o'clock in the morning till after midnight. He experienced vertigoes, and his legs trembled under him during the remainder of the day.

No. 5, the fame faccharine tafte. On quitting the bladder, he had a dizzinefs of fight, which was fucceeded by a fenfation of great pleafure throughout the body. His legs were weakened.

No. 6. Saccharine flavour throughout the day; tingling in the ears; legs tottering, and the ftomach oppreffed. All that he experienced was rather painful than agreeable.

Receiving the gas from a bladder, had no influence on the refult of the experiment.

In order to afcertain what influence the mode of breathing from a bladder might have on the foregoing refults, the parties were requefted to infpire common air in the fame manner.—They were all mechanically fatigued by it, and nothing more.

Oxigen gas differed from common air only by a fmall increafe of the heat of the lungs.

The bladders were next filled with oxygen gas, and applied as before to the fame perfons, who found only a flight difference between it and common air, confifting in an augmentation of the heat of the lungs.

Conclufion.

The fingular effects above defcribed, can, therefore, and ought only to be afcribed to the gafeous oxide of azote.

Other experiments.

Another meeting of the fociety was held for repeating the experiments more at large, on the refpiration of gafeous oxide of azote.

Defcription of the apparatus with upwards of 2lbs. of the falt.

Eight hectogrammes (27½ oz. troy) of nitrate of ammonia, prepared as before, were put into a retort, with its neck fitted to a double-bodied receiver, from whence, by means of a tube of welter, the gas paffed into an inverted veffel over water. The retort was placed on a fand-bath.

Particulars of the procefs.

As foon as the heat affected the retort, the falt melted; and nearly at the fame moment, fparkling vapours arofe in the retort, but in very fmall quantity. The air which the heat expelled from the veffels had a nitrous odour; but this as well as the vapours gradually diminifhed, and as the procefs continued they difappeared altogether; they were fucceeded by a lively fmell of pruffic acid. At length the retort became filled with white vapours, and the gafeous oxide of azote began to pafs over. The difengagement foon became fo abundant that it was judged proper to draw out the fire; but afterwards, on replacing the coals, the gas, which in the interval had diminifhed,

nished, was again so rapidly developed that the luting of the vessels began to give way. But notwithstanding the loss which this occasioned, the disengagement continued extremely rapid in the receiver for at least a quarter of an hour.

M. Dispan supposes, that if the luting had not given way, an explosion would have taken place, as has happened to others in this process.

Danger of explosion.

He next proceeds to state the effects of the respiration of this gas.

Twelve persons underwent the experiment, and on many it was repeated. He observes that most of them had inhaled the gas of the former operation, where two out of seven experienced pleasing sensations; but on this second occasion, not one felt pleasure; on the contrary, they all felt pain, and many suffered extremely.

The effects produced by the last gas were more powerful than the former.

One person stamped with his foot the whole time of the breathing: when the bladder was removed, he recovered from the profound stupor into which he had been plunged, and complained of a pain in the back part of his head, as if he had received a violent blow from a dagger: he could not be prevailed on to make another trial. The other persons in general were affected with vertigoes and dizziness of sight, succeeded in some by involuntary convulsive fits of laughter.

M. Dispan tried the effects of this gas on himself, which he thus describes:—

M. Dispan's description of the effect of the gas upon himself.

"At the first inspiration, I emptied the bladder, and my mouth was instantaneously filled with a saccharine flavour, which extended into my lungs and inflated them. I emptied and filled them again; but on the third attempt, my ears were filled with a tingling noise, and I dropped the bladder. I did not, however, become altogether insensible, but remained in a kind of benumbed astonishment, rolling my eyes about without fixing them on any particular object: I was then suddenly seized with convulsive laughing fits, such as I never in my life before experienced. In a few seconds this propensity to laugh stopped suddenly, and I no longer felt any unpleasant symptom."

Two others on whom the gas was tried, experienced only a convulsive movement of some of the muscles of the face; but were in the course of the day attacked with violent diarrhœa.

Effects on two other persons.

changes the appearances of organized bodies. The fact, however, is decisive, as to the principal question. It has summoned the discordant opinions of philosophers before a tribunal, from which there is no appeal.

Williamsburgh, October 6th, 1805.

Note on the preceding Paper. By the Editor.

Facts by Mr. Nevil on long preserved vegetable bodies.

Mr. Francis Nevil, in his account of the elephantine teeth that were discovered in the north of Ireland, early in the eighteenth century, has mentioned some facts relative to the long preservation of vegetable matters, which seem worthy of our notice in this place: and the more so, as this gentleman's paper seems not to have excited any attention among the modern writers on the exuviæ of animals found in countries in which the living animals themselves are no longer seen. Some extravagant conjectures are mixed with Mr. Nevil's account: but these do not, in the least, invalidate the truth of what he says, relative to the bed upon which the Irish elephant was laid.

His narrative.

"The place (says he) where this monster lay, was thus prepared, which makes me believe it had been buried, or that it had lain there since the deluge. It was about four feet under ground, with a little rising above the superficies of the earth, which was a plain under the foot of a hill, and about thirty yards from the brook * or thereabout. The bed whereon it lay had been laid with fern, with that sort of rushes here called sprits, and with bushes intermixed. Under this was a stiff blue clay on which the teeth and bones were found: above this was first a mixture of yellow clay and sand much of the same colour; under that a fine white sandy clay, which was next to the bed: the bed was for the most part a foot thick, and in some places thicker, with a moisture clear through it; it lay sad and close, and cut much like turf, and would divide into flakes, thicker or thinner as you would; and in every layer the seed of the rushes was as fresh as if new pulled, so that it was in the height of seed-time that those bones were laid there. The branches of the fern, in every

* "A small brook that parts the counties of Cavan and Monaghan."

lay

lay as we opened them, were very diſtinguiſhable, as were the ſeeds of the ruſhes and the tops of the boughs. The whole matter ſmelt very ſour as it was dug, and tracing it I found it 34 feet long and about 20 or 22 feet broad."—" I forgot to mention that there was a great many nut-ſhells found about the bed, perhaps thoſe might have been on the buſhes which compoſed part of the bed *."

XVII.

Obſervations on the Danger of uſing Earthen-ware or Pottery of a bad Quality. By M. Poidevin *of Rouen* †.

PURE white argil forms the body of the fineſt pottery which bears the name of porcelain; clays leſs pure, and coloured more or leſs with iron, ſerve to form the ſtone ware, or hard earthen-ware, and the common or ſoft ſort, which differs from the other, in not experiencing a commencement of fuſion at their ſurface in baking, like porcelain or ſtone ware.

Different kinds of pottery.

This badly prepared common earthen-ware is the kind which is occaſionally attended with danger in its uſe, and is the ſubject of this paper.

Earthen-ware.

Common brown ware.

The biſcuit of brown earthen-ware is prepared from a ferruginous clay; that of white earthen-ware is compoſed of a mixture of ferruginous clay, of another clay containing much ſilicious ſand, a little lime, and finally of a porous clay, which renders it leſs compact, and gives it whiteneſs after baking.

Nature not always affording theſe earths in the ſame ſtate of combination, occaſions differences in the biſcuit, when it becomes ſubjected to the heat: other differences alſo ariſe in the action of the enamel on the biſcuit. If the earth be too ferruginous, or too much mixed with ſilicious particles, the enamel, during the baking, acts as a flux on the biſcuit, ſoftens it, and occaſions the pieces to loſe their ſhape.

Differences in the ware from the quality the materia

If the earth is too porous it abſorbs the enamel and remains

* A Natural Hiſtory of Ireland, in three parts, by Dr. Gerrard Boate, Thomas Molineux, M.D. F.R.S. and others. Pages 128—130. Dublin: 1755.

† Annales de Chemie, T. 55.

rough,

rough, and as it were dried. If it contains too much lime, it throws off the enamel, which falls from it in fcales inftead of adhering to it.

Compofition of the enamel or glaze.

On the other hand, the white enamel is compofed of filicious fand, a little lime, lead and tin oxides, and fome flux, ground together with water in mills. The brown fort is compofed of the fame materials, with the addition of manganefe and perigord ftone *.

Caufes which occafion variations in the glaze.

The greater or lefs fufibility of the fand; the greater or lefs purity of the lead, of the tin, and of the faline fubftances employed as fluxes; the different degrees of heat which the mixture receives in the glazing; the variations of the finenefs given to the glazing materials by the action of the mill, are fo many circumftances which caufe changes in the enamel in its ftate of fufion on the pieces, relative to the ftate in which it finds the bifcuit and to the fufible layer, with which this laft is covered.

Pottery.

Brown pottery.

The body of the brown pottery is a red clay, more or lefs ferruginous and compact according to the places where it is procured.

Yellow pottery.

The common or yellow pottery is made of a white clay, which contains a little lime and magnefia, and a confiderable quantity of filicious fand, which may be generally efteemed a fourth of the mafs.

Glaze for brown ware.

The glazing of the brown pottery is formed with a mixture of filicious fand, yellow or red oxide of lead, and manganefe pulverifed together.

Glaze for yellow ware.

That of the yellow earthen-ware is compofed of a mixture of filicious fand, and red oxide of lead, which, during its baking vitrifies at its furface, and forms a yellow glazing more or lefs tranfparent. To this mixture is commonly added, in France, a little oxide of magnanefe in powder, more or lefs fine, without grinding them together. This is called the *grain*, becaufe it fufes more difficultly than the other materials, without mixing with them, and by that means forms ftreaks, fpots, or brown fpecks, according to the coarfenefs of the powder itfelf.

Mottled ftreaks in foreign ware.

Cloudy tinges in the glaze.

In fome manufactories they mix oxide of copper with the common glazing, to give it a green colour, and in others they form defigns on the pieces, with oxide of copper, which pro-

* A black ftone or compact manganefe. T.

duces

duces a green, with oxide of iron, which causes a red, or with oxide of manganese, which gives a brown.

Great imperfections are produced in pottery, from the injudicious use of glazing over earths of an unsuitable nature, and this is more remarkable when the earths are not so well prepared for their glazings as they are for those of the finer wares. The articles of common pottery are less carefully prepared both in their materials and baking. This last is usually performed at a single operation, and with less fire. Causes of imperfection in pottery.

The means of producing good pottery and earthen-ware, consist in carefully chusing the earths for forming the body: in producing an exact coincidence of expansion by heat between them, and the vitrifiable glaze with which they are to be covered, and in baking them by a proper degree of fire, produced from combustibles not capable of changing the nature of the glazing. Cautions requisite to insure its goodness.

The neglect of these attentions occasion defects in the manufactured articles, which are either unsightly and nothing more, or both unsightly and dangerous.

The unsightly defects which are found in ill conditioned pottery or earthen-ware, are, *scaling*; the *dropping* or *drops*; *smoke*; *drying* of the ware, and *flaws* or *cracks*. Defects or deformities enumerated.

The scaling is the appellation used when the glazing of a piece detaches itself in scales, by the action of moist air, or on the least touch, and leaves the biscuit uncovered. Scaling of the glaze.

The *dropping* or *drops* take place when the moisture of the fuel having struck the pieces during the baking, the enamel is collected in *drops* on the surface, and remains vitrified in that form, instead of being equally spread. Dropping or drops.

The smoky appearance happens when a piece has not been purified by a clear flame, but remains blackened or stained. Smoky tinge.

The drying happens when the pieces are, as it were, roasted in the firing, and come out rough from the absorption of the enamel into their substance. Drying.

The flaws happen, when the earth or the biscuit, having a different pyrometrical expansibility from that of the enamel; the body contracts in cooling more than the glaze which is therefore split, or which is divided into an infinite number of small parts, sometimes not perceptible to the eye when the pieces are new, but which become very visible, when the goods have imbibed any greasy substance in using. Flaws.

All these defects, though disagreeable to the eye, have The coarse pottery is most defective.

really,

really, with regard to the ware itself, only the inconvenience of a dirty appearance, provided the biscuit is always compact, and well baked. But it is different in the common pottery in which the dropping, the scaling, and the flaws produce more injurious defects. As the earth is more porous and less baked in those, the liquids preserved in them enter into the pores where they become altered and decomposed, and produce sulphurated hydrogen, which injures every thing kept in them.

Cavities or pits from bad firing.

The most noxious defects in pottery are the cavities or pits, and the underbaking. The pits are roughnesses or hollow bubbles which are found on those pieces, whose enamel being injured by rubbing, or being too little acted on by the fire, has not been fused into a vitreous substance. In these the metallic oxides are in a state capable of doing injury, being still soluble in fat or acid substances.

Underbaking or imperfect fusion of the glaze.

The underbaking occasions one of the most dangerous defects in pottery; the pieces thus affected have not had sufficient heat to cause the enamel to do more than agglutinate together, and in some cases it even still remains in powder. It is therefore capable of being divided, and taken up by all the liquids with which it may come in contact.

It is easy to shew the danger to which the public must be exposed in buying those articles at a low price which are called waste or refuse and which ought to be carefully thrown away. In vain may it be said that they are used daily without any immediate mischief happening; from the injury being more concealed, it is no less destructive. It is known that lead and its oxides act insensibly on the organs of digestion, especially when taken in small quantities: They do not, however, less certainly cause, at length, emaciation, cholics, convulsions, sometimes of all parts of the body, with obstinate diarrhœas; and the wretched people who use such vessels become the victims of their own ignorance, and of the imprudent avarice of the manufacturer.

It would be to the honour of enlightened manufacturers, not to offer to the public pieces which have imperfections beyond a certain degree, and to make this sacrifice to the good of national commerce, especially as they can avoid the loss by a greater attention to their materials.

XVIII. *Extract*

XVIII.

Extract of a Letter from M. JOHN MICHAEL HAUSSMANN, *to* M. BERTHOLLET, *on the Existence of intermediate Terms of Oxidation.* *

I THINK there are sufficient grounds for admitting, with you, that there exist, in the oxidation of many metallic bodies, intermediate degrees between the *minimum* and the *maximum*.

Existence of intermediate degrees of oxidation of metals.

The first example I shall cite, is, that of a *minimum* oxide of tin, precipitated from the muriatic solution, and dissolved in an excess of caustic potash; a metallic alkaline solution which I have before noticed in my Observations on the Red Dye of Adrianople, inserted in the "*Annales de Chimie,*" and also in a Memoir on the coloured Oxides of Tin, inserted in the "*Journal de Physique.*"

Oxide of tin.

By avoiding any dilution of the muriate of tin, and using a very concentrated solution of caustic potash, the mixture disengages much caloric, part of the tin is precipitated in the metalline state, whilst the remainder is held in solution in an intermediate state of oxidation. This alkaline solution is so disoxidant, that it changes the yellow oxide of gold, fixed on cotton, by means of ammonia, to a grey; whilst a similar yellow pattern underwent no change of colour on being steeped in the simple liquor of caustic potash. A like alteration took place on dipping a cotton cloth, which had been previously stained with the solution of gold, and well dried in the alkaline solution of tin, which also produced the same effect on pouring into it the pure solution of gold diluted with water.

Experiment. Muriate of tin is in part precipitated metallic and the rest suspended with intermediate oxidation.

This change of the yellow colour of oxide of gold by the alkaline solution of tin, is not the only proof of an intermediate state of oxidation; this liquor possesses besides, a property of destroying the blackish-brown colour of the oxide of manganese stained upon cotton by an alkaline precipitant.

Other proofs of that intermediate state.

All these changes are more rapidly produced, if, prior to the precipitation and solution in the caustic potash liquor, the muriatic solution of tin be diluted with six or eight parts of water, in which case there is no sensible disengagement of

* Annales de Chimie, Vol. LVI. 5.

caloric

caloric, and no tin is precipitated in the metalline ſtate. This ſolution, whoſe oxidation approaches the degree of *minimum*, for the moſt part retains an aqueous tranſparency, without any precipitation of oxide; even when long expoſed to the atmoſpheric air, it does not loſe the property of changing the yellow oxide of gold to a grey colour, or of deſtroying the blackiſh brown tint of oxide of mangancſe, when fixed upon cotton.

Oxide of manganeſe.

The oxide of manganeſe is capable of various degrees of oxidation; if a piece of cotton cloth be dipped in the tranſparent ſolution of ſulphate of manganeſe, it will, when dry, retain its original whiteneſs; but on their dipping the ſame cloth in the liquor of carbonated or cauſtic potaſh, it will, after waſhing and expoſure to the atmoſpheric air, be coloured brown; which colour will acquire a deeper ſhade, approximating to black, on being ſteeped for a time in an oxigenated alkaline, muriatic liquor. The oxigenated alkaline liquor, on being for any length of time ſubmitted to the action of the brown precipitate of manganeſe, inſtead of the rag ſteeped therein (which is to diſſolve by means of an increaſed oxidation) will aſſume a purple colour, of greater or leſs tranſparency as the time of their union has been longer or ſhorter.

Other oxides.

There ſeems reaſon, generally, to expect particular reſults from ſubmitting any of the metallic oxides to the action of this oxigenated muriatic alkaline liquor; which might, perhaps, be a means of giving them acid properties, and at the ſame time of proving the gradual oxidation of many metals; this is the more obſervable in white oxide of lead, which becomes gradually coloured by long expoſure to the oxigenated liquor, and being frequently ſtirred.

Muriatic and nitro-muriatic ſolutions of tin, though colourleſs in themſelves, acquire by admixture a vinous tint.

Muriatic and nitro-muriatic ſolutions of tin, well diluted with water, have an aqueous tranſparency, when properly made; but if the two be mixed together, they acquire a fine vinous colour, ſimilar to that of Malaga; this can only ariſe from the oxigen of the nitro-muriatic being in part communicated to the muriatic ſolution of tin.

The addition of ſolution of gold produces a purple dye;

If a ſolution of gold with great exceſs of acid, and diluted with from 130 to 160 parts of water, be gradually poured into the above mixture, ſtirring it all the time, the intenſity of the colour will be increaſed, till at length the liquor becomes

comes of a beautiful purple hue, in which all kinds of goods may be dyed; this may be changed to the tint of peach or lilac blossoms, by increasing the proportions of the nitro-muriatic solution; or, on the other hand, by causing the muriatic solutions of tin to preponderate; shades of grey will be obtained, deeper or paler in colour, according to the quantity of the solution added. Care must, however, be taken, in the latter experiment, that too great a proportion of the muriatic liquor of tin be not used; for by depriving the oxide of gold of too much of its oxigen, it might be too much disoxided and precipitated. The precipitate caused by such an accident is not altogether void of oxigen, which prevents its gilding cold silver, as do the ashes of burned cloth impregnated with the solution of gold. The degree to which the preservation of the tincture of gold may be carried, must depend on the proportions of the two solutions of tin, their being more or less surcharged with acids, and the quality of the solution of gold, wherein also there should be a very great excess of acid.

capable of being changed to the bloom of peaches or lilac; or even to grey, by proportioning the quantity of the two solutions of tin.

The precipitate of the oxide of gold will not gild silver, without the assistance of heat.

The purple tincture of gold, though of the most perfect transparency, is decomposed by exposure to a strong heat, and throws down what is known by the name of "*Purple of Cassius*," whose beauty depends on the quantity of nitro-muriatic solution of tin made use of. The latter, however, if mixed alone with the solution of gold, without the presence of muriate of tin, produces no alteration of colour, and, if the mixture be not too much weakened with water, is a very long time before it gives a precipitate.

Purple powder of Cassius.

The purple tincture of gold, is, properly speaking, nothing more than the powder of cassius, held in solution by means of the oxigen of the nitro-muriatic liquor of tin; and there is every reason to believe, that in the powder of cassius, the oxide of gold is in some way combined with the oxide of tin, which, by transmitting to it its own origin, during its fixation upon porcelain, prevents it, I think, from returning to its metallic state. I find a difficulty in subscribing to the opinion of Dr. Richter, of Berlin, who, in a memoir (which I have not read) attempts to prove, by mathematical demonstration, that the crimson-coloured gold on porcelain is in the metallic state.

Dr. Richter's opinion of the crimson-coloured gold upon porcelain.

The purple tincture of gold might be advantageously employed in dying silks, without greatly enhancing the price. The

Purple tincture of gold surpasses all others for dying silks.

The colour obtained from it surpasses all others in duration, since nothing less than combustion can destroy it. It is necessary, however, to leave the silk a long time in this dye; and the depth of the shade will be in proportion to the number of times the article is dipped; it must be well wrung, rinced, and dried, between each immersion.

The gradation of shades already noticed are indications of a gradual oxidation.

The gradation of colours produced by mixture of the nitro-muriatic, and muriatic solutions of tin, being much weakened by dropping solution of gold in a great excess of acid, considerably diluted with water, into the mixture, seems to me to indicate a gradual oxidation. The acetic solution of iron proves the same truth; for on being exposed to the atmospheric air, or to the contact of oxigen gas, it gradually changes from a sea green to a reddish yellow colour. I have shewn, in a memoir on the alkaline Tincture of Mars of Stahl, that sulphate of iron may be super-oxigenated, and also lose its excess of oxigen by the action of light. On mixing concentrated sulphuric acid with nitric solution of iron, I obtained, after the nitric acid was evaporated, by leaving the residuum to imbibe the moisture of the air for several months; crystals of super-oxigenated sulphate of iron, which were at first distinguishable by their whiteness from sulphate of alumine; but the action of the light gradually tinged their surface with a yellow colour; their original whiteness, however, might, by a gentle washing, be restored at pleasure. Super-oxigenated sulphate of iron, of nearly an equal degree of whiteness, may in like manner, be obtained by precipitating nitrate of iron, and dissolving the precipitate, edulcorated and freed from water, gradually in sulphuric acid, which, if well concentrated, will produce crystals of super-oxigenated sulphate of iron without evaporation. This salt possesses an incomparable degree of astringency.

Sulphate of iron loses its excess of oxigen by exposure to the light.

The progress of the transmission of oxigen is more manifest on linen simply printed with acetate of iron and madder, which must be a long time exposed in the air to bleach, unless the artificial means of bleaching be adopted. The printed part of the linen frequently perishes, bearing the appearance of having been cut with a sharp instrument, or burned with concentrated acid; this, it should seem, must proceed from the action of the oxigen contained in the coloured oxide of iron, continually replenished from the atmospheric air.

The fact that linens printed with acetate of iron are liable to become rotten, a proof of the gradual transmission of oxigen.

It

It is not among minerals alone that substances are found which are gradually oxided, and by intermediate degrees.

Indigo affords an instance that vegetable and animal bodies offer similar proofs; for any solution of indigo (excepting the sulphate of indigo) will, on disoxidation, or on having its oxigen restored, pass through all the degrees of shade, from bluish green to very yellow olive, preserving in the mean time the same quantity of indigo in solution. The beauty and stability of the colours, either for dying or painting, will chiefly depend on the degree of oxidation. On some other occasion, Sir, I shall write to you more amply on this subject.

Vegetable and animal bodies furnish similar cases with minerals, of gradual oxidation.

SCIENTIFIC NEWS.

Mémoires de l'Académie impériale des Sciences, &c. Memoirs of the imperial Academy of Sciences, Literature, and fine Arts, of Turin, for the Years 12 and 13, 2 Vols. Quarto. 1805. Turin.

WHEN the Royal Academy of Turin assumed the name of Imperial, in consequence of Piedmont being annexed to France, the number of academicians was increased, to form a new class, that of literature and the fine arts. Of the two volumes published, one is appropriated to the labours of this class, the other to that of the physical and mathematical sciences.

Memoirs of the Imperial Academy of Sciences, &c. of Turin.

The latter is compiled by the secretary, Mr. Vassali Eandi, who first mentions the changes that have taken place in the list of academicians, next the various papers that have been read at their meetings, and then the books and other articles presented to the society. These lists are followed by a well written account of the labours of the academy up to the year 1805, which occupies 250 pages. After this follow the different memoirs.

1. Description and use of a new portable barometer, for measuring heights and depths, with observations made with this instrument in the circles of Turin and Saluzzo. This instrument, of which a figure is given, was invented by the secretary; who has subjoined to his paper some very curious historical notes on the places where his observations were made.

2. Account

Memoirs of the Imperial Academy of Sciences, &c. of Turin.

2. Account of a waterſpout, that occurred in the territory of Revel, in the circle of Saluzzo, March 27, 1798, with remarks on the cauſe of the phenomenon, by the ſame.

3. On the different capacities for conducting heat aſcertained by experiment in different articles uſed for clothing, by J. Sennebier.

4. Of a new ſpecies of hawkweed, *crepis*, to which are added ſome cryptogamiæ of Piedmont, by J. Baptiſt Balbis.

A figure of this plant, which Mr. B. calls *crepis ambigua*, is given. Among the cryptogamiæ are the following new ſpecies, *mucor floſculentus*, *peziza amentacea*, *lichen nivalis*. Theſe likewiſe are figured.

5. Experiments on the effects of the nitric and oxigenated muriatic acid, employed topically in the treatment of various diſeaſes, by Mr. Roſſi. Mr. R. gives an account of the cure of ſeveral gangrenous ulcers, venereal buboes, and even contagious carbuncles, cured by the application of theſe acids.

6. Meteorological obſervations made during the ſolar eclipſe on the 30th of Jan. 1805, at the obſervatory of Turin; with reflections on them, by Ant. Mar. Vaſſali Eandi.

7. On a ſpecies of caſſia, that may be ſubſtituted for the ſenna of the ſhops, by Mr. Bellardi. This is the *caſſia marilandica*, which Mr. B. would call *ſuccedanea*, becauſe, according to him, it may ſupply the place of the *caſſia lanceolata*.

8. Inquiries into the nature of the galvanic fluid, by A. M. Vaſſali Eandi.

9. On the mines of plumbago in the departments of the Sture and the Po, by Mr. Bonvoiſin.

10. Attempts to improve nut oil, by the ſame. Mr. B. points out a method of purifying this oil, and rendering it as fit for lamps as other fine oils.

11. Examination of the action of the galvanic fluid on different gaſes, by J. A. Giobert.

12. An anatomical and phyſiological eſſay on the lymphatic glands, by profeſſor Roſſi.

13. Solution of a problem depending on the theory of permutations and combinations, by profeſſor Balbo.

14. Explanation of the circumſtance of a fiſh being occaſionally found with prickles in the river of the 27th military

 diviſion.

division, by Mr Giorna. This fish is the *cyprinus idus*; the male only has prickles, and loses them after spawning time.

Memoirs of the Imperial Academy of Sciences, &c. of Turin.

15. A chemico-medical essay on the pulmonary consumption, by Jos. Hyac. Rizzetti. The principal subject of this essay is the nature of the matter expectorated.

The following papers are by foreign members.

1. Memoir on the use of varying the constant quantity in summing up equations with variable coefficients, by Dr. Brunacci.

2. A systematical enumeration of the coleoptera found in the territory of Saluzzo, with observations, by Law. Ponza. To this catalogue are annexed two plates, containing the following new species. *Coccinella numeralis,—c. obsoleta,—curculio spinosus,—c. dubius,—c. rugosus,—cerambyx praeustus,—c. melanocephalus,—chrysomela melanocephala,—ch. variegata,—ch. pretiosa,—ch. luctuosa,—scarabaeus rufescens,—cantharis impressifrons,—attelabus funereus,—dytiscus silphoides,—tenebrio rufus,—birrhus rossii,—carabus attenuatus,—c. metallicus,—c. rossii,—forficula bipunctata,—silpha sinuata,—s. scabra.*

3. On the motion of the hairs of the *hypnum adiantoides*, by Palamedas de Suffren. Parts endued with irritability had already been observed in the hairs of some mosses. Mr. De S. has found it in those of the *h. a.* and describes all the singularities of the phenomenon. This paper is accompanied with a plate.

4. Of a resin employed by the bee in constructing its combs. By Fr. Mouxy Deloche.

5. Entomological observations; by Mr. Disderi. Mr. D. first sketches the history of the silkworm; and then proceeds to certain hymenoptera, chiefly of the *genera tenthredo, ichneumon, sphex, et vespa.*

6. Specimen of the fungi of the vale of Pisa, by Hugh Canino. The new species are figured on three plates. They are Agaricus elatior: a. miniatus: a. pezizoides: a. astrosanguineus: a. tricolor: Boletus scobinaceus: Helvella grandis: h. reflexa: h. inflata: Peziza achracea: p. pyriformis: Reticularia rosea: Mucor fruticulosus.

7. Observations on the native gold found among sand, by Lew. Bossi, of Milan.

To

Barometer.

MY correſpondent from Edinburgh is reminded, with regard to his project for a barometer, that no enlargement or diminution of the bore will make the leaſt difference in the ſcale of the common barometer, conſiſting of a tube or veſſel, cloſed above, and having its lower end open, and communicating with a baſon of mercury of conſiderable diameter.

Subdiviſion of an arc by wheel and chain.

The contrivance, received ſome time ago from T. I. for making an aſtronomical inſtrument, in which the angular quantities ſhall be meaſured by the communication of a chain, ſtrap, or ſtring, poſſeſſes ſo much ingenuity and promiſe, that it has exerciſed the heads and hands of a number of eminent men. Among theſe are Robert Hooke, for a quadrant; Muſchenbroeck, for a pyrometer, and many operative men, ſuch as Siſſon and others, for theodolites and quadrants.—Where the intention of the inſtrument is ſimply to magnify the motion, without any particular attention to preciſion, the contrivance has a happy effect; particularly in public lectures, where a number of ſpectators may obſerve the ſame effect at the ſame time. It is likewiſe cheap, and may be carried into effect in ſituations where the uſe and application of more accurate apparatus cannot be referred to.

It cannot be made very exact.

A ſlight attention to the ſubject, will ſhew that all contrivances of the kind here alluded to muſt be conſiderably inaccurate. For they demand, 1, that the wheels ſhould be very truly circular: 2, and free from all dirt and impurity: 3, that they be well centered: 4. that the chain or ſtring ſhould be every where of the ſame thickneſs: 5, and its tenſion in all poſitions alike, &c. &c. If the quantity of error, taken at a minimum, which muſt ariſe from theſe and other cauſes, be attended to, it will be found that a ſimple diviſion of an arc (ſubdivided by a ſcrew or a nonius) and examined or read off by a ſmall magnifier, will afford greater preciſion; even when the work is performed by a careful deſigner, who is no mathematical inſtrument maker. It is certain that much greater delicacy and preciſion may be had in the diviſion of mathematical inſtruments by the patient diligence of a cultivator of practical mechanics than is generally ſuppoſed.

A
JOURNAL
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AND
THE ARTS.

SUPPLEMENT TO *VOL. XIII.*

ARTICLE I.

On the Saline Efflorefcences upon Walls; Salivary Concretions; Deflagration of Mercury by Galvanifm; Biliary Calculi; and the freezing Point of Spermaceti. By JOHN BOSTOCK, *M.D.*

To Mr. NICHOLSON.

SIR,

IN the third and fixth volumes of your Journal you have inferted an account of fome experiments that I performed on the faline efflorefcences found upon walls. I have lately had an opportunity of examining two other fpecimens, of which I now fend you the particulars. The firft was obtained in confiderable quantity from the inner walls of a warehoufe that had been erected about twenty years. By a feries of fimple experiments, which it is unneceffary to detail at full length, I found it to be a fulphate of foda, which, as in the former cafes, feemed to exift in a ftate of almoft perfect purity. The circumftances attending the fecond of thefe efflorefcences were more fingular. It was given me by a friend who had fcraped it from off the ftones which are fituated on the infide of the weft aifle of York Minfter. My friend, on whofe ac-

Examination of two fpecimens of efflorefcence found upon walls.

The firft was fulphate of foda;

The fecond was fcraped from the

curacy

face of the stone withinside York Minster, and not from the mortar.

curacy I place the fullest confidence, expressly stated, that it was taken from the surface of the stone itself, and not from the joints, or any part that had been covered with mortar. It existed there in large quantity, and was disposed in the form of projecting spiculæ. Upon subjecting it to the usual trials, I found it to be a very pure sulphate of magnesia. In order to ascertain with precision the degree of its purity, I prepared a quantity of the sulphate of magnesia, by uniting together its constituent parts. This artificial salt, and the salt from York, after being crystallized, were exposed for some time to the same degree of heat, and when all the water of crystallization appeared to be expelled, equal weights of them were dissolved in equal weights of water: 100 grains of these solutions had the muriate of barytes respectively added, until no farther precipitation was produced, when it appeared that exactly the same weight of barytes was necessary to saturate each solution. The portions of precipitated sulphate of barytes were collected and dried, and when examined by a nice balance, exhibited scarcely any perceptible difference in weight; they each amounted to 7.9 grains. A similar process being adopted with respect to the common Epsom salt of the shops, the precipitate was found to be 7.35 grains only. Before I quit this subject, I may remark that another friend, in visiting the cathedral at Tewkesbury, noticed a saline efflorescence on the inside of some part of that building; he collected a portion of it, intending to give it me for examination; but it was accidentally lost. Perhaps some of your readers, who reside in that neighbourhood, may be induced to examine it, and transmit the result to your Journal. I confess myself totally unable to explain the production of the sulphate of magnesia on the surface of a freestone, such as, I believe, forms the body of York Minster.

It was sulphate of magnesia, very pure.

Qu. Whence came the magnesia?

Account of a concretion in the salivary ducts.

Among the solid concretions which are formed in different parts of the human body, those from the salivary ducts are occasionally met with. I lately procured one of these substances, of which I will give you a brief account. It was a cylinder, pointed at one end, of half an inch in length, and somewhat more than $\frac{1}{12}$ of an inch in diameter; it weighed $1\frac{1}{2}$ gr. It was white and smooth on the outside, and its internal fracture did not exhibit any marks of regular organization. To half a grain of the concretion a few drops of diluted muriatic

muriatic acid were added; no effervefcence was excited. By the application of a gentle heat the whole was diffolved, except a few films that fwam in the fluid. A copious precipitation was produced in this folution by pure ammonia, but none by the carbonate of ammonia. A part of the muriatic folution was evaporated; the refidue was not foluble in water, but was fpeedily re-diffolved by the muriatic acid. The muriatic folution, faturated with the carbonate of ammoniac, had a precipitation produced by the oxalate of ammoniac. It appears therefore that the concretion confifted of the phofphate of lime, mixed with a little animal matter, probably coagulated albumen; it did not contain any carbonate of lime, and its component parts appeared not to poffefs any regularly organized ftructure. M. Fourcroy * and Dr. Thomfon † have examined fimilar bodies, and agree in confidering the earthy matter to be the phofphate of lime; we may therefore reafonably conclude that this fubftance always compofes the earthy part of the falivary concretions. I am difpofed, however, to differ from thefe diftinguifhed chemifts in my idea refpecting the nature of the animal matter which enters into their compofition; M. Fourcroy confiders it as confifting of a fpecies of mucilage, while Dr. Thomfon defcribes it as "a membranous fubftance, which retains the fhape of the concretion after the folution of the phofphate." This was certainly not the cafe with the one which I examined. I am difpofed to confider the animal matter as coagulated albumen, rather than mucus, in confequence of its infoluble nature, and the greater facility with which it would on this account be detained by the phofphate of lime.

It was phofphate of lime chiefly,

—with coagulated albumen.

The power which the electric fluid poffeffes, when generated by the galvanic apparatus, of burning metallic plates, affords one of the moft beautiful experiments of which the fcience of chemiftry can boaft. All the metals have by this means been fubjected to combuftion, except mercury, which, owing to its fluidity, is incapable of being formed into thin laminæ. ‡ I have, however, been fortunate enough to accomplifh this object, and that by the moft fimple method.

Mercury has not heretofore been deflagrated by galvanifm.

* Syfteme, IX. 368. † Chemiftry, IV. 658.
‡ Thomfon's Chemiftry, I. 125.

I was

I was performing fome experiments with Mr. Richard Dalton, an ingenious lecturer in natural philofophy of this place, with a pile compofed of 60 pair of fix-inch plates of zinc and copper, when it occurred to me to place a minute globule of mercury in an iron fpoon, refting on the top of the pile, and to approach to it a thick iron wire connected with the other end of the apparatus; the effect was, that a brilliant ftar of light was produced from the mercury, attended with a crackling noife and a copious emiffion of fparks; the mercury was found converted into the black oxide.

Experiment in which this was effected.

The dark coloured particles of gall ftones do not appear to be infpiffated refin of the bile.

The moft common fpecies of biliary calculus is that compofed of the peculiar cryftalline matter, which in fome of its properties refembles fpermaceti, through which are interfperfed a number of dark coloured particles, that are fuppofed to confift of hardened bile. This is the idea entertained by M. Fourcroy,* and the one which I adopted, when I made the experiments on this fubject which are related in the fourth volume of your Journal. I have, however, fince that time been difpofed to alter my opinion; in two fpecimens of the biliary calculi, which I examined, after feparating the cryftalline matter by alcohol, I was unable to diffolve the dark coloured particles by any menftruum which I applied to them; they imparted a yellowifh tinge to water and other fluids, but the great bulk of their fubftance remained unchanged. It is, I conceive, not probable that the mere infpiffation of the refin of the bile could fo far alter its properties. I mention this circumftance principally with a view of attracting the attention of any of your readers who may be in poffeffion of a number of gall-ftones, fo as to afcertain whether the untractable nature of thefe particles is a general property of the cyftic-adipobilious concretions, or fomething peculiar to the fpecimens upon which I experimented.

Melting point of fpermaceti. Repetition of experiment confirms that it is a little above 112°.

I fhall conclude this mifcellaneous letter with fome remarks upon the melting point of fpermaceti. In the paper to which I have already referred, I mentioned the diverfity of opinion that had been entertained on this fubject, and afterwards ftated that my own experience induced me to fix it at the 112th degree. Dr. Thomfon, in the firft volume of his Chemiftry, fixes the melting point at 133°,† while in the fourth he ftates

* Syfteme, X. 591

† Page 358.

it to be 112°, upon the authority of my paper.* Yet in his anſwer to the Edinburgh reviewers, he has mentioned this eſtimate of the melting point of ſpermaceti as one of his acknowledged errors, and upon the authority of Dr. Gibbes, fixes it at 115°. This circumſtance determined me to repeat the experiment; I employed a very delicate thermometer, and uſed every requiſite precaution; the reſult was that the inſtrument deſcended to *a little above the* 112*th degree*, and remained ſtationary until the ſubſtance was become ſolid. I may add that Dr. Irvine, in ſome experiments related in the ninth vol. of your Journal, fixes the point at 113°, which agrees ſo nearly with my obſervations, as to afford me an additional confidence in their accuracy.

I am, Sir,

Your obedient ſervant,

JOHN BOSTOCK.

Liverpool, April 9, 1806.

II.

Inveſtigation of the Temperature at which Water is of greateſt Denſity, from the Experiments of Dr. Hope on the Contraction of Water by Heat at low Temperatures. In a Letter from Mr. JOHN DALTON.

To Mr. NICHOLSON.

SIR,

IN your Journal for February 1805 was inſerted a letter of mine containing certain facts relative to the ſubject of my preſent communication, which led me to diſbelieve the common opinion that water is denſeſt at 40°, and inclined me to think it is at 32°. Since that time my attention has again been turned to the ſubject; ſome ſmall but immaterial corrections of the facts have been made and additional ones obtained, by which I have been enabled to demonſtrate, at leaſt to my own ſatisfaction, that the temperature at which water is of greateſt denſity is at or near 36° of Fahrenheit. The reſults have lately been communicated to the Mancheſter Society, and may

Reference to a former communication, in which the maximum denſity of water was taken at 32°.

Preſent inference that it is 36°.

* Page 512.

perhaps

perhaps appear in a future volume. My present object is to ſhew that the reſults of Dr. Hope's experiments are explicable on the ſuppoſition of water being denſeſt at 36°, but on no other.

bſervations on e expanſions f water on each de of its point f greateſt denty.

Dr. Hope and myſelf concur in the opinion that water is denſeſt at ſome one point of temperature, and that above and below that point it expands alike by heat and cold in a gradually increaſing manner. De Luc was the firſt to obſerve that the expanſion is the ſame quantity for the ſame number of degrees, whether of increaſe or diminution of temperature; the remarkable fact was extended by my former experience from a range of 8° to 25° or more, above and below the ſtationary point. I have lately examined this fact with greater attention to preciſion than formerly, and find that it is accurate, except that the expanſion for degrees *below* the ſtationary point is always ſomewhat more than for a correſponding number of degrees *above* the ſaid point. Thus, water is ſtationary in a glaſs thermometer at 42°; if heated to 75° by the mercurial ſcale, it expands very conſiderably; if plunged into a frigorific mixture of 13°, it falls to 42°, and then expands again to the ſame point of 75°, at which it remains ſtationary as long as continued in the mixture. It may be remarked too, that congelation rarely if ever takes place in the bulb, when the mixture is not below 15°, which may eaſily be procured by putting ſnow into water ſaturated with common ſalt. Hence we ſee that 29° below, afford the ſame expanſion as 33° above the ſtationary point. This, I imagine, is occaſioned by the error attached to the equal diviſion of the mercurial ſcale. For a ſmall number of degrees, however, we may admit that the expanſions for correſponding intervals above and below are equal; hence we obtain the following table of correſponding temperatures at which water is of the ſame denſity.

	Suppoſing greateſt denſity at 40°			at 36°	
Correſponding denſities will be at	39°	and 41°	Correſponding denſities will be at	35°	and 37°
	38	— 42		34	— 38
	37	— 43		33	— 39
	36	— 44		32	— 40
	35	— 45		31	— 41
	34	— 46		30	— 42
	33	— 47		29	— 43
	32	— 48		28	— 44

Dr.

Dr. Hope also admits with me the fact that water subjected to be cooled without agitation in a frigorific mixture, usually descends several degrees below the freezing point, and still retains its liquidity. Though it is easy to obtain water in a glass bulb 20 or 25° below freezing, I could never cool water in an open jar more than 10 or 11° below freezing, agreeable to the experience of Sir Charles Blagden. But I find water in such circumstances will admit of being cooled to 25°, and the bulb of a thermometer to be immersed and withdrawn several times, without freezing.

The author and Dr. Hope concur that water may continue fluid below its freezing point.

We come now to the experiments of Dr. Hope.

Experiment I.

A jar eight inches deep and $4\frac{1}{2}$ in diameter, filled with water of 32°, and placed on a table, &c. Air 60—62°. Two thermometers inserted, one at the top, another at the bottom.

Dr. Hope's experiment quoted.

	Top Thermometer.	Differences.	*Bottom Thermometer.*	Differences.
	32°	0	32°	
		1+		2+
In 10 min.	33+		34+	
		2.5−		3−
30 ——	35.5		37	
		1.5		1+
50 ——	37		38+	
		1		0
1 hour ——	38		38+	
		4		.25−
1 10 ——	42		38.25	
		2		1.75
1 30 ——	44		40	&c.

In the first interval of 10 minutes we observe the bottom thermometer to have gained 2°+, and the top only 1°+; the former has the heat which enters directly, together with the heat which descends by the side of the vessel; the latter has only the heat which enters directly, and as these are nearly as one to two, we may infer that the acquisition of direct heat, and heat by descent, are nearly equal in the bottom thermometer during that interval.

Inference that the water descends as it acquires heat,

In the next interval of 20 minutes we observe the bottom thermometer gains 3°−, and the top 2°,5−. Here we see the

the ascending current still continues, but has produced little effect, having not added more than half a degree to the temperature.

—until the temperature rather exceeds 36°.

During the next 20 minutes the top gains 1°.5, the bottom only 1°+. In this interval we may observe the current has turned, but not yet acquired much force. The point of greatest density must therefore have existed at the last observation or near it: the mean of 35°.5 and 37° is 36¼° for the required point, as deduced from this experiment.

After which the heated water ascends.

In 10 minutes more the top gains 1°, and the bottom little or nothing; here we find the ascending current has become such as to manifest its influence very sensibly.

In the next 10 minutes the top gains 4°, and the bottom only ,25; here the ascending current has become quadruple what it was 2° below; because the farther the temperature is raised above the stationary point, the more powerful is the force of ascent arising from the same interval of temperature.

These facts do not agree with the supposed maximum density at 39° or 40°.

It would be in vain to attempt to reconcile the above experiment with the opinion that water is densest at 39° or 40°. At the very moment when the mean temperature of the water is 39°, we observe the *ascending* current the most active, when it ought to have been *descending* or imperceptible.

The effect is not modified by the table or support.

I once imagined that the experiment might be explained on the supposition of 32° being the point of greatest density; that the sudden increase of temperature at the bottom arose from the heat of the table, and that the cohesion of the particles of water prevented their ascent under the propulsion of so small a force; but having procured a large glass jar which could be suspended, I found the same order of differences nearly as when placed on a table, and was therefore obliged to abandon that explanation.

Intending to send the remainder of this investigation for a future number, I remain

Your friend,

JOHN DALTON.

Manchester, April 14, 1806.

III. *Account*

III.

Account of a Series of Experiments, shewing the Effects of Compression in modifying the Action of Heat. By Sir James Hall, *Bart. F. R. S. Edinburgh.*

(Concluded from Page 328.)

Section III.

Experiments made in Tubes of Porcelain.—Tubes of Wedgwood's Ware.—Methods used to confine the carbonic Acid, and to close the Pores of the Porcelain in a horizontal Apparatus.—Tubes made with a View to these Experiments.—The vertical Apparatus adopted.—View of Results obtained, both in Iron and Porcelain.—The Formation of Lime-stone and Marble.—Inquiry into the Cause of the partial Calcinations.—Tubes of Porcelain weighed previous to breaking—Experiments with Porcelain Tubes proved to be limited.

While I was carrying on the above-mentioned experiments, I was occasionally occupied with another set, in tubes of porcelain. So much, indeed, was I prepossessed in favour of this last mode, that I laid gun-barrels aside, and adhered to it during more than a year. The methods followed with this substance differ widely from those already described, though founded on the same general principles.

Set of experiments in tube of porcelain.

I procured from Mr. Wedgwood's manufactory at Etruria, in Staffordshire, a set of tubes for this purpose, formed of the same substance with the white mortars, in common use, made there. These tubes were fourteen inches long, with a bore of half an inch diameter, and thickness of 0.2; being closed at one end (figs. 9, 10, 11, 12, 13.) *Pl.* XI.

I proposed to ram the carbonate of lime into the breech (*Fig.* 9. A); then filling the tube to within a small distance of its muzzle with pounded flint (B), to fill that remainder (C) with common borax of the shops (borat of soda) previously reduced to glass, and then pounded; to apply heat to the muzzle alone, so as to convert that borax into solid glass; then, reversing the operation, to keep the muzzle cold, and apply the requisite heat to the carbonate lodged in the breech.

They were clos[ed] at one end and the other aperture was stop[ped] with glass of borax.

I thus expected to confine the carbonic acid; but the attempt was attended with considerable difficulty, and has led to

Difficulties of this process.

to the employment of various devices, which I ſhall now ſhortly enumerate, as they occurred in the courſe of practice. The ſimple application of the principle was found inſufficient, from two cauſes: Firſt, The carbonic acid being driven from the breech of the tube, towards the muzzle, among the pores of the pounded ſilex, eſcaped from the compreſſing force, by lodging itſelf in cavities which were comparatively cold: Secondly, The glaſs of borax, on cooling, was always found to crack very much, ſo that its tightneſs could not be depended on.

nd the method partly obviing them.

To obviate both theſe inconveniences at once, it occurred to me, in addition to the firſt arrangement, to place ſome borax (*Fig.* 10. C) ſo near the breech of the tube, as to undergo heat along with the carbonate (A); but interpoſing between this borax and the carbonate, a ſtratum of ſilex (B), in order to prevent contamination. I truſted that the borax in a liquid or viſcid ſtate, being thruſt outwards by the expanſion of the carbonic acid, would preſs againſt the ſilex beyond it (D), and totally prevent the elaſtic ſubſtances from eſcaping out of the tube, or even from wandering into its cold parts.

In ſome reſpects, this plan anſwered to expectation. The glaſs of borax, which can never be obtained when cold, without innumerable cracks, unites into one continued viſcid maſs in the loweſt red-heat; and as the ſtreſs in theſe experiments begins only with redneſs, the borax being heated at the ſame time with the carbonate, becomes united and impervious, as ſoon as its action is neceſſary. Many good reſults were accordingly obtained in this way. But I found, in practice, that as the heat roſe, the borax began to enter into too thin fuſion, and was often loſt among the pores of the ſilex, the ſpace in which it had lain being found empty on breaking the tube. It was therefore found neceſſary to oppoſe ſomething more ſubſtantial and compact, to the thin and penetrating quality of pure borax.

ottle glaſs was ound much preerable to pure orax for the urpoſe of reraining the arbonic acid.

In ſearching for ſome ſuch ſubſtance, a curious property of bottle-glaſs occurred accidentally. Some of this glaſs, in powder, having been introduced into a muffle at the temperature of about 20° of Wedgwood; the powder, in the ſpace of about a minute, entered into a ſtate of viſcid agglutination, like that of honey, and in about a minute more, (the heat always continuing unchanged), conſolidated into a firm and compact

pact mass of *Reaumur's porcelain* *. It now appeared, that by placing this substance immediately behind the borax, the penetrating quality of this last might be effectually restrained; for, Reaumur's porcelain has the double advantage of being refractory, and of not cracking by change of temperature. I found, however, that in the act of consolidation, the pounded bottle-glass shrunk, so as to leave an opening between its mass and the tube, through which the borax, and, along with it, the carbonic acid, was found to escape. But the object in view was obtained by means of a mixture of pounded bottle-glass, and pounded flint, in equal parts. This compound still agglutinates, not indeed into a mass so hard as Reaumur's porcelain, but sufficiently so for the purpose; and this being done without any sensible contraction, an effectual barrier was opposed to the borax; (this arrangement is shewn in *Fig*. 11.); and thus the method of closing the tubes was rendered so complete, as seldom to fail in practice †. A still further refinement upon this method was found to be of advantage. A second series of powders, like that already described, was introduced towards the muzzle, (as shewn in *Fig*. 12). During the first period of the experiment, this last-mentioned series was exposed to heat, with all the outward half of the tube *(a b)*; and by this means, a solid mass was produced, which remained cold and firm during the subsequent action of heat upon the carbonate.

Improvements on this metho[d]

I soon found, that notwithstanding all the above-mentioned precautions, the carbonic acid made its escape, and that it pervaded the substance of the Wedgwood tubes, where no flaw could be traced. It occurred to me, that this defect might be remedied, were borax, in its thin and penetrating state of fusion, applied to the inside of the tube; and that the pores of the porcelain might thus be closed, as those of leather are closed by oil, in an air-pump. In this view, I rammed the carbonate into a small tube, and surrounded it with pounded glass of borax, which, as soon as the heat was applied, spread on the inside

Remedy for p[o]rosity in the earthen tubes.

* In the same temperature, a mass of the glass of equal bulk would undergo the same change; but it would occupy an hour.

† A substance equally efficacious in restraining the penetrating quality of borax, was discovered by another accident. It consists of a mixture of borax and common sand, by which a substance is formed, which, in heat, assumes the state of a very tough paste, and becomes hard and compact on cooling.

fide of the large tube, and effectually clofed its pores. In this manner, many good experiments were made with barrels lying horizontally in common muffles, (the arrangement juft defcribed being reprefented in *Fig.* 13.)

Beft material for tubes.

I was thus enabled to carry on experiments with this porcelain, to the utmoft that its ftrength would bear. But I was not fatisfied with the force fo exerted; and hoping to obtain tubes of a fuperior quality, I fpent much time in experiments with various porcelain compofitions. In this, I fo far fucceeded, as to produce tubes by which the carbonic acid was in a great meafure retained without any internal glaze. The beft material I found for this purpofe, was the pure porcelain-clay of Cornwall, or a compofition in the proportion of two of this clay to one of what the potters call *Cornifh-ftone*, which I believe to be a granite in a ftate of decompofition. Thefe tubes were feven or eight inches long, with a bore tapering from 1 inch to 0.6. Their thicknefs was about 0.3 at the breech, and tapered towards the muzzle to the thinnefs of a wafer.

Improvement by placing the tube vertically.

I now adopted a new mode of operation, placing the tube vertically, and not horizontally, as before. By obferving the thin ftate of borax whilft in fufion, I was convinced, that it ought to be treated as a complete liquid, which being fupported in the courfe of the experiment from below, would fecure perfect tightnefs, and obviate the failure which often happened in the horizontal pofition, from the falling of the borax to the lower fide.

Particular defcription of the procefs.

In this view, (fig. 16.) I filled the breech in the manner defcribed above, and introduced into the muzzle fome borax (C) fupported at the middle of the tube by a quantity of filex mixed with the bottle glafs (B). I placed the tube, fo prepared, with its breech plunged into a crucible filled with fand (E), and its muzzle pointing upwards. It was now my object to apply heat to the muzzle-half, whilft the other remained cold. In that view, I conftructed a furnace (figs. 14 and 15.) having a muffle placed vertically (*c d*,) furrounded on all fides with fire (*e e*), and open both above (at *c*), and below (at *d*). The crucible juft mentioned, with its tube, being then placed on a fupport directly below the vertical muffle, (as reprefented in fig. 14. at F) it was raifed, fo that the half of the tube next the muzzle was introduced into the

the fire. In confequence of this, the borax was feen from above to melt, and run down in the tube, the air contained in the powder efcaping in the form of bubbles, till at laft the borax ftood with a clear and fteady furface like that of water. Some of this falt being thrown in from above, by means of a tube of glafs, the liquid furface was raifed nearly to the muzzle, and, after all had been allowed to become cold, the pofition of the tube was reverfed; the muzzle being now plunged into the fand, (as in fig. 17.) and the breech introduced into the muffle. In feveral experiments, I found it anfwer well, to occupy great part of the fpace next the muzzle, with a rod of fand and clay previoufly baked, (fig. 19. K K), which was either introduced at firft, along with the pounded borax, or, being made red hot, was plunged into it when in a liquid ftate. In many cafes I affifted the compactnefs of the tube by means of an internal glaze of borax; the carbonate being placed in a fmall tube, (as fhewn in fig. 18.)

Effect of expanfion in th fufed borax upon the tub

Thefe devices anfwered the end propofed. Three-fourths of the tube next the muzzle was found completely filled with a mafs, having a concave termination at both ends, (*f* and *g* figs. 17, 18, 19.) fhewing that it had ftood as a liquid in the two oppofite pofitions in which heat had been applied to it. So great a degree of tightnefs indeed was obtained in this way, that I found myfelf fubjected to an unforefeen fource of failure. A number of the tubes failed, not by explofion, but by the formation of a minute longitudinal fiffure at the breech, through which the borax and carbonic acid efcaped. I faw that this arofe from the expanfion of the borax when in a liquid ftate, as happened with the fufible metal in the experiments with iron-barrels; for, the crevice here formed, indicated the exertion of fome force acting very powerfully, and to a very fmall diftance. Accordingly, this fource of failure was remedied by the introduction of a very fmall air-tube. This, however, was ufed only in a few experiments.

Thefe experiments were upon the who fuccefsful.

In the courfe of the years 1801, 1802, and 1803, I made a number of experiments, by the various methods above defcribed, amounting, together with thofe made in gun barrels, to one hundred and fifty-fix. In an operation fo new, and in which the apparatus was ftrained to the utmoft of its power, conftant fuccefs could not be expected, and in fact many experiments failed, wholly or partially. The refults, however,

upon the whole, were satisfactory, since they seemed to establish some of the essential points of this inquiry.

These experiments prove, that, by mechanical constraint, the carbonate of lime can be made to undergo strong heat, without calcination, and to retain almost the whole of its carbonic acid, which, in an open fire, at the same temperature, would have been entirely driven off: and that, in these circumstances, heat produces some of the identical effects ascribed to it in the Huttonian Theory.

Pounded carbonate of lime in its several varieties became agglutinated into stony masses.

By this joint action of heat and pressure, the carbonate of lime which had been introduced in the state of the finest powder, is agglutinated into a firm mass, possessing a degree of hardness, compactness, and specific gravity * nearly approaching to these qualities in a sound limestone; and some of the results, by their saline fracture, by their semitransparency, and their susceptibility of polish, deserve the name of marble.

The same trials have been made with all calcareous substances; with chalk, common limestone, marble, spar, and the shells of fish. All have shewn the same general property, with some varieties as to temperature. Thus, I found, that, in the same circumstances, chalk was more susceptible of agglutination than spar; the latter requiring a heat two degrees higher than the former, to bring it to the same pitch of agglutination.

The chalk used in my first experiments, always assumed the character of a yellow marble, owing probably to some slight contamination of iron. When a solid piece of chalk, whose bulk had been previously measured in the gauge of Wedgwood's pyrometer was submitted to heat under compression, its contraction was remarkable, proving the approach of the particles during their consolidation; on these occasions, it was found to shrink three times more than the pyrometer-pieces in the same temperature. It lost, too, almost entirely, its power of imbibing water, and acquired a great additional specific gravity. On several occasions, I observed, that masses of chalk, which, before the experiment, had shewn one uniform character of whiteness, assumed a stratified appearance, indicated by a series of parallel layers of a brown colour. This

* See Appendix.

5 circumstance

circumſtance may hereafter throw light on the geological hiſtory of this extraordinary ſubſtance.

I have ſaid, that, by mechanical conſtraint, almoſt the whole of the carbonic acid was retained. And, in truth, at this period, ſome loſs of weight had been experienced in all the experiments, both with iron and porcelain. But even this circumſtance is valuable, by exhibiting the influence of the carbonic acid, as varied by its quantity.

And moſt of carbonic acid was retained.

When the loſs exceeded 10 or 15 *per cent.* * of the weight of the carbonate, the reſult was always of a friable texture, and without any ſtony character; when leſs than two or three *per cent.* it was conſidered as good, and poſſeſſed the properties of a natural carbonate. In the intermediate caſes, when the loſs amounted, for inſtance, to ſix or eight *per cent.* the reſult was ſometimes excellent at firſt, the ſubſtance bearing every appearance of ſoundneſs, and often poſſeſſing a high character of cryſtallization; but it was unable to reſiſt the action of the air; and, by attracting carbonic acid or moiſture, or both, crumbled to duſt more or leſs rapidly, according to circumſtances. This ſeems to prove, that the carbonate of lime, though not fully ſaturated with carbonic acid, may poſſeſs the properties of limeſtone; and perhaps a difference of this kind may exiſt among natural carbonates, and give riſe to their different degrees of durability.

Qualities of t product differ ing according the loſs of ca bonic acid.

I have obſerved, in many caſes, that the calcination has reached only to a certain depth into the maſs; the internal part remaining in a ſtate of complete carbonate, and, in general, of a very fine quality. The partial calcination ſeems thus to take place in two different modes. By one, a ſmall proportion of carbonic acid is taken from each particle of carbonate; by the other, a portion of the carbonate is quite calcined, while the reſt is left entire. Perhaps one reſult is the effect of a feeble calcining cauſe, acting during a long time, and the other of a ſtrong cauſe, acting for a ſhort time.

Some of the reſults which ſeemed the moſt perfect when firſt produced, have been ſubject to decay, owing to partial calcination. It happened, in ſome degree, to the beautiful

Some reſults were ſubject decay from partial calcina tion.

* I have found, that, in open fire, the entire loſs ſuſtained by the carbonate varies in different kinds from 42 to 45.5 *per cent.*

ſpecimen

ſpecimen produced on the 3d of March, 1801, though a freſ fracture has reſtored it.

A ſpecimen, too, of marble, formed from pounded ſpar, on th 15th of May, 1801, was ſo complete as to deceive the workma employed to poliſh it, who declared, that, were the ſubſtanc a little whiter, the quarry from which it was taken would b of great value, if it lay within reach of a market. Yet, in few weeks after its formation, it fell to duſt.

ry many re durable rble.

Numberleſs ſpecimens, however, have been obtained, whic reſiſt the air, and retain their poliſh as well as any marbl Some of them continue in a perfect ſtate, though they hav been kept without any precaution during four or five year That ſet, in particular, remain perfectly entire, which we ſhewn laſt year in this Society, though ſome of them we made in 1799, ſome in 1801 and 1802, and though the fir eleven were long ſoaked in water, in the trials made of the ſpecific gravity.

markable t.

A curious circumſtance occurred in one of theſe expe ments, which may hereafter lead to important conſequence Some ruſt of iron had accidentally found its way into t tube: 10 grains of carbonate were uſed, and a heat of 28° w applied. The tube had no flaw; but there was a certain that the carbonic acid had eſcaped through its pores. Wh broken, the place of the carbonate was found occupied, part by a black ſlaggy matter, and partly by ſphericles of vario ſizes, from that of a ſmall pea downwards, of a white ſu ſtance, which proved to be quicklime; the ſphericles bei interſperſed through the ſlag, as ſpar and agates appear whinſtone. The ſlag had certainly been produced by a mi ture of the iron with the ſubſtance of the tube; and the ſph rical form of the quicklime ſeems to ſhew, that the carbona had been in fuſion along with the ſlag, and that they h ſeparated on the eſcape of the carbonic acid.

The ſubject was carried thus far in 1803, when I ſhou probably have publiſhed my experiments, had I not been i duced to proſecute the inquiry by certain indications, a accidental reſults, of a nature too irregular and uncertain meet the public eye, but which convinced me, that it w poſſible to eſtabliſh by experiment the truth of all that w hypothetically aſſumed in the Huttonian theory.

ndeavour to nprove the ex- riments by

The principal object was now to accompliſh the entire ſion of the carbonate, and to obtain ſpar as the reſult of t fuſi

fusion, in imitation of what we conceive to have taken place in nature.

preventing all calcination, and perfectly fusing the carbonate.

It was likewise important to acquire the power of retaining all the carbonic acid of the carbonate, both on account of the fact itself, and on account of its consequences; the result being visibly improved by every approach towards complete saturation. I therefore became anxious to investigate the cause of the partial calcinations which had always taken place, to a greater or less degree, in all these experiments. The question naturally suggests itself, What has become of the carbonic acid, separated in these partial calcinations from the earthy basis? Has it penetrated the vessel, and escaped entirely, or has it been retained within it in a gaseous, but highly compressed state? It occurred to me, that this question might be easily resolved, by weighing the vessel before and after the action of heat upon the carbonate.

By experiment it was found that iron barrels vitiated the result.

With iron, a constant and inappreciable source of irregularity existed in the oxidation of the barrel. But with porcelain the thing was easy; and I put it in practice in all my experiments with this material, which were made after the question had occurred to me. The tube was weighed as soon as its muzzle was closed, and again, after the breech had been exposed to the fire; taking care, in both cases, to allow all to cool. In every case, I found some loss of weight, proving, that even in the best experiments, the tubes were penetrated to a certain degree. I next wished to try if any of the carbonic acid separated, remained within the tube in a gaseous form; and in that view, I wrapt the tube, which had just been weighed, in a sheet of paper, and placed it, so surrounded, on the scale of the balance. As soon as its weight was ascertained, I broke the tube by a smart blow, and then replaced upon the scale the paper containing all the fragments. In those experiments, in which entire calcination had taken place, the weight was found not to be changed, for all the carbonic acid had already escaped during the action of heat. But in the good results, I always found that a loss of weight was the consequence of breaking the tube.

With porcelain tubes this cause of irregularity existed along with the escape of the acid.

These facts prove, that both causes of calcination had operated in the porcelain tubes; that, in the cases of small loss, part of the carbonic acid had escaped through the vessel, and that part had been retained within it. I had in view methods by

by which the last could be counteracted; bnt I saw no remedy for the first. I began, therefore, to despair of ultimate success with tubes of porcelain *.

These last could not bear elevated heats.

Another circumstance confirmed me in this opinion. I found it impracticable to apply a heat above 27° to these tubes, when charged as above with carbonate, without destroying them, either by explosion, by the formation of a minute rent, or by the actual swelling of the tube. Sometimes this swelling took place to the amount of doubling the internal diameter, and yet the porcelain held tight, the carbonate sustaining but a very small loss. This ductility of the porcelain in a low heat is a curious fact, and shews what a range of temperature is embraced by the gradual transition of some substances from a solid to a liquid state: For the same porcelain, which is thus susceptible of being stretched out without breaking in a heat of 27°, stands the heat of 152°, without injury, when exposed to no violence, the angles of its fracture remaining sharp and entire.

IV.

Experiments in Gun-Barrels resumed.—The Vertical Apparatus applied to them.—Barrels bored in solid Bars.—Old Sable Iron.—Fusion of the Carbonate of Lime.—Its Action on Porcelain.—Additional Apparatus required in Consequence of that Action.—Good Results; in particular, four Experiments, illustrating the Theory of Internal Calcination, *and shewing the Efficacy of the Carbonic Acid as a Flux.*

Experiments with gun-barrels resumed,

SINCE I found that, with porcelain tubes, I could neither confine the carbonic acid entirely, nor expose the carbonate in them to strong heats; I at last determined to lay them aside, and return to barrels of iron, with which I had formerly obtained some good results, favoured, perhaps, by some accidental circumstances.

* I am nevertheless of opinion, that, in some situations, experiments with compression may be carried on with great ease and advantage in such tubes. I allude to the situation of the geologists of France and Germany, who may easily procure, from their own manufactories, tubes of a quality far superior to any thing made for sale in this country.

On

On the 12th of February, 1803, I began a series of experiments with gun-barrels, resuming my former method of working with the fusible metal, and with lead; but altering the position of the barrel from horizontal to vertical; the breech being placed upwards during the action of heat on the carbonate. This very simple improvement has been productive of advantages no less remarkable, than in the case of the tubes of porcelain. In this new position, the included air, quitting the air-tube on the fusion of the metal, and rising to the breech, is exposed to the greatest heat of the furnace, and must therefore react with its greatest force; whereas, in the horizontal position, that air might go as far back as the fusion of the metal reached, where its elasticity would be much feebler. The same disposition enabled me to keep the muzzle of the barrel plunged, during the action of heat, in a vessel filled with water; which contributed very much both to the convenience and safety of these experiments.

In a vertical position, with the breech upwards, and stopped with fusible metal.

In this view, making use of the brick-furnace with the vertical muffle, already described in page 384, I ordered a pit (*a a a fig.* 20.) to be excavated under it, for the purpose of receiving a water-vessel. This vessel (represented separately, *fig.* 21.) was made of cast iron; it was three inches in diameter, and three feet deep; and had a pipe (*d e*) striking off from it at right angles, four or five inches below its rim, communicating with a cup (*e f*) at the distance of about two feet. The main vessel being placed in the pit (*a a*) directly below the vertical muffle, and the cup standing clear of the furnace, water poured into the cup flowed into the vessel, and could thus conveniently be made to stand at any level. (The whole arrangement is represented in *fig.* 20.) The muzzle of the barrel (*g*) being plunged into the water, and its breech (*b*) reaching up into the muffle, as far as was found convenient, its position was secured by an iron chain (*g f*). The heat communicated downwards generally kept the surface of the water (at *c*) in a state of ebullition; the waste thus occasioned being supplied by means of the cup, into which, if necessary, a constant stream could be made to flow.

Description and drawing of the apparatus.

As formerly, I rammed the carbonate into a tube of porcelain, and placed it in a cradle of iron, along with an air-tube and a pyrometer; the cradle being fixed to a rod of iron, which rod I now judged proper to make as large as the barrel

 would

would admit, in order to exclude as much of the fufible metal as poffible; for the expanfion of the liquid metal being in proportion to the quantity heated, the more that quantity could be reduced, the lefs rifk there was of deftroying the barrels.

Simple mode of withdrawing the contents from the tube.

In the courfe of practice, a fimple mode occurred of removing the metal and withdrawing the cradle: it confifted in placing the barrel with its muzzle downwards, fo as to keep the breech above the furnace and cold, while its muzzle was expofed to ftrong heat in the muffle. In this manner, the metal was difcharged from the muzzle, and the pofition of the barrel being lowered by degrees, the whole metal was removed in fucceffion, till at laft the cradle and its contents became entirely loofe. As the metal was delivered, it was received in a crucible, filled with water, ftanding on a plate of iron placed over the pit, which had been ufed, during the firft ftage of the experiment, to contain the water-veffel. It was found to be of fervice, efpecially where lead was ufed, to give much more heat to the muzzle than fimply what was required to liquefy the metal it contained; for when this was not done, the muzzle growing cold as the breech was heating, fome of the metal delivered from the breech was congealed at the muzzle, fo as to ftop the paffage.

According to this method, many experiments were made in gun-barrels, by which fome very material fteps were gained in the inveftigation.

Experiment in the new method.

On the 24th of February, I made an experiment with fpar and chalk; the fpar being placed neareft to the breech of the barrel, and expofed to the greateft heat, fome baked clay intervening between the carbonates. On opening the barrel, a long-continued hiffing noife was heard. The fpar was in a ftate of entire calcination; the chalk, though crumbling at the outfide, was uncommonly hard and firm in the heart. The temperature had rifen to 32°.

Internal calcination, where the carbonic acid did not efcape out of the apparatus.

In this experiment, we have the firft clear example, in iron barrels, of what I call *Internal Calcination*; that is to fay, where the carbonic acid feparated from the earthy bafis, has been accumulated in cavities within the barrel. For, fubfequently to the action of ftrong heat, the barrel had been completely cooled; the air therefore introduced by means of the air-tube, muft have refumed its original bulk, and by itfelf could have

have no tendency to rush out; the heat employed to open the barrel being barely sufficient to soften the metal. Since, then, the opening of the barrel was accompanied by the discharge of elastic matter in great abundance, it is evident, that this must have proceeded from something superadded to the air originally included, which could be nothing but the carbonic acid of the carbonate. It follows, that the calcination had been, in part at least, internal; the separation of the acid from the earthy matter being complete where the heat was strongest, and only partial where the intensity was less.

Part of the included carbonat was calcined, another part retaining its acid Reasoning on this fact.

The chemical principles stated in a former part of this paper, authorised us to expect a result of this kind. As heat, by increasing the volatility of the acid, tended to separate it from the earth, we had reason to expect, that, under the same compression, but in different temperatures, one portion of the carbonate might be calcined, and another not: And that the least heated of the two, would be the least exposed to a change not only from want of heat, but likewise in consequence of the calcination of the other mass; for the carbonic acid disengaged by the calcination of the hottest of the two, must have added to the elasticity of the confined elastic fluid, so as to produce an increase of compression. By this means, the calcination of the coldest of the two might be altogether prevented, and that of the hottest might be hindered from making any further advancement. This reasoning seemed to explain the partial calcinations which had frequently occurred where there was no proof of leakage; and it opened some new practical views in these experiments, of which I availed myself without loss of time. If the internal calcination of one part of an inclosed mass, promotes the compression of other masses included along with it, I conceived that we might forward our views very much by placing a small quantity of carbonate, carefully weighed, in the same barrel with a large quantity of that substance; and by arranging matters so that the small fiducial part should undergo a moderate heat, while a stronger heat, capable of producing internal calcination, should be applied to the rest of the carbonate. In this manner, I made many experiments, and obtained results which seemed to confirm this reasoning, and which were often very satisfactory, though the heat did not always exert its greatest force where I intended it to do so.

On

Experiment. Carbonate partly fused, and in part deprived of carbonic acid.

On the 28th of February, I introduced fome carbonate, accurately weighed, into a fmall porcelain tube, placed within a larger one, the reft of the large tube being filled with pounded chalk; thefe carbonates, together with fome pieces of chalk, placed along with the large tube in the cradle, weighing in all 195.7 grains. On opening the barrel, air rufhed out with a long-continued hiffing noife. The contents of the little tube were loft by the intrufion of fome borax which had been introduced over the filex, in order to exclude the fufible metal. But the reft of the carbonate, contained in the large tube, came out in a fine ftate, being porous and frothy throughout; fparkling every where with facetts, the angular form of which was diftinguifhable in fome of the cavities by help of a lens: in fome parts the fubftance exhibited the rounding of fufion; in many it was in a high degree tranfparent. It was yellow towards the lower end, and at the other almoft colourlefs. At the upper end, the carbonate feemed to have united with the tube, and at the places of contract to have fpread upon it; the union having the appearance of a mutual action. The general mafs of carbonate effervefced in acid violently, but the thin ftratum immediately contiguous to the tube, feebly, if at all.

Similar experiment, in which the carbonate exhibited more remarkable facettes.

On the 3d of March, I introduced into a very clean tube of porcelain 36.8 of chalk. The tube was placed in the upper part of the cradle, the remaining fpace being filled with two pieces of chalk, cut for the purpofe; the uppermoft of thefe being excavated, fo as to anfwer the purpofe of an air-tube. The pieces thus added, were computed to weigh about 300 grains. There was no pyrometer ufed; but the heat was gueffed to be about 30°. After the barrel had ftood during a few minutes in its delivering pofition, the whole lead with the rod and cradle, were thrown out with a fmart report, and with confiderable force. The lowermoft piece of chalk had fcarcely been acted upon by heat. The upper part of the other piece was in a ftate of marble, with fome remarkable facettes. The carbonate, in the little tube, had fhrunk very much during the firft action of heat, and had begun to fink upon itfelf, by a further advancement towards liquefaction. The mafs was divided into feveral cylinders, lying confufedly upon each other; this divifion arifing from the manner in which the pounded chalk was rammed into the tube in fucceffive portions. In

feveral

ſeveral places, particularly at the top, the carbonate was very porous, and full of decided air-holes, which could not have been formed but in a ſoft ſubſtance; the globular form and ſhining ſurface of all theſe cavities, clearly indicating fuſion. The ſubſtance was ſemitranſparent; in ſome places yellow, and in ſome colourleſs. When broken, the ſolid parts ſhewed a ſaline fracture, compoſed of innumerable facettes. The carbonate adhered, from end to end, to the tube, and incorporated with it, ſo as to render it impoſſible to aſcertain what loſs had been ſuſtained. In general, the line of contact was of a brown colour; yet there was no room for ſuſpecting the preſence of any foreign matter, except, perhaps, from the iron-rod which was uſed in ramming down the chalk. But, in ſubſequent experiments, I have obſerved the ſame brown or black colour at the union of the carbonate with the porcelain tubes, where the powder had been purpoſely rammed with a piece of wood; ſo that this colour, which has occurred in almoſt every ſimilar caſe, remains to be accounted for. The carbonate efferveſced violently with acid; the ſubſtance in contact with the tube, doing ſo, however, more feebly than in the heart, leaving a copious depoſit of white ſandy matter, which is doubtleſs a part of the tube, taken up by the carbonate in fuſion.

Another experiment with ſlow cooling. Saline ſtructure and cryſtallization produced in chalk previouſly pounded.

On the 24th of March, I made a ſimilar experiment, in a ſtout gun-barrel, and took ſome care, after the application of heat, to cool the barrel ſlowly, with a view to cryſtallization. The whole maſs was found in a fine ſtate, and untouched by the lead; having a ſemitranſparent and ſaline ſtructure, with various facettes. In one part, I found the moſt decided cryſtallization I had obtained, though of a ſmall ſize: owing to its tranſparency it was not eaſily viſible, till the light was made to reflect from the cryſtalline ſurface, which then produced a dazzle, very obſervable by the naked eye; when examined by means of a lens, it was ſeen to be compoſed of ſeveral plates, broken irregularly in the fracture of the ſpecimen, all of which are parallel to each other, and reflect under the ſame angle, ſo as to unite in producing the dazzle. This ſtructure was obſervable equally well in both parts of the broken ſpecimen. In a former experiment, as large a facette was obtained in a piece of ſolid chalk; but this reſult was of more conſequence, as having been produced from chalk previouſly pounded.

The

The gun-barrels, though superior to porcelain were still too weak.

The foregoing experiments proved the superior efficacy of iron vessels over those of porcelain, even where the thickness was not great; and I persevered in making a great many experiments with gun-barrels, by which I occasionally obtained very fine results: but I was at last convinced, that their thickness was not sufficient to ensure regular and steady success. For this purpose, it appeared proper to employ vessels of such strength, as to bear a greater expansive force than was just necessary; since, occasionally, (owing to our ignorance of the relation between the various forces of expansion, affinity, tenacity, &c.) much more strain has been given to the vessels than was requisite. In such cases, barrels have been destroyed, which, as the results have proved, had acted with sufficient strength during the first stages of the experiments, though they had been unable to resist the subsequent overstrain. Thus, my success with gun-barrels, depended on the good fortune of having used a force no more than sufficient, to constrain the carbonic acid, and enable it to act as a flux on the lime. I therefore determined to have recourse to iron barrels of much greater strength, and tried various modes of construction.

Barrels formed by boring in solid bars of iron which proved excellent.

I had some barrels executed by wrapping a thick plate of iron round a mandrel, as is practised in the formation of gun-barrels; and likewise by bringing the two flat sides together, so as to unite them by welding. These attempts, however, failed. I next thought of procuring bars of iron, and of having a cavity bored out of the solid, so as to form a barrel. In this manner I succeeded well. The first barrel I tried in this way was of small bore, only half an inch: Its performance was highly satisfactory, and such as to convince me, that the mode now adopted was the best of any that I had tried. Owing to the smallness of the bore, a pyrometer could not be used internally, but was placed upon the breech of the barrel, as it stood in the vertical muffle. In this position, it was evidently exposed to a much less heat than the fiducial part of the apparatus, which was always placed, as nearly as could be guessed, at the point of greatest heat.

Finely levigated spar became agglutinated by heat, semi-transparent, vitreous, with a few facettes.

On the 8th of April, an experiment was made in this way with some spar; the pyrometer on the breech giving 33°. The spar came out clean, and free from any contamination adhering to the inside of the porcelain tube: it was very much shrunk

shrunk, still retaining a cylindrical form, though bent by partial adhesions. Its surface bore scarcely any remains of the impression taken by the powder, on ramming it into the tube: it had, to the naked eye, the roughness and semitransparency of the pith of a rush stripped of its outer skin. By the lens, this same surface was seen to be glazed all over, though irregularly, shewing here and there some air-holes. In fracture, it was semitransparent, more vitreous than crystalline, though having a few facettes: the mass, was seemingly formed of a congeries of parts, in themselves quite transparent: and, at the thin edges, small pieces were visible of perfect transparency. These must have been produced in the fire; for the spar had been ground with water; and passed through sieves, the same with the finest of those used at Etruria, as described by Mr. Wedgwood, in his paper on the construction of his pyrometer.

With the same barrel I obtained many interesting results, giving as strong proofs of fusion as in any former experiments; with this remarkable difference, that, in these last, the substance was compact, with little or no trace of frothing. In the gun-barrels where fusion had taken place, there had always been a loss of 4 or 5 *per cent.* connected, probably, with the frothing. In these experiments, for a reason soon to be stated, the circumstance of weight could not be observed; but appearances led me to suppose, that here the loss had been small, if any.

In these experiments the escape of carbonic acid appears to have been less.

On the 6th of April, I made another experiment with the square barrel, whose thickness was now much reduced by successive scales, produced by oxidation, and in which a small rent began to appear externally, which did not, however, penetrate to the bore. The heat rose high, a pyrometer on the breech of the barrel giving 37°. On removing the metals, the cradle was found to be fixed, and was broken in the attempts made to withdraw it. The rent was much widened externally: but it was evident, that the barrel had not been laid open, for part of the carbonate was in a state of saline marble; another was hard and white, without any saline grains, and scarcely effervesced in acid. It was probably quicklime, formed by internal calcination, but in a state that has not occurred in any other experiment.

Gradual failure of the barrel bored from the solid.

The

Remarkable fact of cryftals which appear to have been formed by fublimation.

The workman whom I employed to take out the remains of the cradle, had cut off a piece from the breech of the barrel, three or four inches in length. As I was examining the crack which was feen in this piece, I was furprifed to fee the infide of the barrel lined with a fet of tranfparent and well-defined cryftals, of fmall fize, yet vifible by the naked eye. They lay together in fome places, fo as to cover the furface of the iron with a tranfparent coat; in others they were detached, and fcattered over the furface. Unfortunately, the quantity of this fubftance was too fmall to admit of much chemical examination; but I immediately afcertained, that it did not in the leaft effervefce in acid, nor did it feem to diffolve in it. The cryftals were in general tranfparent and colourlefs, though a few of them were tinged feemingly with iron. Their form was very well defined, being flat, with oblique angles, and bearing a ftrong refemblance to the cryftals of the Lamellated Stylbite of Hauy. Though made above two years ago, they ftill retain their form and tranfparency unchanged. Whatever this fubftance may be, its appearance, in this experiment, is in the higheft degree interefting, as it feems to afford an example of the mode in which Dr. Hutton fuppofes many internal cavities to have been lined, by the fublimation of fubftances in a ftate of vapour; or, held in folution, by matters in a gafeous form. For, as the cryftals adhered to a part of the barrel, which muft have been occupied by air during the action of heat, it feems next to certain that they were produced by fublimation.

The old Sable Siberian iron is very tough at high heats.

The very powerful effects produced by this laft barrel, the fize of which (reduced, indeed, by repeated oxidation) was not above an inch fquare, made me very anxious to obtain barrels of the fame fubftance, which being made of greater fize, ought to afford refults of extreme intereft. I found upon inquiry, that this barrel was not made of Swedifh iron, as I at firft fuppofed, but of what is known by the name of *Old Sable*, from the figure of a Sable ftamped upon the bars; that being the armorial badge of the place in Siberia where this iron is made.*

All iron is crufhed under the hammer at fome definite heat. Caft iron at a low heat;

A workman explained to me fome of the properties of different kinds of irons, moft interefting in my prefent purfuit; and he

* I was favoured with this account by the late Profeffor Robifon.

he illustrated what he said by actual trial. All iron, when exposed to a certain heat, crushes and crumbles under the hammer; but the temperature in which this happens, varies with every different species. Thus, as he shewed me, cast iron crushes in a dull red heat, or perhaps about 15° of Wedgwood; steel, in a heat perhaps of 30°; Swedish iron, in a bright white heat, perhaps of 50° or 60°; old sable itself, likewise yields, but in a much higher heat, perhaps of 100°. I merely guessed at these temperatures; but I am certain of this, that in a heat similar to that in which Swedish iron crumbled under the hammer, the old sable withstood a strong blow, and seemed to possess considerable firmness. It is from a knowledge of this quality, that the blacksmith, when he first takes his iron from the forge, and lays it on the anvil, begins by very gentle blows, till the temperature has sunk to the degree in which the iron can bear the hammer. I observed, as the strong heat of the forge acted on the Swedish iron, that it began to boil at the surface, clearly indicating the discharge of some gaseous matter; whereas, the old sable, in the same circumstances, acquired the shining surface of a liquid, and melted away without any effervescence. I procured, at this time, a considerable number of bars of that iron, which fully answered my expectations.

Steel at a higher, Swedish iron at a bright white heat, and old Sable at a still higher temperature.

By the experiments last mentioned, a very important point was gained in this investigation; the complete fusibility of the carbonate under pressure being thereby established. But from this very circumstance, a necessity arose of adding some new devices to those already described: for the carbonate, in fusion, spreading itself on the inside of the tube containing it, and the two uniting firmly together, so as to be quite inseparable, it was impossible, after the experiment, to ascertain the weight of the carbonate by any method previously used. I therefore determined in future to adopt the following arrangement.

The complete fusibility of the carbonate under pressure was ascertained in these strong barrels.

A small tube of porcelain (*ik*, *Fig.* 23.) was weighed by means of a counterpoise of sand, or granulated tin; then the carbonate was firmly rammed into the tube, and the whole weighed again: thus the weight of the carbonate, previous to the experiment was ascertained. After the experiment, the tube, with its contents, was again weighed; and the variation of weight obtained, independently of any mutual action that had taken

Arrangement for obviating some difficulties which arose from the fusion. The carbonate was put into a small porcelain tube and this properly secured in a larger. These

were placed in a cradle or frame and the whole put into the iron barrel, &c.

taken place between the tube and the carbonate. The balance which I ufed, turned in a conftant and fteady manner, with one hundredth of a grain. When pounded chalk was rammed into this tube, I generally left part of it free, and in that fpace laid a fmall piece of lump-chalk (*i*), dreffed to a cylinder, with the ends cut flat and fmooth, and I ufually cut a letter on each end, the more effectually to obferve the effects produced by heat upon the chalk; the weight of this piece of chalk being always eftimated along with that of the powder contained in the tube. In fome experiments, I placed a cover of porcelain on the muzzle of the little tube, (this cover being weighed along with it), in order to provide againft the cafe of ebullition: but as that did not often occur, I feldom took the trouble of this laft precaution.

Continuation of the method of experiment.

It was now of confequence to protect the tube, thus prepared, from being touched during the experiment, by any fubftance, above all, by the carbonate of lime, which might adhere to it, and thus confound the appreciation by weight. This was provided for as follows: The fmall tube (*Fig.* 23, *i k*) with its pounded carbonate (*k*), and its cylinder of lump-chalk (*i*), was dropt into a large tube of porcelain (*p k*, *Fig.* 24). Upon this a fragment of porcelain (*l*), of fuch a fize as not to fall in between the tubes, was laid. Then a cylinder of chalk (*m*) was dreffed, fo as nearly to fit and fill up the infide of the large tube, one end of it being rudely cut into the form of a cone. This mafs being then introduced, with its cylindrical end downwards, was made to prefs upon the fragment of porcelain (*l*). I then dropped into the fpace (*n*), between the conical part of this mafs and the tube, a fet of fragments of chalk, of a fize beyond what could poffibly fall between the cylindrical part and the tube, and preffed them down with a blunt tool, by which the chalk being at the fame time crufhed and rammed into the angle, was forced into a mafs of fome folidity, which effectually prevented any thing from paffing between the large mafs of chalk and the tube. In practice, I have found this method always to anfwer, when done with care. I covered the chalk, thus rammed, with a ftratum of pounded flint (*o*), and that again with pounded chalk (*p*) firmly rammed. In this manner, I filled the whole of the large tube with alternate layers of filex and chalk; the muzzle being always occupied with chalk, which was eafily preffed into a mafs of tolerable firmnefs

ırmnefs, and, fuffering no change in very low heats, excluded the fufible metal in the firft ftages of the experiment. Continuation of the method of experiment.

The large tube, thus filled, was placed in the cradle, fometimes with the muzzle upwards, and fometimes the reverfe. I have frequently altered my views as to that part of the arrangement, each mode poffeffing peculiar advantages and difadvantages. With the muzzle upwards, (as fhewn in *Fig.* 24 and 25), the beft fecurity is afforded againft the intrufion of the fufible metal; becaufe the air, quitting the air-tube in the working pofition, occupies the upper part of the barrel; and the fufible metal ftands as a liquid (at *q*, *Fig.* 25.) below the muzzle of the tube, fo that all communication is cut off, between the liquid metal and the infide of the tube. On the other hand, by this arrangement, the fmall tube, which is the fiducial part of the apparatus, is placed at a confiderable diftance from the breech of the barrel, fo as either to undergo lefs heat than the upper part, or to render it neceffary that the barrel be thruft high into the muffle.

With the muzzle of the large tube downwards, the inner tube is placed (as fhewn in *Fig.* 22), fo as ftill to have its muzzle upwards, and in contact with the breech of the large tube. This has the advantage of placing the fmall tube near to the breech of the barrel: and though there is here lefs fecurity againft the intrufion of liquid metal, I have found that a point of little confequence; fince, when the experiment is a good one, and that the carbonic acid has been well confined, the intrufion feldom takes place in any pofition. In whichever of the two oppofite pofitions the large tube was placed, a pyrometer was always introduced, fo as to lie as near as poffible to the fmall tube. Thus, in the firft-mentioned pofition, the pyrometer was placed immediately below the large tube, and, in the other pofition above it; fo that, in both cafes, it was feparated from the carbonate by the thicknefs only of the two tubes.

Much room was unavoidably occupied by this method, which neceffarily obliged me to ufe fmall quantities of carbonate, the fubject of experiment feldom weighing more than 10 or 12 grains, and in others far lefs*.

On

* I meafured the capacity of the air-tubes by means of granulated tin, acting as a fine and equal fand. By comparing the weight of

Experiment made with the foregoing precautions. The heat amounted to 64° Wedgwood. The carbonates had lost gas and undergone fusion.

On the 11th of April, 1803, with a barrel of old sable iron having a bore of 0.75 of an inch, I made an experiment in which all these arrangements were put in practice. The large tube contained two small ones; one filled with spar, and the other with chalk. I conceived that the heat had risen to 33°, or somewhat higher. On melting the metals, the cradle was thrown out with considerable violence. The pyrometer, which, in this experiment, had been placed within the barrel, to my astonishment, indicated 64°. Yet all was found. The two little tubes came out quite clean and uncontaminated. The spar had lost 17.0 *per cent.* the chalk 10.7 *per cent.* The spar was half sunk down, and run against the side of the little tube: Its surface was shining, its texture spongy, and it was composed of a transparent and jelly-like substance. The chalk was entirely in a state of froth. This experiment extends our power of action, by shewing, that compression, to a considerable degree, can be carried on in so great a heat as 64°. It seems likewise to prove, that, in some of the late experiments with the square barrel, the heat had been much higher than was supposed at the time, from the indication of the pyrometer placed on the breech of the barrel; and that in some of them, particularly in the last, it must have risen at least as high as in the present experiment.

Experiment in which the barrel failed after its contents had undergone fusion.

On the 21st of April, 1805, a similar experiment was made with a new barrel, bored in a square bar of old sable, of about two and a half inch in diameter, having its angles merely rounded; the inner tube being filled with chalk. The heat was maintained during several hours, and the furnace allowed to burn out during the night. The barrel had the appearance of soundness, but the metals came off quietly, and the carbonate was entirely calcined, the pyrometer indicating 63°. On examination, and after beating off the smooth and even scale of oxide peculiar to the old sable, the barrel was found to have yielded in its peculiar manner; that is, by the opening of the longitudinal fibres. This experiment, notwithstanding the failure of the barrel, was one of the most interesting I had

of this tin with an equal bulk of water, I found that a cubic inch of it weighed 1330.6 grains, and that each grain of it corresponded to 0.00075 of a cubic inch. From these data I was able, with tolerable accuracy, to gage a tube by weighing the tin required to fill it.

made,

made, since it afforded proof of complete fusion. The carbonate had boiled over the lips of the little tube, standing, as just described, with its mouth upwards, and had run down to within half an inch of its lower end: most of the substance was in a frothy state, with large round cavities, and a shining surface; in other parts, it was interspersed with angular masses, which have evidently been surrounded by a liquid in which they floated. It was harder, I thought, than marble; giving no effervescence, and not turning red like quicklime in nitric acid, which seemed to have no effect upon it in the lump. It was probably a compound of quicklime with the substance of the tube.

With the same barrel repaired, and with others like it, many similar experiments were made at this time with great success; but to mention them in detail, would amount nearly to a repetition of what has been said. I shall take notice of only four of them, which, when compared together, throw much light on the theory of these operations, and likewise seem to establish a very important principle in geology. These four experiments differ from each other only in the heat employed, and in the quantity of air introduced.

Account of some experiments at very elevated heats. The carbonate most heated was calcined: that which had suffered less heat had the form of lime-stone and of marble, which retained their carbonic acid.

The first of these experiments was made on the 27th of April 1803, in one of the large barrels of old sable, with all the above-mentioned arrangements. The heat had risen, contrary to my intention, to 78° and 79°. The tubes came out uncontaminated with fusible metal, and every thing bore the appearance of soundness. The contents of the little tube, consisting of pounded chalk, and of a small piece of lump-chalk, came out clean, and quite loose, not having adhered to the inside of the tube in the smallest degree. There was a loss of 41 *per cent.* and the calcination seemed to be complete; the substance, when thrown into nitric acid, turning red, without effervescence at first, though, after lying a few minutes, some bubbles appeared. According to the method followed in all these experiments, and lately described at length, (and shewn in *Fig.* 24 and 25), the large tube was filled over the small one, with various masses of chalk, some in lump, and some rammed into it in powder; and in the cradle there lay some pieces of chalk, filling up the space, so that in the cradle there was a continued chain of carbonate of four or five inches in length. The substance was found to be less and less calcined, the more

it was removed from the breech of the barrel, where the heat was greateft. A fmall piece of chalk, placed at the diftance of half an inch from the fmall tube, had fome faline fubftance in the heart, furrounded and intermixed with quicklime, diftinguifhed by its dull white. In nitric acid, this fubftance became red, but effervefced pretty brifkly; the effervefcence continuing till the whole was diffolved. The next portion of chalk was in a firm ftate of limeftone; and a lump of chalk in the cradle, was equal in perfection to any marble I have obtained by compreffion: the two laft-mentioned pieces of chalk effervefcing with violence in the acid, and fhewing no rednefs when thrown into it. Thefe facts clearly prove that the calcination of the contents of the fmall tube had been internal, owing to the violent heat which had feparated its acid from the moft heated part of the carbonate, according to the theory already ftated. The foundnefs of the barrel was proved by the complete ftate of thofe carbonates which lay in lefs heated parts. The air-tube in this experiment had a capacity of 0.29, nearly one-third of a cubic inch.

Another experiment in which the barrel failed.

The fecond of thefe experiments was made on the 29th of April, in the fame barrel with the laft, after it had afforded fome good refults. The air-tube was reduced to one-third of its former bulk, that is, to one-tenth of a cubic inch.— The heat rofe to 60°. The barrel was covered externally with a black fpongy fubftance, the conftant indication of failure, and a fmall drop of white metal made its appearance. The cradle was removed without any explofion or hiffing. The carbonates were entirely calcined. The barrel had yielded, but had refifted well at firft; for the contents of the little tube were found in a complete ftate of froth, and running with the porcelain.

Third experiment, very thin fufion.

The third experiment was made on the 30th of April, in another fimilar barrel. Every circumftance was the fame as in the two laft experiments, only that the air-tube was now reduced to half its laft bulk, that is, to one-twentieth of a cubic inch. A pyrometer was placed at each end of the large tube. The uppermoft gave 41°, the other only 15°. The contents of the inner tube had loft 16 *per cent.* and were reduced to a moft beautiful ftate of froth, not very much injured by the internal calcination and indicating a thinner ftate of fufion than had appeared.

3 The

Fig. 7

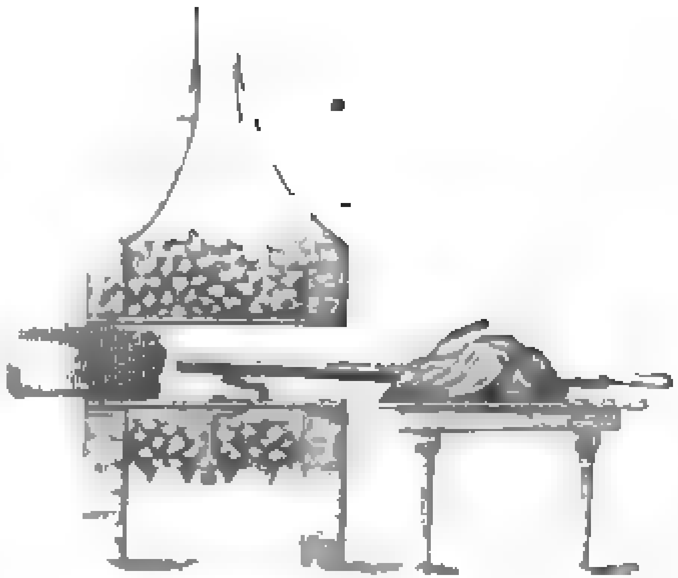
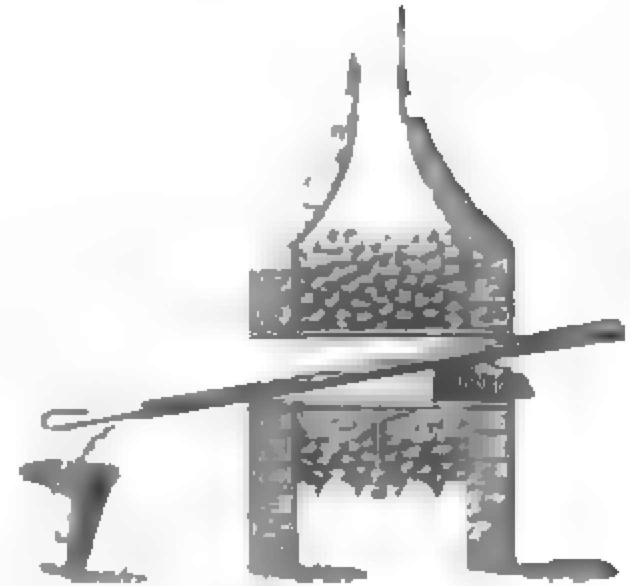


Fig. 8



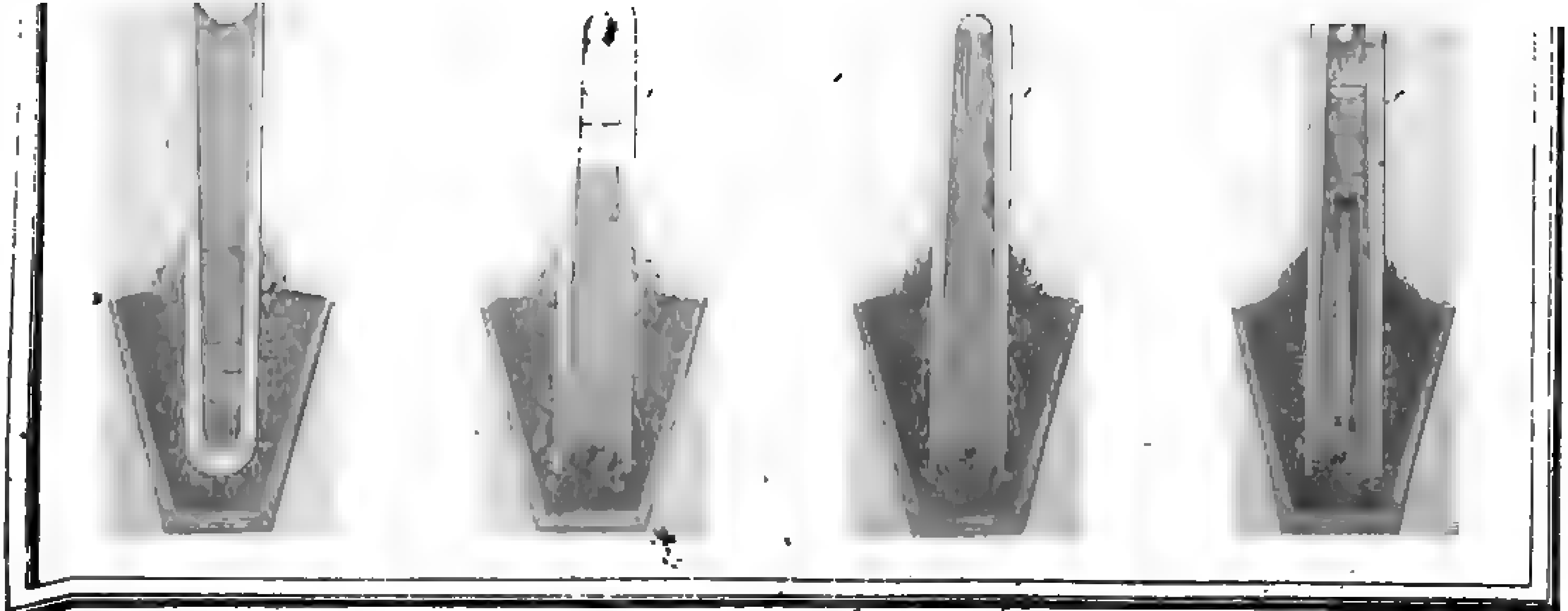


Fig 21 Fig 27 Fig 29 Fig 31 Fig [illegible] Fig 33 Fig 34

The fourth experiment was made on the 2d of May, like the rest in all respects, with a still smaller air-tube, of 0.0318, being less than one thirtieth of a cubic inch. The upper pyrometer gave 25°, and the under one 16°: The lowest masses of carbonate were scarcely affected by the heat. The contents of the little tube lost 2.9 *per cent.* both the lump and the pounded chalk were in a fine saline state, and, in several places, had run and spread upon the inside of the tube, which I had not expected to see in such a low heat. On the upper surface of the chalk rammed into the little tube, which, after its introduction had been wiped smooth, were a set of white crystals, with shining facettes, large enough to be distinguished by the naked eye, and seeming to rise out of the mass of carbonate. I likewise observed, that the solid mass on which these crystals stood, was uncommonly transparent.

Fourth experiment under greater pressure, fusion at a moderate heat, slight loss of carbonic acid, &c.

In these four experiments, the bulk of the included air was successively diminished, and by that means its elasticity increased. The consequence was, that in the first experiment, where that elasticity was the least, the carbonic acid was allowed to separate from the lime, in an early stage of the rising heat, lower than the fusing point of the carbonate, and complete internal calcination was effected. In the second experiment, the elastic force being much greater, calcination was prevented, till the heat rose so high as to occasion the entire fusion of the carbonate, and its action on the tube, before the carbonic acid was set at liberty by the failure of the barrel. In the third experiment, with still greater elastic force, the carbonate was partly calcined, and its fusion accomplished, in a heat between 41° and 15°. In the last experiment, where the force was strongest of all, the carbonate was almost completely protected from decomposition by heat, in consequence of which it crystallized and acted on the tube, in a temperature between 25° and 16°. On the other hand, the efficacy of the carbonic acid as a flux on the lime, and in enabling the carbonate to act as a flux on other bodies, was clearly evinced; since the first experiment proved that quicklime by itself could neither be melted, nor act upon porcelain, even in the violent heat of 79°; whereas, in the last experiment where the carbonic acid was retained, both of these effects took place in a very low temperature.

Observations: the fusion takes place at lower heats when the escape of carbonic acid is prevented. The acid acts as a flux.

(To be continued.)

 IV. *Observations*

IV.

Obſervations on the Effect of Madder Root on the Bones of Animals. By Mr. B. GIBSON.*

Account of the firſt diſcovery of the property of madder to tinge the bones of living animals.

THERE is, perhaps, no phenomenon, which occurs in an animal body more curious, than the tinge communicated to the bones of living animals, whoſe food has been mixed with madder root. This, like many other facts, to which no reaſoning *à priori* could have directed us, was diſcovered by chance. Mr. Belcher, dining with a calico printer on a leg of freſh pork, was ſurprized that the bones, inſtead of poſſeſſing their uſual whiteneſs, were of a deep-red colour; and on enquiring the cauſe of it, was informed, that the pig had been fed upon the refuſe of the dyers' vats, and had received ſo much of the colouring matter of madder into the ſyſtem, that its bones were dyed by it. So intereſting a fact has attracted very much the attention of anatomiſts, and has been uſed in many phyſiological and pathological enquiries; it may not therefore be unintereſting to give a ſhort hiſtory of the phenomena connected with it, and the purpoſes to which it has been applied, previous to entering upon the more immediate object of this paper.

Experiments ſhewing that the tinge is more quickly given to the bones of growing animals.

Many experiments have been made to aſcertain how long a time is required to produce the tinge, and whether it be permanent or only temporary. Belcher and Morand, about the ſame time, mixed madder root with the food of chickens and young pigeons. The reſult of their obſervations was, that the tinge was more quickly communicated to the bones of growing animals, than to the bones of animals which had already completed their growth; the bones of young pigeons being tinged of a roſe-colour in twenty-four hours, and of a deep ſcarlet in three days; whilſt the bones of adult animals only exhibited a roſe-colour in fifteen days. They found the tinge moſt intenſe in the ſolid parts of thoſe bones, which were neareſt to the centre of circulation; whilſt in bones of equal ſolidity, at a greater diſtance from the heart, the tint was more faint. The dye was deep in proportion to the length of time the madder had been continued, and when it was diſcontinued, the colour gradually became more and more faint, till

Short time and other facts.

* Mancheſter Memoirs, 1805.

it

it entirely disappeared. According to the experiments of these gentlemen, other vegetable dyes, such as logwood, turmeric and alkanet root, did not communicate their respective tints to the bones.*

This effect of madder upon the bones, was soon afterwards made use of by Du Hamel, in his attempt to prove the manner in which the bones of animals are encreased in thickness. Observing in the vegetable kingdom, that the bark, by a sort of secretion, formed the ligneous part of a tree, in successive layers; so he conceived that the periosteum, or membrane surrounding bones, being converted into osseous matter, encreased their diameter by adding to them concentric laminæ in succession. In order to prove the justness of his opinion, he mixed the food of a cock with madder root for a month, withheld it for a month, and then gave it again. He afterwards killed the animal, and upon inspection thought he observed the appearance which he expected; viz. two layers of red bone inclosing one of white, corresponding to the periods of the madder's being given or withheld.

Du Hamel u[se]s this property t[o] shew the grow[th] of bones.

This experiment, and some others related by Du Hamel, appear to be conclusive in favour of the theory, which he wished to establish; and as they were conducted by a physiologist of high character, the accuracy of the observations could not have been doubted, had these experiments stood alone. But when they are compared with some of his own previous experiments, and those of other authors, it is difficult to reconcile them. In some of Du Hamel's experiments, for instance, the bones of a cock were tinged of a rose-colour through their whole substance in sixteen days, and those of young pigeons of a deep scarlet in three days. In several ex-

It is very doub[t]ful whether th[e] growth could [be] so indicated.

* From some experiments I made on young pigeons, I found that a considerable quantity of logwood, in the form of extract, communicated an evidently purple tint to the bones. With regard to turmeric, it appears to be altered in its colour by passing through the digestive organs, for the fœces of the animals, who took it in considerable quantity, were constantly green: whilst either logwood or madder root exhibited their respective hues after passing through the intestines. Saffron exhibits properties different from any of these substances; for though a pigeon took it in considerable quantity, and thereby had its fœces tinged, yet no perceptible alteration of colour was produced in its bones.

 periments

periments I have made on the fubject, I have found the bones of young pigeons tinged of a uniform rofe-colour, internally as well as externally, in twenty-four hours. This communication of colour to the whole fubftance of the offeous fyftem in fo fhort a time, makes it highly improbable that the laminated appearance, remarked by Du Hamel, was produced by the new formation of red and white offeous layers, correfponding to the times (months) the madder had been given or withheld. For, as Mr. John Bell very juftly remarks,* "If a bone fhould increafe by layers thick enough to be vifible and of a diftinct tint, and fuch layers be continually accumulated upon each other every week, what kind of bone fhould this grow to?" The only way in which we can reconcile with each other the phenomena obferved in the different experiments, and account for their apparent contradiction, is, by fuppofing that Du Hamel miftook for an obfcurely laminated appearance, the variety in the tint, which is more deeply communicated to the more folid, and more faintly to the lefs compact parts of a bone.

Late experiments of Dr. M'Donald on the bones.

This property of madder of tinging the bones of animals, has lately been employed by Dr. M'Donald,† in his ingenious refearches into the formation and death of bones. Amongft other objects, he attempted to afcertain in what manner and how foon a cylindrical bone is regenerated to fupply the place of one artificially killed. As the procefs is highly curious, I fhall briefly relate the principal points.

Very curious procefs of a bone deftroyed,

Dr. M'Donald's experiments were made by amputating the proper leg-bone of young pigeons or chickens immediately above the joint. The marrow was then extracted, and the cavity which contained it, filled with lint. This procefs caufed the death of the bone, and the formation of a new bone furrounding that deftroyed enfued. Immediately after the experiment, the animal had its food mixed with madder root, and the part was infpected in different animals, at different periods.

and the regular procefs of

On examination three days afterwards, the periofteum or enveloping membrane, was found much thickened; and underneath it a gelatinous humour was effufed, furrounding the

* Anatomy of the bones, &c. p. 15.

† Difputatio inauguralis de Necrofi ac Callo. 1799.

dead

dead bone, and ſpotted with red oſſeous nuclei; proving that the regeneration of the bone had commenced at this early period.

In ſeven days the new bone was found ſoft and flexible, not to be diſtinguiſhed from cartilage or griſtle, except by the red tint the madder had communicated to it; yet the bone deſtroyed was not at all coloured, although the other bones of the animal had acquired a bright red. From this time the new bone continued to encreaſe in hardneſs, ſurrounding the old one like a ſheath. The latter in about three weeks was ſo looſe as to be drawn out, and in about fifteen days from this time, the cavity of the regenerated bone was filled with marrow, and in every reſpect performed the office of that for which it was a ſubſtitute. This may be conſidered as a general outline of the progreſſive changes which take place during the regeneration of a cylindrical bone, in a young animal, ſuch as a pigeon, or chicken; and the ſame proceſs is frequently performed in the human body, when, from ſome internal cauſe, the life of a bone is deſtroyed. Theſe changes involve many intereſting particulars; but the circumſtance moſt immediately connected with the ſubject of this paper is, that although the ſhaft of the bone required three weeks for its renewal, yet in ſeven days the oſſeous ſyſtem generally had acquired a bright red. Now if we explain this change in colour according to the common opinion of abſorption of the white, and depoſition of the red oſſeous matter,* we muſt neceſſarily draw this concluſion; that the oſſeous ſyſtem of the animal will be renewed three times during the period, which the formation of the ſubſtitute bone requires; a concluſion which we ſhould be inclined to reject merely from its improbability. But beſides this, the appear- —its regeneration.

Inference. From the very ſpeedy acquiſition and ſubſequent loſs of the red colour that the oſſeous ſyſtem was naturally abſorbed and renewed in that period.

* The common opinion of phyſiologiſts, with regard to this curious fact, is, that when a bone becomes red, during the exhibition of madder root, the white oſſeous particles which compoſed it, have been entirely removed by abſorption and replaced by new oſſeous matter of a red colour: and when a bone aſſumes its natural colour, theſe red particles have been removed and replaced by white. If this be the fact, it neceſſarily follows, that an animal has at leaſt fifty-two new ſets of bones in a year: for the oſſeous ſyſtem, according to the experiments of the moſt reſpectable phyſiologiſts, acquires a deep red tint from madder in one week, and aſſumes its natural colour in another.

ance

ance of the parts ſtrongly militate againſt it—for, if we may judge at all of the activity of the proceſs in the two parts, by their comparative degrees of vaſcularity, that employed in forming the ſubſtitute bone far exceeds that going on in the oſſeous ſyſtem generally; one ſtriking phenomenon attending the regeneration of a bone being, the very high degree of increaſed vaſcularity which the parts employed in the proceſs rapidly aſſume.

Cauſe of doubt.

The bones are alone reddened by madder, becauſe the phoſphate of lime acts as a mordent on the madder.

After this effect of madder upon the bones was known, it long remained a myſtery, why ſome other white parts of the body, ſuch as nerves, cartilages and perioſteum, were not equally liable to be coloured by it, as the bones. This fact, I believe, did not receive any explanation, until Dr. Rutherford gave a very ingenious and ſatisfactory one. When ſpeaking of this property of madder, he ſays,* "We have, in the fact before us, a beautiful example of a particular caſe of chemical attraction; ſuch as in numberleſs inſtances, is obſerved to take place between the colouring particles of both animal and vegetable ſubſtances and various other bodies, eſpecially earths and earthy ſalts, and oxides of metals. So ſtrong is the affinity of the colouring matter to theſe bodies, that it is frequently obſerved to quit the menſtruum, in which it may chance to be diſſolved, to unite with them: they, in conſequence of its union, acquiring a particular tinge, whilſt the menſtruum is proportionably deprived of colour. From this principle, this mutual attraction, is deduced the various uſe of thoſe bodies as mordents, as they are called, intermedia, or means for fixing the colours in dying or ſtaining thread or cloth, whether it be compoſed of animal or vegetable materials. Upon the ſame principle depends the preparation of thoſe pigments known to painters under the name of lakes; theſe are truly precipitates of the colouring matter, in combination with various mordents, as their baſis.—The colouring of the bones of a living animal by means of madder, is, in every circumſtance, analogous to the formation of theſe lakes. The colouring matter of madder, paſſing unaltered through the digeſtive organs of the animal, enters the general maſs of fluids, and is diſſolved in the ſerum of the blood, to which,

The red matter is a kind of lake.

* See Dr. Blake's inaugural Diſſertation. De dentium formatione, p. 119.—1798.

indeed,

indeed, if it be in large proportion, it communicates a sensibly red tinge. *But there is always present in the blood, and in a state of solution in the serum, a quantity of the earthy matter of the bones, phosphate of lime, ready to be deposited, as the exigencies of the animal may require. Now the phosphate of lime is an excellent mordent to madder, and has a strong affinity to it, and is consequently admirably fitted to afford a base for the colouring matter of it: in such experiments, therefore, they concrete in the state of a bright red lake, whence the colour of the bones is derived.* That this is actually the case, may be shewn by a variety of experiments. Thus, if to an infusion of madder in distilled water, be added a little of the muriate of lime, no change is perceived: but if to this mixture be added a solution of the phosphate of soda, immediately a double elective attraction takes place. The muriatic acid combining with the soda, remains suspended, or dissolved in the water; whilst the phosphoric acid, thus deprived of its soda, combines with the lime which the muriatic acid parted with, and forms phosphate of lime or earth of bones. This substance, however, being insoluble in water, falls to the bottom; but having combined at the instant of its formation with the colouring matter of the madder, they fall down united into a crimson lake; precisely of the same tint with that of the bones of young animals, which have been fed with madder. From this simple representation of the matter, we have a ready explication of every circumstance which has been remarked as extraordinary respecting this subject."

—formed as it seems before the osseous deposition.

Whilst Dr. Rutherford thus gives a most satisfactory explanation of the colour of madder being communicated to the bones alone, of all the white parts of an animal; we find that he embraces the same opinion as other physiologists, that the osseous materials acquire their colour previous to their deposition, whilst in a state of solution or mixture in the blood; from whence they are afterwards deposited, and concrete in the form of a bright lake. In no part of his ingenious remarks does he hint at the probability that the bones already formed in an animal, may, during the use of madder, become red, and after its disuse gradually resume their natural colour, by the agency of a power entirely independent of their deposition and absorption: that this is probable I shall now proceed to prove.

Dr. Rutherford admits the absorption and deposition.

Before

More particular explanation of the doctrine of the abſorption and regeneration of the parts of animals,

Before it was diſcovered that madder poſſeſſed this property of tinging bones, phyſiologiſts had long been of opinion, that the various parts of the body, being worn out by the performance of their actions and functions, were gradually removed, and replaced by new materials. They had ſeen, as Mr. J. Bell obſerves, the whole oſſeous ſyſtem by the morbid removal of its ſolid part, rendered ſo ſoft and flexible as to bend under the common weight of the body and ordinary action of parts; the regeneration of many bones which had been deſtroyed by diſeaſe; the rapid abſorption of fat in ſome diſeaſes, and its ſpeedy reproduction; and laſtly, the gradual change which the fluids of the body undergo, as well as ſome of its inſenſible parts, the hair and nails; hence they ſuppoſed that the ſame proceſs of change and renovation went on in every organ, and that the bodies of animals were not compoſed of the ſame identical particles of which they would conſiſt at ſome future period. This proceſs, which was before but conjectural, or ſupported by analogy, phyſiologiſts conſidered as fully proved by the effects of madder upon the bones. They had by this means an opportunity of ſeeing the bones altered in colour, from the ſlighteſt tint to the deepeſt red; they could obſerve this gradually removed, until the bones had regained their natural whiteneſs; and explaining the whole proceſs on the principle of depoſition and abſorption, they conſidered it as ocular demonſtration of a moſt rapid change in the conſtituent elements of a part, of which, from its ſolidity, they could ſcarcely have believed it ſuſceptible.

—ſuppoſed to be confirmed in the bones.

Probability that this explanation is erroneous.

I apprehend, however, that it is by giving an erroneous explanation of the phenomena; by ſuppoſing that a change of the oſſeous particles is denoted by an alteration in their colour that phyſiologiſts have conſidered this fact as concluſive.— However indubitable and well ſupported may be the opinion which attributes an imperceptible change to the various parts of the body, we ſhall, I believe diſcover upon a more cloſe examination, that it is by no means ſupported by the appearances, which the bones diſplay on the exhibition of madder root. The rapid change in their particles, which ſuch appearances indicate, when explained in the common way, is completely at variance with all the proceſſes performed by the bones, both in their healthy and diſeaſed ſtates. Thus we find the formation of the oſſific matter, called Callus, for the union

union of fractured bones, or the exfoliation of a part of a bone, are processes requiring a considerable length of time for their performance. In Dr. M'Donald's experiments, the formation of a regenerated bone required nearly six weeks; but during the same space of time, the bones of the same animal would be renewed several times, if the common explanation of the communication and disappearance of the tinge of madder were well founded. From these circumstances, I am led to believe that the appearances produced by the exhibition of madder, require another mode of explanation. That which I have to offer is not liable to the same objections, and is strongly supported by comparative experiments.

For the processes by which bones are restored are slow.

It was observed by Du Hamel, in his experiments, that the bones of animals which had been deeply tinged by madder, by long exposure to air lost their colour and became white.—It was this fact which suggested to me a simple explanation of the process. It occurred to me, that if any one of the component parts of the blood naturally exerted a stronger attraction for the colouring matter of madder, than the phosphate of lime, it might be deprived of the tint by a chemical power. In order to prove this, as far as I could by experiment, I took one dram of the phosphate of lime tinged, as in Dr. Rutherford's experiment, and exposed it for half an hour to the action of two ounces of fresh serum, at the temperature of 98 degrees. By this operation, the serum gradually acquired a red tinge, whilst the phosphate of lime was proportionably deprived of colour. In a comparative experiment, a similar quantity of tinged phosphate of lime was exposed to the action of distilled water under similar circumstances; but no change took place. The knowledge of this strong affinity in the serum for colouring matter, affords an easy and simple explanation of the effects of madder on the bones, upon the principle of chemical attraction.

A simple explanation grounded on experiment.

The serum of blood has a stronger attraction for the colouring matter of madder than phosphate of lime has.

Thus, when an animal has madder mixed with its food, the blood becomes highly charged with it, and imparts the superabundant colouring matter to the phosphate of lime, contained in the bones already formed; as it circulates through them and moistens them throughout. But as soon as an animal has ceased to receive the madder, and the blood is freed from the colouring matter by the excretions, the serum then exerts its superior attraction, and by degrees entirely abstracts it from the

Hence the bones are dyed when much madder is in the system, and bleached by the serum when the quantity becomes less.

the phosphate of lime, and the bones resume their natural whiteness. In short, the bones are at one time dyed by the colouring matter, at another time bleached by the serum.

Phosphate while suspended does not strongly take the colouring matter.

Whilst I have attempted to explain the probable manner in which the bones, *already formed* in an animal, at one time receive, and at another are deprived of the colouring matter of madder, I by no means intend to assert that the phosphate of lime does not acquire a similar colour during its solution in the serum, or at the time it is precipitated from it to enter into the composition of the bones; the fact is indisputable. I have, however, found from some experiments lately made upon a hen during oviparation, that only a slight tinge can be communicated to the shell, formed whilst a large quantity of colouring matter is circulating with the blood. So slight indeed is the blush, that it would not be seen by a common observer, unless contrasted with a natural egg: which is probably the reason why it has, I believe, been denied by physiologists that the shell of an egg is altered by the exhibition of madder. If this may be considered as a test of the quantity of colouring matter, which the phosphate attracts at the time it is separated from the blood, it forms another strong argument against the theory, which Dr. Rutherford, and all preceding physiologists have adopted; for, consistent with this fact, the bones should never exhibit more than a slight blush. When explained upon the principle of chemical attraction, we see that the phenomena, exhibited by the bones of an animal, by giving or withholding madder root, give no support to the opinion that the various parts of the body continually undergo an imperceptible change; and I consider it a fortunate circumstance for that doctrine, that so simple an explanation of the effect of madder can be given. For whilst so specious a fact has been considered, by the highest authorities, as complete proof of the imperceptible renovation of parts; the rapid change in the constituent elements of the bones, which the communication and disappearance of the colour indicates, must have appeared astonishing to every physiologist. Of this I cannot give you a stronger instance than in the words of Mr. J. Bell.* Nothing," says he, "can be more curious than this continued renovation and change of parts even in the hardest bones. We are accustomed to say of the whole body, that it is daily

Example in eggs.

The doctrine of a rapid and continual change is not supported by the facts of bones tinged by the madder.

* Anatomy of the bones, &c. p. 13.

changed,

changed; that the older particles are removed, and new ones supply their place; that the body is not now the same individual body, that it was; but it could not be easily believed that we speak only by guess concerning the softer parts, which we know for certain of the bones.—When madder is given to animals, withheld for some time and then given again, the colour appears in their bones, is removed, and appears again with such a sudden change, as proves a rapidity of deposition and absorption exceeding all likelihood or belief; all the bones are tinged in twenty-four hours; in two or three days their colour is very deep, and if the madder be left off but for a few days, the red colour is entirely removed."

Although by this chemical explanation of the effect of madder upon the bones, the doctrine of the imperceptible change in the component parts of animal bodies, loses the support of a fact, which has, since its discovery, been universally considered as its strongest proof; nevertheless, indisputable arguments, derived from different sources, still place that doctrine amongst the best supported opinions in physiology.

V.

On Fairy Rings and the Waste of Fish in Scotland. By A. T.

To Mr. NICHOLSON.

SIR,

HAVING frequently noticed the fairy-rings your correspondent, M. Florian Jolly mentions in your Journal for February, I should be glad to know from him whether hares or rabbits abounded in Broadlands park, as I have generally observed these rings most prevalent, in light sandy soils, particularly among rabbit burrows. This species of soil from its dryness would be very unfavourable to the idea of these things being formed from a central heap of horse dung; besides, were this the cause of them, we should expect them to be always circular, or when not circular, that those parts most remote from the centre would appear not to have benefited so strongly from the manure as those which were nearer. I have generally observed that the rings were composed of a double circle, or rather a little circular path, the middle of which appeared to be

Observations and inquiry whether fairy rings may not have been made by hares and rabbits.

be trodden, and the edges grown up, and more in vigour than any of the furrounding grafs. I had occaofin to remark one of thofe fairy-rings laft fummer: it was perfectly circular, and about ten feet in diameter, it was fituated at the edge of a copfe wood, and in a vicinity where there are abundance of both hares and rabbits; but what appeared to me moft fingular, was its being interfected exactly through the middle, by a well frequented foot path. The hare is rather given to gravity, the rabbit is more playful; but whether it is given to the amufement of *lounging in the ring*, fome of your more informed correfpondents may be enabled to inform you.

Fifh is undoubtedly wafted in Scotland.

I OBSERVE fome of your correfpondents have got into a controverfy refpecting the wafte of fifh in Scotland. No doubt can exift upon that head; not however arifing from the wafteful difpofition of the natives, their delicacy in appetite or fuperabundance of provifion, but from the want of a market for the confumption of their overplus. To talk of Aberdeen fifhermen bringing frefh fifh to Newcaftle, Norwich, or Leeds, is as ridiculous as to propofe taking them to Amfterdam, or London; for befides the difficulty of again making their own ports, they will conftantly find an over-ftocked market, as the fame weather that permits them to fifh will permit their neighbours to do the fame. But the grand caufe of all the wafte is the horrible monopoly which their country labours under in refpect to their falt laws, where for the fake of a few paltry pans, Englifh falt is excluded under the fevereft penalties, although it can be delivered in any part of Scotland at one half the price that we are forced to pay for Scotch falt under the prefent circumftances. Give them falt at a cheap rate, if it does not permit them to export the fifh, as that requires capital and new eftablifhments, it would at leaft enable them to fupply the interior; a thing as worthy the attention of the public as the fupply of any other market I know.

The fifheries of Scotland are deftroyed by the operation of the falt laws.

Your moft obedient
A. T.

March 23, 1806.

Letter

VI.

Letter from AMICUS *respecting the supposed Waste of Crab-Fish in Scotland.*

To Mr. NICHOLSON.

SIR,

Observations concerning the fact that the bodies of crab-fish are wasted at Arbroath. It is a bad species which is rejected, the good ones are eaten.

THE very respectable and distinguished rank which the Philosophical Journal holds among the periodical publications will at all times prevent its becoming the vehicle of unnecessary dispute or contradiction: yet as public information and utility is sometimes promoted by the correction of mistakes, when this is likely to be the case, any thing that can elucidate a fact either misrepresented or partially stated, is doubtless compatible with the spirit of your publication. In your 48th number it is stated by "an Enquirer" that the crab fishery is so productive about Arbroath that, after boiling them, the bodies of the crabs are thrown away, and the large claws only brought to table, of which the Enquirer says he has been a witness. The fact is literally true, but wants further explanation. It is well known to every person resident on the coasts where crab-fish are commonly to be had, that many of that species are scarcely eatable, being often found after boiling to contain hardly any thing but water. The writer of this article has repeatedly seen from twelve to twenty crabs boiled at one time, and every one of them, more or less, in the above situation. When this is the case, the meat of the great claws (although they still may be eaten) is also watery and insipid compared to those of a good crab, the body of which is filled with a very rich substance, which is so far from being thrown away, that it is in general esteemed a luxury, even where crabs are plenty. Some persons are, indeed, fond of the claws, who cannot eat the bodies at all; but these are only exceptions from general taste and common practice. The claws of a good crab (as has been already observed) are much firmer, more rich, and sweet to the taste than those of an inferior kind, which are by far the most abundant. The claws of the male are larger in proportion than those of the female: the male crab is also reckoned superior in quality, except for a very short period (in what time of the year I have not been able

 to

to afcertain) when in the the opinion of fome who pretend to be connoiffeurs, the females are equal, or nearly fo in delicacy.

Crabs are in feafon nine months in the year; May, June, and July are the only months in which they are not. Some pifcatory epicures pretend to certain marks for diftinguifhing good crabs, but they are very far from being infallible; perhaps the moft general diftinction is, that a good crab has a fhell of a dufky red colour, with a certain degree of roughnefs, particularly on the claws; while the bad ones have fhells white, clear, fmooth, and watery; but the diftinction is much better underftood from obfervation than any detailed account. Trufting that you, Mr. Editor, will have the goodnefs to infert this communication, and that your correfpondent, "the Enquirer," will do me the juftice to believe that my fole motive for troubling you was to give information, I am with efteem, SIR,

Your moft obedient fervant,

AMICUS.

Arbroath, March 4, 1806.

VII.

Probability that the Hindoos were acquainted with Saturn's Ring.

To Mr. NICHOLSON.

SIR,

I TAKE the liberty of requefting the infertion of the following quotation in your Philofophical Journal, from the 7th vol. of Mr. Maurice's Indian Antiquities, page 605. If it does really mean the ring of the planet Saturn, perhaps fome of your readers can explain how it could have been difcovered by the Brahmins in fuch remote ages.

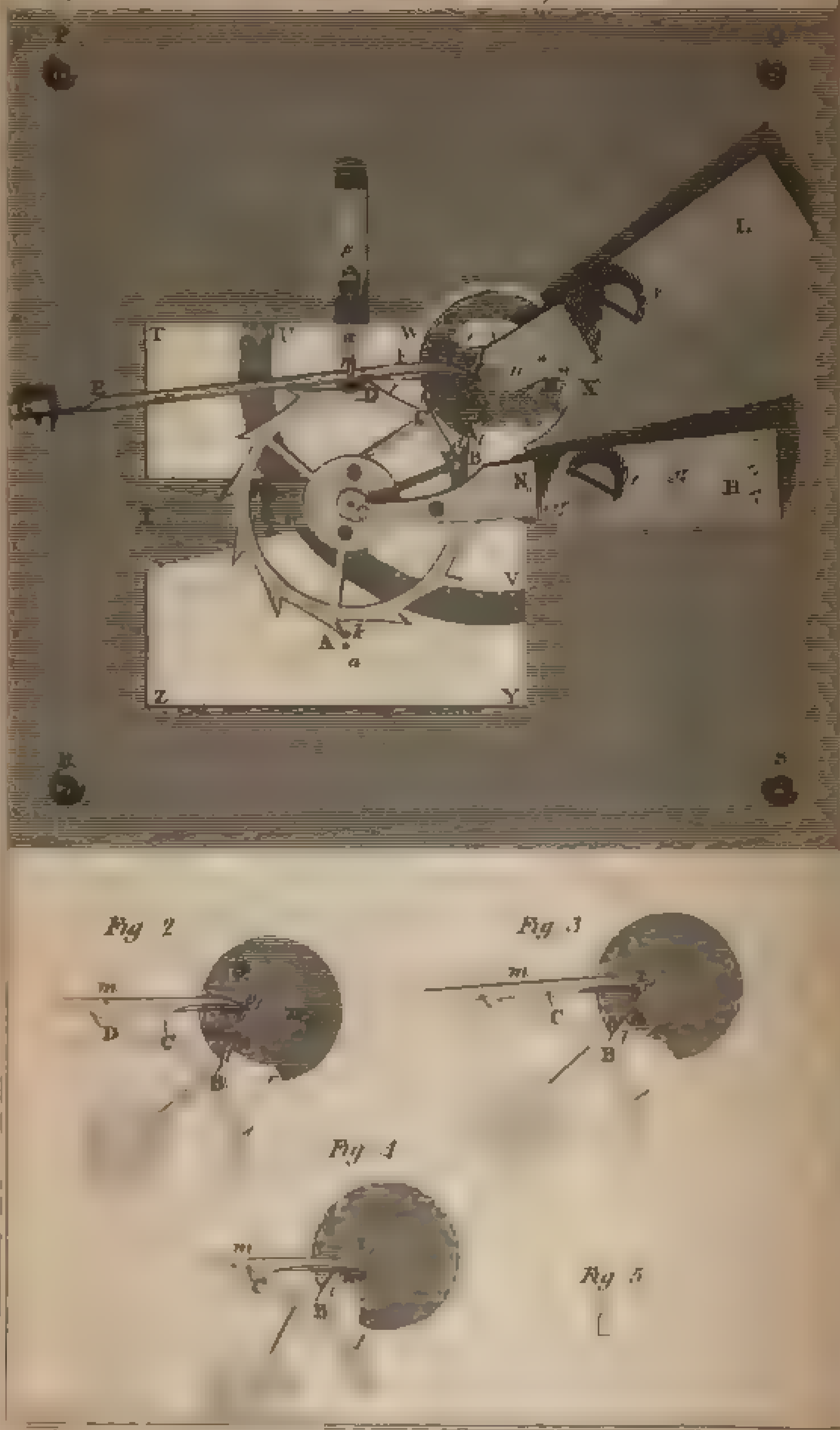
Your's refpectfully,

April 7, 1806. A. B. C.

Extract from Maurice's Indian Antiquities.

"I have already intimated in a former volume, that the *circle* formed around SANI (the Saturn of the Hindoos) by intertwining ferpents, was probably intended to denote his RING. I have fince had the figure engraved for the reader's infpection and decifion. It is impoffible to afcertain the exact age of the pictured image in the Pagoda, from which the portrait was taken;

Fig. 1 Mr Earnshaw's Escapement



taken; but probably both are of a very remote age, for the Indian pagodas are not fabrications of yesterday, nor in their conceptions and designs are they given to frequent vicissitude. Now if Sani were thus designated in very antient periods, the fact proves that they must, by what means can scarcely be conjectured, have discovered the phenomenon of his *ring*, for what besides could that serpentile oval inclosing the body of Sani be intended to represent? That phenomenon however was not known in Europe till about the year 1628, when Galileo, with the first perfect telescope discovered, what he conceived to be two stars at the extreme parts of the planet, but which in reality proved to be the *ansæ* of that ring, the natural existence of which was afterwards demonstrated by Huygens and succeeding astronomers. The circumstance is not the least wonderful of those that occur in the discussion of Indian antiquities and literature. I have stated the fact, and engraved the image; I leave to abler judges the task of decision."

VIII.

Explanation of Time keepers constructed by Mr. Thomas Earnshaw: for which a Reward of Three Thousand Pounds was awarded by the Commissioners of Longitude. From the Communications made by him to the Commissioners *.

THE model, from which the annexed drawings were taken contains, besides the parts necessary to explain the nature of the Escapement, a box inclosing a spring, which when wound up communicates, by means of some more wheels, a force to the balance-wheel, sufficient, when the balance is put in motion, to keep it in action for some time. These wheels are contained between two brass plates, fastened together by four upright

Description of the Escapement of Mr. Earnshaw's time piece.

* The Escapement with a model was communicated in June, 1804, and subsequent explanation in March, 1805. The former is here given, and so much of the latter as directly relates to the time-keepers. The latter paper is no otherwise abridged than by omitting certain observations upon other artists, and some general remarks which do not form part of the disclosure.

I have been solicitous to give as early an account as might be proper, of the Escapements of Mr. Earnshaw and Mr. Arnold which

Description of the Escapement of Mr. Earnshaw's time piece.

upright pillars; the uppermost of these plates is that which is represented by Fig. 1ft. plate XIII, where PQRS are the four screws that take into the heads of the four pillars above mentioned, and connect it to the remaining part of the model. The plate PQRS contains, however, the whole of the parts necessary for the present purpose. The side of this plate represented to view, is the undermost when fixed in the model; so that the figure represents this plate as taken off, with the side next to the balance laid upon a table, and the eye is supposed to be placed perpendicular over it.

In the plate PQRS is an opening, or a piece taken out, represented by TUWXYZ. In this opening, the balance-wheel ABCD, pallet MSK, and part of the balance UV are seen. The balance-wheel is supported by two pieces of brass, ONH, OI; the piece ONH is screwed to the side of the plate nearest to view by a strong screw *t*, and made firm by small pins represented by $\pi\ \pi\ \pi\ \pi\ \pi\ \pi$; these pins are called steady pins; they are riveted fast into the supporting piece OH, and take into holes in the plate PQRS, made exactly to fit them. The part ON of this supporting piece is supposed to be raised above the part *t* H by a joint or bend at N; the other supporting piece OI is fastened to the opposite side of the plate; and between these two pieces the balance-wheel turns freely and steadily in the direction of the letters ABCD. The small wheel MSK is called the large pallet; it is a cylindrical piece of steel, having a notch or piece cut out of it at *l h r*; against the side of this notch is a square flat piece of ruby, or any hard stone, *h l*, ground and polished very smooth, and fixed fast into the pallet. The cylinder is so placed, with respect to the balance-wheel, that it may not be more than just clear of two adjoining teeth. EF is a long thin spring, which

(which last appears in No. 55 of our Journal) as they have been so highly distinguished by the national munificence. Some discussion of the important subject of time pieces may be seen in the Philos. Journal, quarto series, Vol. I. 56, and Vol. II. 106. As I expect shortly to be favoured with a valuable communication respecting the original inventors of free 'Scapements and compensations, and may, according to circumstances, offer a few remarks on the subject myself, I have been careful in the first place to give the accounts of the above mentioned artists in their own words.

W. N.

is

which is made faſt at one end, by being pinned into a ſtud, G, and made to bear gently againſt the head of an adjuſting ſcrew *m*; the other end is bent a little into the form of a hook; to this ſpring there is fixed another very ſlender ſpring at *r*, which projects to a ſmall diſtance beyond it. This ſmall ſpring lies on the ſide of the thick ſpring neareſt to the balance-wheel. The adjuſting ſcrew, *m*, takes into a ſmall braſs-cock, *a p*, which is ſcrewed faſt to the plate PQRS by a ſtrong ſcrew at *p*. Upon the ſpring EF there is fixed a ſemi-cylindrical pin, which ſtands up perpendicular upon it, and of a ſufficient length to fall between the teeth of the balance-wheel ABCD. This pin is called the locking-pallet, and is placed on the oppoſite ſide of the ſpring repreſented to view. Through the center of the cylindrical pallet MSK, a ſtrong ſteel axis paſſes called the verge; the pallet is made faſt to this axis, which alſo paſſes through the center of the balance, and is made faſt to it; it has two ſine pivots at its extremities, upon which it turns very freely, between two firm ſupporting pieces of braſs ſcrewed firmly, and made as permanent as poſſible, by ſteady pins to the principal plate PQRS; one of theſe pieces is repreſented in the figure by *w y* L; the part *w* is raiſed above the part *y* L by a bend or joint at *n*; the part *y* L being repreſented as fixed firm to the plate by the ſtrong ſcrew at *y*. This piece is called the potence, and is exactly ſimilar to the other ſupporting piece, which is called the cock, that is ſimilarly fixed to the oppoſite ſide of the plate and hid from the ſight in the figure. A little above the cylindrical pallet MSK (as it appears in the figure) is fixed a ſmall cylindrical piece of ſteel *in*, having a ſmall part projecting out at *i*, through which the verge alſo paſſes; this is called the lifting pallet; it fixes upon the verge like a collar, and is made faſt by a twiſt, ſo as to be ſet in any poſition with reſpect to the large pallet MSK. The balance lying below the plate PQRS, only the part UV is repreſented to view; the continuation of the poſition of the circumference, however, is repreſented by the dotted lines ULHV. The end EG of the long ſpring EF being made very ſlender, if a ſmall force be applied at the point *o* to preſs that end out from the wheel ABCD, it eaſily yields in that direction, turning as it were upon a center at G; it is alſo made to ſlide in a groove made in this ſtud in ſuch a manner that the end *o* may be placed at any required diſtance

Deſcription of the Eſcapement of Mr. Earnſhaw's time piece.

 from

Description of the Escapement of Mr. Earnshaw's time piece.

from the center of the verge. Having described the several parts as they appear in the figure, we next come to their connexion or situation with respect to each other. Let the long spring EF be supposed to be so placed that the end of the slender spring *yi* may project a little way over the point of the lifting pallet *in*, but not so close but that the point of the pallet may pass by the hooked end of the spring EF without touching it; the head of the adjusting screw *m* is also supposed to bear gently on the inner side of the said spring EF, or that nearest to the wheel, and at the same time the locking pallet is so placed that one of the teeth D, of the balance-wheel, may just take hold of it. This pallet is not visible in its proper place in the figure, being covered from sight by the screw *m*, and part of the spring EF; its position is therefore represented by the dot *k*, on the opposite side of the wheel, having the tooth A just bearing up against it. From the above description of the several parts of the escapement, and their connexion with each other, it will be easy to see the mode of its action, which is as follows.

A force being supposed to be applied to the balance-wheel, so as to cause it to move round in the direction of the letters ABCD, one of the teeth, as D, will come up against the locking pallet (as represented at A, and the locking pallet by *k*). The wheel is then said to be locked, being prevented from moving forward by this pin. Let the balance be now supposed to rest in its quiescent position, and it will have the situation represented in the figure; the lifting point *i*, of the pallet *in*, will be just clear of the projecting end of the slender spring, the face *kl* of the large pallet MSK will fall a little below the point of the tooth B, and the balance having its spiral or helical spring applied to it (which is here supposed on the other side of the plate PQRS, and of course not visible in the figure) remains perfectly at rest in this position. Now as the balance ULHV, and the two pallets MSK and *in*, are fixed fast to the verge, it is plain they must all move together; let therefore the balance be carried a little way round in the direction of the letters VULH; by this motion the end *i* of the lifting pallet *in* will be brought to press up against the projecting end of the slender spring, and as this spring is fixed on the side of the spring EF, nearest to the balance-wheel, the point *i* will press the two springs together out from the balance-wheel; then, as only the point of the tooth D (see its position at *k*) touches the locking

locking pallet, when the ſpring EF was at reſt again the head of the ſcrew *m*, it will, by the ſpring being preſſed out from the tooth, have ſlipped off (for the locking pallet which was before ſuppoſed at *k*, will now be at *a*, clear of the tooth A of the balance-wheel); the wheel being now at liberty will move round by the force ſuppoſed to be applied to it; but as the point *i* of the lifting pallet moves on and preſſes out the ſpring, the point *l* of the large pallet approaches towards the point of the tooth B of the balance-wheel, ſo that when the ſpring EF is ſufficiently puſhed out to unlock the wheel, the point *l* of the large pallet will be got to *d*, and in this poſition the point of the tooth B of the balance-wheel will fall upon it (ſee *Fig*. 2,) where the tooth B is repreſented in contact with the pallet at *l*; at the ſame time the point of the tooth D has juſt dropt off from the locking pallet *m*; the force of the wheel being by this means applied to the top of the pallet *kl*, gives an increaſed momentum to the balance, and aſſiſts it in its motion in the ſame direction, and by the continued motion of the large pallet in the direction MSK the point of the tooth B, which keeps preſſing and urging it forward, moves up towards the bottom of the face of the pallet towards *h*, until the plain flat ſurfaces of the tooth and pallet come into contact (ſee *Fig* 3); by this time the end *o* of the ſlender ſpring has dropt off from the point *i* of the lifting pallet, and the two ſprings have returned again into their quieſcent poſition, the ſpring EF gently bearing againſt the head of the adjuſting ſcrew *m*, and the locking pallet in a poſition to receive the next tooth C of the balance-wheel; (ſee the poſition of the point of the lifting pallet at *i*, *Fig*. 3, alſo the locking pallet at *m*, and the approaching tooth at C.) When the two ſurfaces of the tooth and pallet are thus in contact, the greateſt force of the wheel is exerted upon the pallet, and of courſe upon the balance moving with it. The tooth ſtill preſſing againſt the face of the pallet, and the pallet moving in the direction MSK, it at laſt drops off, (ſee *Fig* 4, where *m* repreſents the poſition of the locking pallet, C the poſition of the tooth of the wheel juſt before it drops upon it, and *l h* the poſition of the face of the large pallet, having the point of the tooth B juſt ready to leave it at *l*,) leaving the balance at perfect liberty to move on in the ſame direction in which it was going. Juſt as the point of the tooth B, which has been preſſing the large pallet round, is ready to leave it,

Deſcription of the Eſcapement of Mr. Earnſhaw's time piece.

Description of the Escapement of Mr. Earnshaw's time piece.

the next tooth C of the wheel is almost in contact with the locking pallet *m* (see *Fig.* 4) so that the instant the tooth D drops off the wheel is again locked, and the action of that tooth upon the balance is finished. As the balance moves with the greatest freedom upon its pivots, the force of the tooth has given it a considerable velocity, so that the balance still keeps moving on in the same direction, after the pressure of the tooth is removed by slipping off from the pallet, until the force of the pendulum spring (which is not represented in the figure) being continually increased by being wound up, overcomes the momentum of the balance, which, for an instant of time, is then stationary, but immediately returns by the action of the pendulum spring, which exerts a considerable force upon it in unwinding itself. As the balance returns, the point *i* of the lifting pallet *in* passes by the ends of the two springs EF and *yo*, and, in passing by, pushes the projecting end, *o*, of the slender spring in towards the balance-wheel, until it has passed it; which, as soon as it has done, the projecting end *o* again returns and applies itself close to the hooked end of the spring EF, as before. The spring *yo* is made so slender, that it gives but little resistance to the balance, during the time the point *i* of the lifting pallet is passing it, and of course causes but little (if any) decrease in its momentum. During the time the point *i* of the lifting pallet is passing the small spring *yo*, the long spring EF remains steadily bearing against the head of the adjusting screw *m*, as the hooked end at *o* just lets the end of the lifting pallet pass by without touching of it. As the spring has now been continually acting upon the balance, from the extremity of its vibration in the direction MSK, it has given it the greatest velocity, when the point *i* of the lifting pallet is passing the end *o* of the slender spring; for at this instant the spring which was wound up by the contrary direction of the balance, is now unwound again, or in the same state as it was in its quiescent position at first, and of course has no effect upon the balance at all in either direction; but the balance having now all the velocity it could acquire from the unwinding of the spring, goes on in the direction UVHL, until the force of this spring again stops it and brings it back again, moving in the same direction as at first, with a considerable velocity. By this return of the balance, the point *i* of the lifting pallet comes up again to the projecting end *o* of the slender spring, pushes

pushes back the long ſpring EF, and unlocks the wheel; and another tooth falling upon the face of the pallet *hl* gives freſh energy to the balance: and thus the action is carried on as before.

The Eſcapement ſhould be made in the following manner: The pivots of the balance axis ſhould be the ſize of the verge-pivots of a good common ſized pocket watch, and of the ſhape of *Fig.* 5. Pl. XIII, which will greatly add to their ſtrength, the extreme end, or acting part only being ſtraight; the jewel hole ſhould be as ſhallow as poſſible, ſo as not to endanger cutting the pivot, and the part of action of the hole made quite back with only a very ſhallow chamfer behind to retain the oil; deep holes are very bad, for when the oil becomes glutinous, it will make the pivots ſtick ſo as to prevent the balance from its uſual vibration. The pallet ſhould be half the diameter of the wheel, or a little larger, for if ſmaller, the wheel will then have too much action on it, which will increaſe friction moſt conſiderably, and likewiſe cauſe the balance to ſwing ſo much farther to clear the wheel; conſequently a check in the motion of the balance may ſtop the watch. The face of the pallet ſhould run in a line of equal diſtance between the centre of the pallet and its extremity, and not in a right line to its centre, that is an increaſe of friction, and a loſs of that power which is obtained by the wheel acting on the extremity of the pallet; this is clearly proved by time, by the hole worn by the points of teeth in all pallets that run in a line to the centre. The ſcape wheel teeth ſhould form the ſame direction as the face of the pallet, under cut for the ſaid purpoſe of avoiding friction, and maintaining the power, and for ſafe locking. The points of the wheel teeth muſt not be rounded off, but left as ſharp as poſſible. The pivots of the ſcape wheel are to be a very little larger than the balance pivots.

Inſtructions for making the Eſcapement.

The wheel is locked by a ſpring inſtead of a detent with pivots, as the French have made them, for thoſe pivots muſt have oil, and when the oil thickens then the ſpring of the pivot detents is ſo affected by it as to prevent the detent from falling into the wheel quick enough, the conſequence of this is irregular time and ſtoppage of the watch, and if ever ſuch a watch went well for twelve months, chance muſt have had by far the moſt hand in it.

Detent with ſpring joint.

When the ſpring is planted on the ſide of the wheel, as in my eſcapements, the part on which the wheel reſts ſhould be a little

How to place the detent, pallets, &c.

a little short of a right angle, so that the wheel may have a tendency to draw the spring into it, for if stopped the other way, or beyond a right angle it will have a tendency to push the spring out; in that case the wheel will have liberty to run; the wheel should take no more hold on the spring than just sufficient to stop it, for if more, friction will be increased. The small return spring should be as thin as possible at the end fastened to the other spring, but at the outer end a little thicker; the spring should be planted down as close to the wheel as to be just free of it. The discharging pallet about one-third, or near one-half the size of the large or main pallet, the face of it in a right line to the centre, the back of it a little rounding and off from the centre. Great care must be used in taking off the edges of this discharging piece, to make it round to prevent cutting the spring, nor can it be made too thin so it does not cut; the end of it nearest the ballance should be a little more out from the centre of the ballance axis than the lower part of it towards the potence, for counteracting the natural tendency of the spring downwards from the pressure of the scape wheel; and that part of the spring on which the wheel rests should be stopped a little down to give the wheel a tendency to force it up, to counteract the natural inclination the wheel has to draw it down by its pressure on it.

Construction of the balance with its compensation weights, &c.

The balance is to be made of the best steel, and turned from its own centre to its proper size, then put it into a crucible with as much of the best brass as when melted will cover it. The brass melted will adhere to the steel (for if any other metal is used by way of solder, that watch cannot go well), then turn it to its proper thickness, and hollow it out so as to leave the steel rim about the thickness of a repeating spring to a small sized repeating watch, turn the brass to twice or near three times that thickness of steel, cross it out with only one arm straight across the centre, and at each end of the arm fix two screws opposite to each other through the rim of the balance to regulate the watch to time, the diameter of the heads of these screws about equal to the thickness of the balance, a little more or less is not material. The compensation weights should be made of the best brass and well hammered, and a groove turned to let the rim of balance into it, and this should be cut into fourteen equal parts by a wheel engine,

gine, then you will have seven pair of pieces of equal size and weight; two of these pieces being screwed on the rim of the balance at equal distances will produce an equilibrium, a balance in the full sense of the word, equal in all its parts. In making balances great care should be taken that they get no bruises or bendings, for if they get a bruise on one side so as to indent the metal, that part will be less affected by heat and cold than the other parts which have not received the same violence to close its pores.

Adjustment to keep time in all temperatures,

To adjust the balance in heat and cold—put the watch into about 85 or 90 degrees of heat, by the common thermometer, mark down exactly how much it gains or loses in 12 hours, then put it into as severe cold as you can get for 12 hours, and if it gains one minute more in 12 hours in cold than in heat, move the compensation weights farther from the arm of the balance about $\frac{1}{8}$ of inch, and if it gains one minute more in 12 hours in heat than in cold move the weights $\frac{1}{8}$ of inch nearer to the arm of the balance, and so on in the like proportion, trying it again and again till you find the watch go the same in whatever change of heat or cold you put it.

—and in all positions,

Much difficulty has fallen to the lot of watchmakers in the endeavour to make timekeepers go nearly the same in the different positions. I have had my share of this, but it is now over; by far the greatest part of this difficulty arises from the balance spring not being properly made. But if the spring is made, as I shall describe hereafter, you have only to make the balance of equal weight and it will go within a few seconds per day in all positions alike, and if it vibrates not more than one circle and a $\frac{1}{4}$, by applying a small matter of weight to that part of the balance which is downward when in the position that it loses most, will correct it with great accuracy; but if it vibrates more than one circle and a $\frac{1}{4}$, then it will require the weight to be above instead of below; and after the watch has been going a few months and its vibration shortens to one $\frac{1}{4}$ circle, then it will go worse and worse by reason of the weight being in the wrong place; therefore, to avoid this evil, it is absolutely necessary to confine the vibrations to one $\frac{1}{4}$ circle, which will produce the most steady performance. It is common for watchmakers to adore a timekeeper when they see it vibrate a circle and a half, or more, and form an opinion of its excellence from this only; but I know

Rule. If the balance be in poise it will go nearly alike in all positions. Correct it by adding weight to the lowest part when in losing position if the same vibration be about 200°; if more degrees then add to the upper part. Hence a moderate vibration is best.

know from experience what would be the confequence, and have been condemned, becaufe, when I have feen fuch watches I faid I faw enough to declare that it would not give very accurate performance.

Concerning the balance fpring.

Balance fpring. To find out the invifible properties of this apparent fimple part of the machine, has given much more trouble than all the reft. I defpaired of bringing timekeepers to the ftate I have done, and unlefs thofe hidden properties are known to timekeeper makers, however well they may execute all other parts they will find their moft fanguine expectations fruftrated. I have feen watchmakers boaft of their timekeepers going well for a month or two, and from the knowledge I had of the effects produced by the balance fpring, I have told them that a month or two more would deftroy their hopes. The cylindrical fpring being in all its turns of equal diftance from the centre, in courfe every turn will be of equal ftrength, and called ifochronal, and believed that all vibrations whether long or fhort would be performed in the fame time; but this is not true, for if a man is to go four miles in the fame time as he has gone one mile, he cannot do it with the fame power; no, he muft have impelling force to quicken his motion, or he will be four times as long in doing it.

It is made tapering.

Therefore inftead of the fpring being equal in all its parts, it muft be made to increafe in thicknefs to the outer end, in fuch proportion as will caufe the balance when thrown to a greater diftance to return fo much the quicker to make them equal; by long perfeverance I found how to make fuch fprings, and then I thought I had got all I wifhed for. But cruel difappointment nearly broke my heart, for I found I had yet another difficulty to break down, as my watches with fuch perfect fprings were continually lofing on their rates. What farther to do I knew not, and I own I was nearly if not quite mad. But obftinate in the caufe and refolving not to give it up but with life, perfeverance came once more to my aid—and with ftill more unremitting ftudy, which nearly finifhed me, before I applied the following remedy for the before mentioned evil, I found, in the courfe of

Springs are fubject to a relaxation of force which is regained by reft.

reafoning on bodies, that watch fprings relax and tire like the human frame, when kept conftantly in motion, and this may be proved by the following experiment: let a watch that has been going a few months go down, let it be down for a week

or

or two, or more, then set it going, and if it be a good timekeeper so as not to be affected by the weather it will go some seconds per day faster than it did when it was let down, but it will again lose its quickness in a gradual manner gaining less and less till it comes to its former rate. Therefore finding that isochronal springs would not do—and likewise having made springs of such shape as would render long and short vibrations equal in time—constantly lose the longer the watch went, I then made them of such shape as to gain in the short vibrations about five or six seconds per day more than the long ones, this quantity could only be found by long experience, and the way I proved this was to try the rate of the watch with the balance vibrating about ¼ of circle, then tried its rate vibrating one circle and a ½, and if the short vibrations go slower than the long ones that watch will lose on its rate, and if they are equal, it will likewise lose, but that only from relaxation, and if it gains in the short vibrations more than five or six seconds in twenty-four hours it will in the long run gain on its rate, but if not more than that quantity, and the timekeeper is perfect in heat and cold and every other part, the above properties will render it deserving of the name of a perfect timekeeper, and this is a principal cause of my timekeepers excelling all others, and this the principal cause of some of my timekeepers going better than others, though made by me, the springs of them being made to accord more exactly to the above proportions; and this is the cause which has enabled me to foretel what my timekeepers would do, which Dr. Maskelyne, Mr. Crosley, and others can testify. The above effect is produced as follows. I find the common relaxation of balance springs to be about five or six seconds per day on their rates in the course of a year, therefore if the short vibrations are made by the shape of the spring to go about that quantity faster than the long ones, and as the spring relaxes in going by time so the watch accumulates in dirt and thickening of the oil which shortens the vibrations, the short ones then being quicker, compensates for the evil of relaxation of the balance spring. From this it is plain, that the causes of error in timekeepers are not undefined and vague in their nature, which has been supposed; for when it is certain that all causes of error may be over compensated we cannot despair of finding the medium, and which

This gradual effect causes a loss on the rate which may be compensated by giving greater speed to the shorter vibrations in the first construction.

which may be easily proved by examining the going of my timekeepers. It will there appear that what errors, they are subject to, arise from causes certain and natural, and in course may be corrected by art*.

IX.

Experimental Enquiry into the Proportion of the several Gases or Elastic Fluids, constituting the Atmosphere. By JOHN DALTON.†

On the component parts of the atmosphere.

IN a former paper which I submitted to this society, "on the constitution of mixed gases," I adopted such proportions of the simple elastic fluids to constitute the atmosphere as were then current, not intending to warrant the accuracy of them all, as stated in the said paper; my principal object in that essay was, to point out the *manner* in which mixed elastic fluids exist together, and to insist upon what I think a very important and fundamental position in the doctrine of such fluids:—namely, that the elastic or repulsive power of each particle is confined to those of its own kind; and consequently the force of such fluid, retained in a given vessel, or gravitating, is the same in a separate as in a mixed state, depending upon its proper density and temperature. This principle accords with all experience, and I have no doubt will soon be perceived and acknowledged by chemists and philosophers in general; and its application will elucidate a variety of facts, which are otherwise involved in obscurity.

Objects of this essay

The objects of the present essay are,

1. To determine the weight of each sep. atmosphere.

1. To determine the weight of each simple atmosphere, abstractedly; or, in other words, what part of the weight of the whole compound atmosphere is due to azote; what to oxygen, &c. &c.

—and the relative weights of of the gases at the surface of the earth

2. To determine the relative weights of the different gases in a given volume of atmospheric air, such as it is at the earth's surface.

* To this communication Mr. Earnshaw has annexed two plates with descriptions, shewing the parts of his time-piece; all which, except those of the Escapement (which we have given) are capable of the same variations as those of any other good movements. He asserts that the best train for time keepers is 18,000; that the scape wheel for pocket ones should have 15 teeth, and for box ones 18 teeth.

† Manchester Mem. Vol. V.

3. To investigate the proportions of the gases to each other, such as they ought to be found at different elevations above the earth's surface. —as well as at different elevations.

To those who consider the atmosphere as a chemical compound, these *three* objects are but *one*; others, who adopt my hypothesis, will see they are essentially distinct. With respect to the first: It is obvious, that, on my hypothesis, the density and elastic force of each gas at the earth's surface, are the effects of the weight of the atmosphere of that gas solely, the different atmospheres not gravitating one upon another. Whence the first object will be obtained by ascertaining what share of elastic force is due to each gas in a given volume of the compound atmosphere; or, which amounts to the same thing, by finding how much the given volume is diminished under a constant pressure, by the abstraction of each of its ingredients singly. Thus, if it should appear that by extracting the oxygenous gas from any mass of atmospheric air, the whole was diminished $\frac{1}{5}$ in bulk, still being subject to a pressure of 30 inches of mercury; then it ought to be inferred that the oxygenous atmosphere presses the earth with a force of six inches of mercury, &c.

Now each single atmosphere presses by its whole weight; which is measured by its spring and that by its volume.

Take away one of the gases and the loss of volume represents its pressure and the weight of that atmosphere.

In order to ascertain the second point, it will be further necessary to obtain the specific gravity of each gas; that is, the relative weights of a given volume of each in a pure state, subject to the same pressure and temperature. For the weight of each gas in any given portion of atmospheric air, must be in the compound ratio of its force and specific gravity.

The weights of each gas in given volume had from sp. gravity.

With respect to the third object, it may be observed, that those gases which are specifically the heaviest must decrease in density the quickest in ascending. If the earth's atmosphere had been a homogeneous elastic fluid of the same weight it is, but ten times the specific gravity, it might easily be demonstrated that no sensible portion of it could have arisen to the summits of the highest mountains. On the other hand, an atmosphere of hydrogenous gas of the same weight, would support a column of mercury nearly 29 inches on the summit of Mount Blanc.

The proportions at different heights are obtained from the progression with each gas in the same manner as in computation for the whole atmosphere.

The several gases constantly found in every portion of atmospheric air, and in such quantities as are capable of being appreciated, are azotic, oxygenous, aqueous vapour, and carbonic acid. It is probable that hydrogenous gas also is con-

ftantly prefent; but in fo fmall proportion as not to be detected by any teft we are acquainted with; it muft therefore be confounded in the large mafs of azotic gas.

1. *On the weight of the Oxygenous and Azotic Atmofpheres.*

Proceffes for determining the oxigen in the atmofphere.

Various proceffes have been ufed to determine the quantity of oxygenous gas.

1. with nitrous gas.

1. The mixture of nitrous gas and air over water.

2. with fulphuret.

2. Expofing the air to liquid fulphuret of potafh or lime, with or without agitation.

3. Explofion with hidrogen.

3. Exploding hydrogen gas and air by electricity.

4. Expofure to green fulphat of iron.

4. Expofing the air to a folution of green fulphat or muriat of iron in water, ftrongly impregnated with nitrous gas.

5. Burning phosphorus.

5. Burning phofphorus in the air.

All produce the fame refult.

In all thefe cafes the oxygen enters into combination and lofes its elafticity; and if the feveral proceffes be conducted fkilfully, the refults are precifely the fame from all. In all parts of the earth and at every feafon of the year, the bulk of any given quantity of atmofpheric air appears to be reduced nearly 21 per cent. by abftracting its oxygen. This fact, indeed, has not been generally admitted till lately; fome chemifts having found, as they apprehended, a great difference in the quantity of oxygen in the air at different times and places; on fome occafions 20 per cent. and on others 30, and more of oxygen are faid to have been found. This I have no doubt was owing to their not underftanding the nature of the operation and of the circumftances influencing it. Indeed it is difficult to fee, on any hypothefis, how a difproportion of thefe two elements fhould ever fubfift in the atmofphere.

The oxigen and azote are not variable.

The firft procefs with nitrous gas tho' difcredited, is here perfected.

As the firft of the proceffes above-mentioned has been much difcredited by late authors, and as it appears from my experience to be not only the moft elegant and expeditious of all the methods hitherto ufed, but alfo as correct as any of them, when properly conducted, I fhall, on this occafion, animadvert upon it,

Inftructions for the procefs.

Pure nitrous gas.

1. Nitrous gas may be obtained pure by nitric acid diluted with an equal bulk of water poured upon copper or mercury; little or no artificial heat fhould be applied. The laft product of gas this way obtained, does not contain any fenfible portion of azotic gas: at leaft it may eafily be got with lefs than two or three *per cent.* of that gas: It is probably nearly free from nitrous oxide alfo, when thus obtained.

2. If 100 meafures of common air be put to 36 of pure nitrous gas in a tube 3-10th of an inch wide and 5 inches long, after a few minutes the whole will be reduced to 79 or 80 meafures, and exhibit no figns of either oxygenous or nitrous gas.

Mixture 100 air and 36 n. gas in a narrow tube. Refidue about 80 azote.

3. If 100 meafures of common air be admitted to 72 of nitrous gas in a wide veffel over water, fuch as to form a thin ftratum of air, and an immediate momentary agitation be ufed, there will, as before, be found 79 or 80 meafures of pure azotic gas for a refiduum.

Mixture 100 air and 72 n. gas in a wide veffel with agitation. Refidue as before 80 azote.

4. If, in the laft experiment, *lefs* than 72 meafures of nitrous gas be ufed, there will be a refiduum containing oxygenous gas; if *more*, then fome refiduary nitrous gas will be found.

Intermediate proportion of n. gas leaves either n. gas or oxigen with the azote.

Thefe facts clearly point out the theory of the procefs: the elements of oxygen may combine with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity. In the former cafe *nitric* acid is the refult; in the latter *nitrous* acid: but as both thefe may be formed at the fame time, one part of the oxygen going to *one* of nitrous gas, and another to *two*, the quantity of nitrous gas abforbed fhould be variable; from 36 to 72 *per cent.* for common air. This is the principal caufe of that diverfity which has fo much appeared in the refults of chemifts on this fubject. In fact, all the gradation in quantity of nitrous gas from 36 to 72 may actually be obferved with atmofpheric air of the fame purity; the wider the tube or veffel the mixture is made in, the quicker the combination is effected, and the more expofed to water, the greater is the quantity of *nitrous* acid and the lefs of *nitric* that is formed.

Theory of the procefs. In the firft cafe nitric gas was formed; in the latter nitrous.

To ufe nitrous gas for the purpofe of eudiometry therefore, we muft attempt to form *nitric acid* or *nitrous* wholly, and without a mixture of the other. Of thefe the former appears from my experiments to be moft eafily and moft accurately effected. In order to this a narrow tube is neceffary; one that is juft wide enough to let air pafs water without requiring the tube to be agitated, is beft. Let little more nitrous gas than is fufficient to form nitric acid be admitted to the oxygenous gas; let no agitation be ufed; and as foon as the diminution appears to be over for a moment let the refiduary gas be transferred to another tube, and it will remain without any further diminution of confequence. Then $\frac{7}{19}$ of the lofs will be due

Practical refult. Operate fo as to form the nitric gas.

to

to oxygen. The transferring is necessary to prevent the nitric acid formed and combined with the water, from absorbing the remainder of the nitrous gas to form nitrous acid.

Method with sulphuret.

Sulphuret of lime is a good test of the proportion of oxygen in a given mixture, provided the liquid be not more than 20 or 30 per cent. for the gas (atmospheric air); if the liquid exceed this, there is a portion of azotic gas imbibed somewhat uncertain in quantity.

Volta's method.

Volta's eudiometer is very accurate as well as elegant and expeditious: according to Monge, 100 oxygen require 196 measures of hydrogen; according to Davy 192; but from the most attentive observations of my own, 185 are sufficient. In atmospheric air I always find 60 per cent. diminution when fired with an excess of hydrogen; that is, 100 common air with 60 hydrogen, become 100 after the explosion, and no oxygen is found in the residuum; here 21 oxygen take 39 hydrogen.

2. *Of the Weight of the Aqueous Vapour Atmosphere.*

To find the weight of aqueous vapour in the atmosphere.

I have, in a former essay, (Manchester Mem. vol. 5. p. 2, page 559.) given a table of the force of vapour in *vacuo* for every degree of temperature, determined by experiment; and in the sequel of the essay, have shewn that the force of vapour in the atmosphere is the very same as in *vacuo*, when they are both at their utmost for any given temperature. To find the force of aqueous vapour in the atmosphere, therefore, we have nothing more to do than to find that degree of cold at which it begins to be condensed, and opposite to it in the table abovementioned, will be found the force of vapour. From the various facts mentioned in the essay it is obvious, that vapour contracts no chemical union with any of the gases in the atmosphere; this fact has since been enforced in the Annales de Chimie, vol. xlii. by Clement and Deforme.

M. De Saussure found by an excellent experiment, that dry air of 64° will admit so much vapour as to increase its elasticity, $\frac{1}{54}$. This I have repeated nearly in his manner, and found a similar result. But the table he has given us of aqueous vapour at other temperatures is very far wrong, especially at temperatures distant from 64°. The numbers were not the result of direct experiment, like the one above. If we could obtain the temperatures of all parts of the earth's surface,

furface, for any given time, a mean of them would probably be 57° or 58°. Now if we may fuppofe the force of vapour equivalent to that of 55°, at a medium, it will, from the table, be = to .443 of mercury; or, nearly $\frac{1}{70}$ of the whole atmofphere. This it will be perceived is calculated to be the weight of vapour in the whole atmofphere of the earth. If that incumbent over any place at any time be required, it may be found as directed above.

It is on an average about one feventieth.

3. *Of the Weight of the Carbonic Acid Atmofphere.*

From fome obfervations of Humboldt, I was led to expect about $\frac{1}{100}$ part of the weight of the atmofphere to be carbonic acid gas: but I foon found that the proportion was immenfely over-rated. From repeated experiments, all nearly agreeing in their refults, and made at different feafons of the year, I have found, that if a glafs veffel filled with 102,400 grains of rain water be emptied in the open air, and 125 grains of ftrong lime water be poured in, and the mouth then clofed; by fufficient time and agitation, the whole of the lime water is juft faturated by the acid gas it finds in that volume of air. But 125 grains of the lime water ufed require 70 grain meafures of carbonic acid gas to faturate it: therefore, the 102,400 grain meafures of common air contain 70 of carbonic acid; or $\frac{1}{1460}$ of the whole. The weight of the carbonic acid atmofphere then is to that of the whole compound as 1:1460; but the weight of carbonic acid gas in a given portion of air at the earth's furface, is nearly $\frac{1}{1000}$ of the whole; becaufe the fpecific gravity of the gas is $1\frac{1}{2}$ that of common air. I have fince found that the air in an affembly, in which two hundred people had breathed for two hours, with the windows and doors fhut, contained little more than 1 per cent. of carbonic acid gas.

Deduction of the weight of carbonic acid in the atmofphere about one thoufandth.

Having now determined the force with which each atmofphere preffes on the earth's furface, or in other words, its weight; it remains next to enquire into their fpecific gravities.

These

Specific gravities of gases.

These may be seen in the following Table.

Atmospheric air	1.000
Azotic gaz	.966
Oxygenous gaz	1.127
Carbonic acid gas	1.500
Aqueous vapour	.700
Hydrogenous gas	.077*

Kirwan and Lavoisier are my authorities for these numbers; except oxygenous gas and aqueous vapour. For the former I am indebted to Mr. Davy's Chemical Researches; his number is something greater than theirs: I prefer it, because, being determined with at least equal attention to accuracy with the others, it has this further claim for credit, that 21 parts of gas of this specific gravity, mixed with 79 parts of azotic gas; make a compound of exactly the same specific gravity as the atmosphere, as they evidently ought to do, setting aside the unfounded notion of their forming a *chemical* compound. The specific gravity of aqueous vapour I have determined myself both by analytic and synthetic methods, after the manner of De Saussure; that is, by abstracting aqueous vapour of a known force from a given quantity of air, and weighing the water obtained—and admitting a given weight of water to dry air and comparing the loss with the increased elasticity. De Saussure makes the specific gravity to be ,71 or ,75; but he used caustic alkali as the absorbent, which would extract the carbonic acid as well as the aqueous vapour from the air. From the experiments of Pictet and Watt, I deduce the specific gravity of aqueous vapour to be ,61 and ,67 respectively. Upon the whole, therefore, it is probable that ,7 is very nearly accurate.

We have now sufficient data to form tables answering to the two first objects of our enquiry.

* The specific gravity of hydrogen must be rated too low: if 100 oxygen require 185 hydrogen by measure, according to this 89 oxygen would require only 11 hydrogen to form water; whereas 85 require 15. Hydrogen ought to be found about $\frac{1}{10}$ part of the weight of common air.

I. Table

I. *Table of the Weights of the different Gases constituting the Atmosphere.*

	Inch of Mercury.
Azotic gas - - - - -	23.36
Oxygenous gas - - - -	6.18
Aqueous vapour - - - -	.44
Carbonic acid gas - - - -	.02
	30.00

Absoluteweights of the different gases in the whole atmosphere.

II. *Table of the proportional Weights of the different Gases in a given Volume of Atmospheric Air, taken at the Surface of the Earth.*

	per cent.
Azotic gas - - - - -	75.55
Oxygenous gas - - - -	23.32
Aqueous vapour - - - -	1.03*
Carbonic acid gas - - - -	—.10
	100.00

Weights of the different gases in equal bulks at the earth's surface.

III. *On the Proportion of Gases at different Elevations.*

Computation of the proportion of gases above the earth's surface. It is not notably different at any accessible height.

M. Berthollet seems to think that the lower strata of the atmosphere ought to contain more oxygen than the upper, because of the greater specific gravity of oxygenous gas, and the slight affinity of the two gases for each other. (See Annal. de Chimie, Tom. 34. page 85.) As I am unable to conceive even the possibility of two gases being held together by affinity, unless their particles unite so as to form *one* centre of repulsion out of two or more (in which case they become *one* gas) I cannot see why rarefaction should either decrease or increase this supposed affinity. I have little doubt, however, as to the fact of oxygenous gas observing a diminishing ratio in ascending; for, the atmospheres being independent on each other, their densities at different heights must be regulated by their specific gravities. Hence, if we take the azotic atmo-

* The proportion of aqueous vapour must be understood to be variable for any one place: the others are permanent or nearly so.

ſphere as a ſtandard, the oxygenous and the carbonic acid will obſerve a decreaſing ratio to it in aſcending, and the aqueous vapour an increaſing one. The ſpecific gravity of oxygenous and azotic gaſes being as ſeven to ſix nearly, their diminution in denſity will be the ſame at heights reciprocally as their ſpecific gravities. Hence it would be found, that at the height of Mount Blanc (nearly three Engliſh miles) the ratio of oxygenous gas to azotic in a given volume of air, would be nearly as 20 to 80;—conſequently it follows that at any ordinary heights the difference in the proportions will be ſcarcely if it all perceptible *.

X.

Obſervation which indicates a ſpontaneous Decompoſition of nitrous Acid and Formation of Ammonia. By D. A.

To Mr. NICHOLSON.

SIR,

Decompoſition of nitrous acid.

I SEND you a ſtatement of the following fact, in caſe it may not hitherto have been obſerved: it ſeems to ſhew the mutual decompoſition of nitrous acid and atmoſpheric air; but the explanation of the theory I will leave to you, or ſome of your learned correſpondents. A phial of bright orange coloured nitrous acid, ſo looſely ſtopped that bubbles of gas eſcaped every five or ten minutes, having ſtood within a few inches of a bottle of muriatic acid, cloſely ſtopped for above a twelvemonth, my attention was attracted by obſerving a white incruſtation of ſalts upon the label paper of the laſt mentioned phial. To determine their nature, diſſolved them in diſtilled water; dropped a little nitric acid in, to ſaturate any uncombined alcalies; then with nitrate of ſilver, a copious precipitate was formed, which indicates the muriatic to be the acid: when I ſaturated the acids with pure potaſs, the

* Air brought from the ſummit of Helvelyn, in Cumberland (1100 yards above the ſea—Barometer being 26,60) in July 1804, gave no perceptible difference from the air taken in Mancheſter.—M. Gay-Luſſac determines the conſtitution of air brought from an elevation of four miles to be the ſame as that at the earth's ſurface.

ſmell

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Waltire

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THE END OF THE THIRTEENTH VOLUME.

Printed by W. Stratford, Crown-Court, Temple-Bar.

mell refembled ammonia; but owing to the folution being fo extremely weak, was fcarce perceptible; but on a finger being dipped into it, and held near a ftopper, moiftened with muriatic acid, evidently produced a white cloud, which difappeared upon withdrawing the acid, and re-appeared on its approach; which teft alone I think may be fufficient to prove ammonia to have been the bafe. I may obferve, that the falts were formed only on that part of the table on which fome muriatic acid had been fpilt; the neck of the nitrous acid phial was covered with a moifture, which had a confiderable ammoniacal fmell, and exhibited the fame appearances with the moiftened ftopper, and was therefore uncombined ammonia, and feems to fhew that the prefence of the muriatic acid was not neceffary for its formation. I have endeavoured to be as concife as poffible, and remain

Your conftant reader,

April 17, 1806. D. A.

SCIENTIFIC NEWS.

Note on the Porcelain of Reaumur Communicated by Veau de Launau.*

M. PECARD of Tours, manufacturer of Rouen ftone ware, has repeated in his furnace Reaumur's experiment of transforming glafs into porcelain; mentioned in the memoir of the Academy of Sciences, for the year 1739, p. 370. M. Pecard obtained a devitrification as complete within as without. His experiment was made upon a common glafs bottle from the Ancenis Foundry. The bottle was filled with Nevers fand, and depofited in a *fagger*, which was afterwards filled up with the fame fort of fand. The fagger or cafe was placed with others, containing earthenware in the chimney or upper part of the furnace, and heated as ufual. When the operation was finifhed, and the furnace was fufficiently cooled, the bottle was taken from its bed of fand in the fagger, and emptied of its contents. The bottle had undergone no alteration of fhape; but its green colour and tranfparency were exchanged for milky opacity, equally fpread over all parts of the bottle. In this, his firft experiment,

Reaumur's porcelain m in an impro ftate.

* Journal de Phifique, Vol. LXI. p. 401.

periment, M. Pecard has obtained a much more equal devitrification than that procured by Reaumur; who remarks in his memoir, that he thinks it not impossible that this point of equality between the internal and external parts may be obtained.

This substance is much harder than glass; it readily gives a spark with steel; and from the advantages it seems to hold forth in many respects deserves to be made an object of investigation.

Darcet applies the glass porcelain to useful purposes.

A distinguished chemist, who pursues the steps of his father, whose name will be ever dear to the sciences, and to those who cultivate them, M. Dárcet, has already made several experiments on this interesting subject, which form part of a work not yet completed. He has made mullers of this substance, exceeding the hardness of flint; also capsules and other articles which easily support the fire, and are not subject to the power of re-agents, such as sulphuric acid, &c. The little cost of the materials whereof these vessels, &c. are fabricated, induce a hope that the labours of Reaumur on this subject will be resumed, and carried on in a way that will be of utility in different arts.

Anatomical Work.

AN extensive work on the anatomy of the organs of hearing in animals, generally, together with the physiology of their several parts, and a series of accoustic phenomena intended to elucidate the subject, is in forwardness for publication this spring by Anthony Carlisle, F. R. S. F. L. S. and surgeon to the Westminster Hospital.

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Pilon's

A JOURNAL OF NATURAL PHILOSOPHY, *CHEMISTRY,* AND THE ARTS.

VOL. XIV.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers in the present Volume are, Sir H. C. Englefield, Bart. M. P. F. R. S.; H. B. K.; A Correspondent; W. N.; Mr. Sylvester; J. Dalton; J. Kidd, M. D.; J. Bostock, M. D.; Mr. J. Arnold; M. M.; Juvenis; T. B.; James Horsburgh, Esq.; G. S. Gibbes, M. D.; R. B.; Dr. Buchan; Dr. Wilkinson; H. Hamill, jun. Esq.; H. K.; Count Rumford, F. R. S.; Mr. Thomas Reid; Mr. W. Hardy; Dr. Thompson.

Of Foreign Works, Professor Proust; M. Bouillon Lagrange; M. Darcet Cointeraux; Thenard; J. F. Westring; Lavoisier; Laplace.

And of English Memoirs abridged or extracted, Sir James Hall, Bart. F. R. S. &c.; Mr. John Arnold; T. Young, M. D. For. Sec. R. S.; T. A. Knight, Esq.; Thos. Holden; Mr. T. Vanherman; Mrs. Jane Richardson; H. Davy, Esq. F. R. S.; W. Brande, Esq.; Mr. Earnshaw.

Of the Engravings the Subjects are, 1. A new Lamp, by Count Rumford: 2. The Escapement of Arnold's Chronometer: 3. Arnold's Expansion Balance for Time-pieces: 4. The Water Ram of Mongolfier: 5. Holden's Machine for Shoe-making; 6. A Galvanic Apparatus, by Mr. Sylvester: 7. Designs for Furnaces, and Apparatus for ascertaining the Force of Compression, by Sir James Hall, Bart. F. R. S. Ed.: 8. Sections and Drawings to illustrate the Huttonian Theory as established on Sir James Hall's Theory: 9. Tools and Implements used in building Houses of rammed Earth: 10. Drawing of a Building constructed by that Art: 11. The Thermometrical Balance of Peter le Roy: 12. Expansion Balance, by the same Artist: 13. Le Roy's modern detached Escapement: 14. The same improved; 15. The same as altered by Berthoud: 16. The same by Arnold: 17. The same by Earnshaw: 18. Ancient free Escapement, by Thiout: 19. New Apparatus by Count Rumford for Experiments on Heat: 20. Mr. Harding's Improvement in Time-pieces: 21. The revolving Balance of Robert Hook: 22. Diagram, by Mr. Brewster, concerning Achromatic Eye Pieces: 23. Another by Robert Hooke concerning Pendulums.

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MAY, 1806.

ARTICLE I.

Account of a simple and cheap portable Barometer, with Instructions to enable a single Observer to determine Heights by that Instrument with considerable Facility and Precision. In a Letter from SIR H. C. ENGLEFIELD, *Bart. M.P. F.R.S. &c. &c.*

To Mr. NICHOLSON.

SIR,

THE mensuration of heights by the barometer has been, by the labours of Mr. De Luc, Sir George Shuckburg, General Roy, and several other scientific men, brought to such perfection, and affords so much an easier mode of ascertaining the elevations of the different parts of the surface of the earth to a considerable precision, than any other known process, that it might have been supposed that, in the course of thirty years which have elapsed since this branch of science has been perfected, a very great number of observations would have been made, and the heights of almost the whole surface of our own country ascertained by the numerous travellers who continually

Though the mensuration of heights by the barometer has been greatly perfected, yet the observations are few.

ally traverse it. The contrary is however the case, and the small number of observations of this kind may be attributed to several causes. The instruments are of considerable expence, and from their complicated construction easily liable to be out of order in the course of a long journey. The observations themselves, though each not taking up any very long time, yet when multiplied on every hill and valley, as they ought to be, for the purpose of obtaining a just idea of the country surveyed, in the aggregate consume much of the traveller's time, and the constant unpacking and repacking the instrument becomes a greater labour than our natural indolence easily submits to.

Causes. The instruments are costly, liable to be deranged, and the observations demand time and care.

It has moreover been generally supposed, that two instruments and two observers making simultaneous observations at the upper and lower stations of the height to be measured, are indispensably necessary. This of course would put it out of the power of a solitary traveller to make any observations at all. Whether from these or other causes, the fact is, that whoever reads the numerous tours, surveys, and reports of different parts of our island published within these last twenty years, and many of them professedly with a view to science, either of agriculture, mineralogy, or geology, will be perpetually disappointed by meeting with mere guesses at the elevations of the tracts of country described, though a knowledge of those elevations is almost indispensable to the geologist, mineralogist, and military surveyor,—highly useful to the scientific agriculturist, and very interesting to every one who from mere motives of enlarged and enlightened curiosity reads books of travels, or employs his own leisure in traversing the countries described by other voyagers.

Two observers with separate instruments have generally been supposed necessary.

Hence it is that the elevations and depressions of the face of the earth, tho' of high utility and interest, are seldom noted in travels.

I cannot therefore but hope, that by simplifying the barometer, and thereby rendering the instrument much less expensive and its use at the same time more easy, and shewing that very considerable accuracy may be attained by a single observer, this most useful branch of science may be cultivated to so great an extent, that in the course of a few years we may have almost as perfect an idea of the relative heights of the different parts of England as we now have of their horizontal distance.

The impediments to observation proposed to be removed by simplifying the instrument and shewing that one observer may obtain valuable results.

A barometer, nearly similar to that which I am now about to describe,

Portable barometer of Dr. Hugh Hamilton.

describe, was constructed several years since by Dr. Hugh Hamilton, and is by him described at large in the fifth volume of the Transactions of the Irish Academy. I saw the instrument in his hands, nearly sixteen years ago, and was much pleased with its performance. I do not know, however, that any more were then made. I have lately constructed the barometer, whose description I shall now give, which is still more simple than Dr. Hamilton's, and much cheaper; and which, in many trials I have made of it, appears to unite solidity, lightness, and ease of observation to as great a degree as can be wished.

—Simplified by the author. It is light, firm, and easy in the use.

The barometer tube is about 33¼ inches in length; its bore is one-tenth of an inch in diameter, and the external diameter is three-tenths of an inch. This sized bore is fully sufficient to allow the free motion of the mercury. The cistern is of box, turned truly cylindrical, and is an inch in its internal diameter, and an inch in depth. A short stem projects from its top, (the instrument being in the position for making an observation,) for the purpose of giving a firmer hold to the tube. This stem is perforated with an hole sufficiently large to admit the tube, which is glued to it in the usual mode. The tube projects into the cistern exactly to half its depth. The bottom of the cistern is closed by a strong lid of box, which screws on the cistern, and pressing against a leather glued to the inside of the lid renders the whole perfectly impervious to the mercury in every position. The tube being filled and boiled in the common way, and the instrument held inverted in a perpendicular position, mercury is poured into the cistern till it is filled within two-tenths of an inch of the top. The lid is then firmly screwed on, and secured from being opened by idle curiosity by a small screw passing through its side. The essential part of the instrument is now finished. The end of the tube in the cistern can never be uncovered by the mercury in any possible position, and of course no air can ever enter into it: and as the areas of the cistern and tube are as the squares of the diameters, the diameter of the bore of the tube being ,1, its external diameter ,3, and the diameter of the cistern 1,0, the area of the cistern is 100 — 9 = 91; and there being two-tenths of an inch left empty in the cistern, the mercury must fall 182-tenths, or 18 inches and 2-tenths, before the cistern is quite full: a space adequate to the measure of greater heights than any

Description. The barometer tube is glued into a box cylinder, the bore of which exceeds that of the tube as 100 to 9 in area. The tube reaches to the middle of the length of the cylinder, the other end of which cylinder is closed with a screw-cap, and leather. When the tube is filled, mercury is also poured into the cylinder, so as to occupy three-fourths its length, and therefore covers the end of the tube in all positions. When this barometer is set upright the atmosphere acts thro' the pores of the wood.

any known mountain on the earth, much more so to any height in this country. It will not easily be believed by those who have not seen it, that the air will act on a cistern thus completely closed, and of which the wood in its thinnest part is above a quarter of an inch in thickness: but the fact is, that even when the pores of the box-wood are closed by thick varnish, except in that part which touches the mahogany tube, in order to prevent the wood from being affected by damps, the mercury on turning up the barometer takes its level almost instantaneously, certainly in less than half a minute; and that when the instrument is suspended by the side of the best mountain barometer of Ramsden's, constructed with an open cistern, no difference whatever can be perceived in their sensibility to the variations of the atmosphere. It is obvious that the variations of altitude in this instrument of dimensions above stated, will be one ninety-first part less than in a barometer furnished with an apparatus for bringing the surface of the mercury in the cistern to a fixed level: this defect might be remedied by dividing the scale accordingly; but it is much more convenient to divide the scale to real inches, and make the necessary allowance in the result.

The barometer is mounted in a mahogany tube of the size of a walking-stick.

The tube and cistern being thus prepared, are mounted in a mahogany tube of the size of a common walking-stick. The stem of the cistern goes tight into the mahogany tube, and is there secured by two small brass screws, or the stem may be on the outside cut into a male screw, and so be screwed into the mahogany tube. The cistern forms an head or pummel to the staff when the instrument is inverted for the purpose of being carried in the hand or a carriage. The tube is secured in the mahogany case by passing through perforated corks in the usual way.

Means and method of observing the height of the mercury.

For the observation of the height of the mercury, two opposite slits are cut in the mahogany tube, reaching from about 32 to 20 inches, for the longer scales, or from 32 to 25 inches for such as are intended for use in this country. The front slit has its sides bevelled, and is exteriorly about three-fourths of an inch wide. On one side is fixed a brass plate, divided as usual to inches, tenths, and twentieths. On this plate a nonius slides moveable by a small knob, which reads off as in other barometers, to the 500th of an inch. To this nonius a small portion

portion of brass tube is attached, which embraces the barometer tube, and its lower edge is, in observation, made a tangent to the convex surface of the mercury, as in other well-constructed barometers, and the very narrow slit behind gives abundant light for observation.

On the bevelled side of the front slit opposite the scale, a thermometer is placed for taking the heat of the instrument, and there is room for the scale of correction placed on Ramsden's attached thermometers as well as Fahrenheit's scale.

Attached and detached thermometers; facilities in the construction, &c.

A thin brass tube with slits in it turns half round on two pins in the usual manner, and covers the apertures above described in the mahogany tube when the barometer is not in use. The mahogany tube is made rather tapering, and with a ferril at the end opposite the cistern. This ferril unscrews and shews a steel ring, by which the barometer may be suspended when convenient; and as the mahogany tube is made nearly thirty-eight inches long, there is full space above the top of the barometer tube to put in a thermometer, which is taken out by unscrewing the ferril, and is to be used as a detached thermometer in observation. Along the mahogany tube is a scale of three feet, carefully divided to inches, the feet being accurately laid down by small dots on the heads of brass pins sunk into the wood. A scale of this kind is always convenient, and may often be of great use.

Having thus described the instrument, a few practical remarks on the manner of using it may not be superfluous.

Practical remarks.

When I am about to make an observation, about five minutes before I arrive at the place I take out the detached thermometer from its place in the end of the mahogany tube, holding it by the upper end at nearly arm's-length from my body and, if the sun shines, in the shade of my person: it very soon takes the temperature of the air, and is not sensibly affected by the heat of the hand. The heat being observed and written down, the barometer is turned up, the brass tube half turned, and the instrument held between the finger and thumb of the left hand above the slide, so as to let it hang freely in a perpendicular position. Few persons, if any, have sufficient steadiness of hand to prevent little vibrations in the mercury in this position: the hand therefore should be either rested against

To determine the temperature of the air.

Observation by the barometer.

against any fixed body, or if no such occurs, by kneeling on one knee; the cistern should be let down so as to touch the ground, the left hand holding the barometer in a vertical position, which a little practice will render very easy. The index must then be moved by the knob till its under surface, as before stated, is tangent to the mercury. A few light taps should be given to the tube to ascertain that the mercury is fallen as low as it can. The height being then read off and registered together with that of the attached thermometer, the brass tube is turned back, so as to cover the slits; the instrument gently inverted; and the whole is finished: all this may be done in two minutes.

Deduction of the heights to be made as usual by the logarithmic tables,

or by other methods.

The most convenient mode for deducing the heights from the barometrical observations, is certainly by the common logarithmic tables; and it is unnecessary here to detail the method, which may be found in numerous books. It is, however, necessary for this method, to carry the tables of logarithms, which is sometimes inconvenient. The engraved table, formed by Mr. Ramsden, is on a single narrow sheet, and extremely portable, besides being very easy in its use; but it may be lost or mislaid when wanted. Several ingenious formulæ have been devised, which may either be engraven on the instrument itself, or committed to memory. Of the former, Sir G. Shuckburg has given a very concise one in his second paper on the measurement of heights by the barometer, in the sixty-eighth vol. of the Philosophical Transactions; and Mr. Professor Leslie has invented a very elegant one of the latter sort: but these, though very simple in form, require a considerable number of figures in the operation, and are on that account inconvenient. For the purpose therefore of computing on the spot, and very near to the truth, any observations made on a journey, and that almost without the necessity of writing at all, I have caused the following short table to be engraven on the scale of the barometer. It expresses the value of the difference of a tenth of an inch in the height of the mercury, at the temperature of freezing, in English feet.

Computations may be made on a journey very near the truth, from a table here given, which is engraven on the barometer.

TABLE

TABLE.

Inch	10th	Feet	Inch	10th	Feet	Inch	10th	Feet
20	. 05	130	23	. 05	113	27	. 15	96
	. 20	129		. 25	112		. 45	95
	. 35	128		. 45	111		. 75	94
	. 50	127		. 65	110	28	. 05	93
	. 66	126		. 87	109		. 35	92
	. 82	125	24	. 10	108		. 65	91
21	. 00	124		. 32	107		. 95	90
	. 18	123		. 55	106	29	. 27	89
	. 35	122		. 80	105		. 61	88
	. 53	121	25	. 05	104		. 95	87
	. 70	120		. 30	103	30	. 30	86
	. 87	119		. 55	102		. 65	85
22	. 05	118		. 80	101	31	. 00	84
	. 25	117	26	. 05	100		. 37	83
	. 45	116		. 30	99		. 75	82
	. 65	115		. 57	98	32	. 10	81
	. 85	114		. 85	97			

Method of using the table.

The method of using it is as follows: 1st. Add the two observed heights of the barometer, and halve the sum to obtain the mean height. 2d. Subtract the lesser height from the greater, the remainder is, of course, the difference in tenths, &c. of an inch. 3d. Enter the table with the mean height, and take out the feet answering to it, making a proportion if the mean height does not exactly answer to a foot. (This proportion may be made by head.) Multiply the number thus obtained by the tenths, &c. of an inch of difference of height. The result will be nearly the number of feet answering to the difference of height between the two barometers at the temperature of freezing. When the lower barometer stands between 29 and 30 inches, and the elevation does not exceed 1500 feet, this rule will give the height within one foot of the result from the logarithmic method. When the elevation is about 3000 feet, the error will be nearly three feet, and at heights greater than 3000 feet the error increases in an higher ratio. It is always in defect. In this country, however, such elevations do not exist, and in those parts where a knowledge of the comparative heights of the different hills is the most generally useful, they seldom exceed 1000 feet. At all events such observations

Its degree of accuracy.

servations as relate to great elevations may be always recomputed by more rigorous methods at leisure.

Correction for the difference of temperature above the freezing point.

The correction of the heights thus obtained, for the temperature of the air above freezing, is, by Sir G. Shuckburg, supposed to be as the height of the thermometer, and to be 2,44 thousandths of the approximate height for each degree of Fahrenheit; additive when the temperature is above freezing, and subtractive when below freezing. General Roy's observations and experiments lead to a supposition that the correction is not exactly as the height of the thermometer, and that at about the temperature of 50° it amounts to 2.5 thousandths, and is less both much above and much below that temperature. For the purpose of immediate computation, I take the correction at 2,5; which, though certainly too great, will in general be productive of very small error, and affords a rule which is easily remembered, and quickly applied. It is this. For every four degrees that the mean temperature of the two detached thermometers exceeds 32°, add one hundredth of the approximate height, as before obtained, to it; for every 40° one-tenth; and so for any greater or lesser number of degrees. I have not hitherto mentioned the correction which, in fact, ought to be the first in order, viz. that, for the difference of temperature of the two barometers themselves: but this correction is, in general, so small as to be safely neglected, and is besides easily to be made from Mr. Ramsden's numbers, which are engraven on the scale of the attached thermometer. It may not be improper to give an example or two of the method already detailed.

Very simple rule for this purpose.

Examples to illustrate the computations, and shew their accuracy.

Observation at bottom,		29.400	Therm. in air,	45
Observation at top,		25.200	Therm. in air,	41
	2	54.600	2	86
Mean,	—	27.300	Mean heat,	43
			Standard,	32
			Difference,	11

Difference,

Difference,	42	tenths.
Value of a tenth by table,	95.5	feet.
	1910	
	3820	
Approximate height,	4011.0	feet.
Do. by Sir G. Shuckburg,	4016.0	
Error, —	5	feet.

CORRECTION FOR TEMPERATURE.

For 8° = 2 hundredths,	80	feet.
For 3° = 3 four hundredths,	30	
Correction, +	110	
Do. by Sir G. Shuckburg,	107.4	
Error, +	2.6	

Approximate Height	by me.	Sir G. Shuckburg.
	4011	4016
Correction for Temper.	110	107.4
Result, — —	4121	4123.4

EXAMPLE II.

Observation at bottom,	30.017	Therm. in air,	60°
Observation at top,	29.534	Therm. in air,	57
2	59.551	2	117
Mean,	29.775	Mean,	28.5
Difference,	4.83	Standard,	32.
		Difference,	26.5

Value of a tenth by table,	87.5
	350.0
	70.00
	2.625
Approximate Height,	422.625
Do. by Sir G. Shuckburg,	422.9
Error, —	00.3

 COR-

CORRECTION FOR TEMPERATURE.

For 24°	=	6 hundredths,	25 . 3
2	=	2 four hundredth,	2 . 0
½	=	1 eight hundredth,	0 . 5
Correction,		+	27 . 8
Do. by Sir G. Shuckburg,			27 . 2
Error,		+	0 . 6

	By me.		By Sir G. Shuckburg.
Approximate height,	422 . 6		422 . 9
Correction for Temper.	27 . 8	—	27 . 2
	459 . 4		450 . 1

These two examples shew how near the truth the method here recommended will come, even in considerable heights.

Estimate of the probable error which may arise from a difference in the proportion of the bore of the tube to that of the cistern; it is probably less than one thousandth part.

It has been already observed, that in observations made with the barometer I have described, a small correction is necessary, on account of the rise in the mercury in the cisterns as the barometer falls. Altitudes being in all cases measured by the differences of the heights of the mercury at the two stations, and these differences being evidently always too small in this barometer, the correction is obviously always additive. As, in constructing different barometers, the interior and exterior diameters of the tube will not always be exactly similar, though the cisterns may be turned always alike, this error, and of course the correction for it, should be in each instrument, deduced from a comparison with a barometer of known accuracy, at different heights. It will probably vary in different instruments from a ninetieth to a seventieth. Indeed, if it were always taken at an eightieth, in instruments constructed as above directed, the possible error could only amount to about one foot on a thousand; a quantity of very little importance.

Observations with one single barometer pos-

It now remains to say a few words on the necessity of two baro-

barometers for the mensuration of heights, and the probable error to be incurred by using a single one. There is no doubt, that where very great accuracy is required, two barometers ought to be used; but even with every precaution, altitudes cannot be taken by barometers sufficiently near for the purpose of carrying water either by pipes or canals, and for the purpose of the geologist, military surveyor, or agriculturist, it is of very little importance whether a mountain is 1000 or 1010 feet high, though it is of the highest utility that he should know whether it is 800 or 1000. I have during the course of many years been in the habit of taking observations of altitudes by a single barometer, and have had many opportunities of repeating my observations on the same hills when the barometer has been at different heights, and when falling or rising during the time of observation; and more than once I have observed heights which had been trigonomically taken by the best instruments, and I can safely say that the difference between these observations have seldom amounted to so much as two feet on an hundred. The mode I use is this:—At setting out I take the height of the mercury, and note the time of observation. I likewise note the time of the second observation, and on returning to the first station, observe again and note the time. If the barometer has altered in the interval, a simple proportion corrects either of the three observations, and reduces the height to what would have been observed had the mercury been stationary. It is true, that this method supposes the motion of the mercury to have been uniform during the interval of observation, but except in very variable weather, which does not very often occur, particularly in summer, when the greater number of these observations will naturally be made, this supposition may be safely made. It is also true, that a traveller has often no opportunity of making a second observation at the spot he set out from. Even in this case, a near approximation may often be made by observing, for example, at a stream on each side of the hill to be measured. If also he observes the barometer repeatedly in the morning before he sets out, and sees its tendency, and does the same at every halt during the day, he will have data whereon to found a nearly accurate correction. But if all this should be out of his power, even under the most unfavourable cir-

sess considerable accuracy.

Account of the method practised by the author.

Observations are highly useful, even under unfavourable circumstances.

circumstances, barometical observations will give a much more accurate idea of the outline of a country than any we now possess; and it should be ever remembered, that observations accurately made, and faithfully recorded, are valuable. The repeated observations of different travellers, though separately defective, will in most cases correct each other, and from the whole very accurate conclusions may be drawn.

Fabrication and price of these barometers.

I have entered into a greater detail than would be necessary for a great part of your readers, in the hope of being intelligible to those who are less acquainted with the subject, and who may wish to employ any instrument-maker for the construction of barometers similar to that which I have described. In justice to a very ingenious young artist, permit me to add, that I have employed in making those which I have, Mr. Thomas Jones, of No. 120, Mount-street, Berkeley-spuare, pupil of the late Mr. Ramsden, and who will furnish them at the price of two guineas and a half without the attached thermometer, three guineas with it, and three guineas and a half with the attached and detached thermometer. Such barometers, however, as are graduated down to 20 inches, will, on account of the additional work, cost five shillings more.

I am, Sir,
Your humble Servant,
H. C. ENGLEFIELD.

Tilney-street,
April 5, 1806.

Difference of speed in the settling of these instruments: It does not affect their accuracy.

P. S. On comparing several barometers made by Mr. Jones, since this description was written, I find that in some of them the mercury does not take its true height on turning up the instrument, quite so quick as in the two which he first constructed for me. This difference is owing to the greater closeness of fibre in some pieces of box-wood than in others; but it does not affect the accuracy of the instrument. It may not be superfluous to say, that the weight of this barometer is one pound and three quarters; the weight of Ramsden's last improved barometer is four pounds and a half; and that of his earliest form, six pounds and three quarters.

I subjoin a few observations made at the top and bottom of Richmond-Hill, by which the accuracy of this barometer may be fairly estimated.

Dec.

		Bason.	Therm.	Results.		
Dec. 22.	Hill top,	28.710				Observations made on Richmond-hill.
	Thames side,	28.868		Feet.		
		158		— 146	doubtful.	
Jan. 1.	Hill top,	29,540	44			
	Thames side,	29.686				
		146		— 133		
Jan. 2.	Hill top,	29.708	38			
	Thames side,	29.860				
		152		— 134		
Jan. 31.	Hill top,	29.301	36			
	Thames side,	29.453	37			
		152		— 137		
Feb. 23.	Hill top,	29.758	51			
	Thames side,	29.912				
		154		— 139		
Feb. 24.	Hill top,	30.180	53			
	Thames side,	30.334	54			
		154		— 140		

II.

Account of a Series of Experiments, shewing the Effects of Compression in modifying the Action of Heat. By SIR JAMES HALL, *Bart. F. R. S. Edinburgh.*

(*Continued from page* 405, Vol. XIII.)

Experiments in which Water was employed to increase the Elasticity of the included Air.—Cases of complete Compression.—General Observations.—Some Experiments affording interesting Results; in particular, shewing a mutual Action between Silex and the Carbonate of Lime.

FINDING that such benefit arose from the increase of elasticity given to the included air in the last-mentioned experiments,

Contrivance by which a very small quantity of water was

Included, in order to increase the compression by its reaction as steam.

ments, by the diminution of its quantity; it now occured to me, that a suggestion formerly made by Dr. Kennedy, of using water to assist the compressing force, might be followed with advantage: That while sufficient room was allowed for the expansion of the liquid metal, a reacting force of any required amount, might thus be applied to the carbonate. In this view, I adopted the following mode, which, though attended with considerable difficulty in execution, I have often practised with success. The weight of water required to be introduced into the barrel was added to a small piece of chalk or baked clay, previously weighed. The weight of water required to be introduced into the barrel was added to a small piece of chalk or baked clay, previously weighed. The piece was then dropped into a tube of porcelain of about an inch in depth, and covered with pounded chalk, which was firmly rammed upon it. The tube was then placed in the cradle along with the subject of experiment, and the whole was plunged into the fusible metal, previously poured into the barrel, and heated so as merely to render it liquid. The metal being thus suddenly cooled, the tube was encased in a solid mass, before the heat had reached the included moisture. The difficulty was to catch the fusible metal at the proper temperature; for when it was so hot as not to fix in a few seconds, by the contact of the cradle and its contents, the water was heard to bubble through the metal and escape. I overcame this difficulty, however, by first heating the breech of the barrel, (containing a sufficient quantity of fusible metal), almost to redness, and then setting it into a vessel full of water, till the temperature had sunk to the proper pitch, which I knew to be the case when the hissing noise produced in the water by the heated barrel ceased; the cradle, during the last stage of this operation, being held close to the muzzle of the barrel, and ready to be thrust into it.

Successful experiment. Temp. 24°.

On the 2d of May, I made my first experiment in this way, using the same air-tube as in the last experiment, which was equal in capacity to one-thirtieth of a cubic inch. Half a grain of water was introduced in the manner just described. The barrel, after an hour of red-heat, was let down by a rope and pulley, which I took care to use in all experiments, in which there was any appearance of danger. All was sound. The

The metals rushed out smartly, and a flash of flame accompanied the discharge. The upper pyrometer gave 24°, and the lower one 14°. The contents of the inner tube had lost less than 1 *per cent.*, strictly 0.84. The carbonate was in a state of good limestone; but the heat had been too feeble: The lower part of the chalk in the little tube was not agglutinated: The chalk round the fragment of pipe-stalk (used to introduce the water), which had been more heated than the pyrometer, and the small rod, which had moulded itself in the boll of the stalk, were in a state of marble.

Another with more water. Temp. 36°.

On the 4th of May, I made an experiment like the last, but with the addition of 1.05 grains water. After application of heat, the fire was allowed to burn out till the barrel was black. The metal was discharged irregularly. Towards the end, the inflammable air produced, burnt at the muzzle, with a lambent flame, during some time, arising doubtless from hydrogen gas, more or less pure, produced by the decomposition of the water. The upper pyrometer indicated 36°, and the lower one 19°. The chalk which lay in the outer part of the large tube was in a state of marble. The inner tube was united to the outer one, by a star of fused matter, black at the edges, and spreading all round, surrounding one of the fragments of porcelain which had fallen by accident in between the tubes. The inner tube, with the starry matter adhering to it, but without the coated fragment, seemed to have sustained a loss of 12 *per cent.*, on the original carbonate introduced. But, the substance surrounding the fragment being inappreciable, it was impossible to learn what loss had been really sustained. Examining the little tube, I found its edges clean, no boiling over having taken place. The top of the small lump of chalk had sunk much. When the little tube was broken, its contents gave proof of fusion in some parts, and in others, of the nearest approach to it. A strong action of ebullition had taken place all round, at the contact of the tube with the carbonate: in the heart, the substance had a transparent granular texture, with little or no crystallization. The small piece of lump-chalk was united and blended with the rammed powder, so that they could scarcely be distinguished. In the lower part of the carbonate, where the heat must have been weaker, the rod had acted more feebly on the tube, and was detached

from

from it: here the substance was firm, and was highly marked in the fracture with crystalline facettes. Wherever the carbonate touched the tube, the two substances exhibited, in the mixture, much greater proofs of fusion than could be found in the pure carbonate. At one place, a stream of this compound had penetrated a rent in the inner tube, which it had filled completely, constituting a real vein, like those of the mineral kingdom: which is still distinctly to be seen in the specimen. It had then spread itself upon the outside of the inner tube, to the extent of half an inch in diameter, and had enveloped the fragment of porcelain already mentioned. When pieces of the compound were thrown into nitric acid, some effervesced, and some not.

Experiment repeated with more water than before.

I repeated this experiment on the same day, with two grains of water. The furnace being previously hot; I continued the fire during one half-hour with the muffle open, and another with a cover upon it. I then let the barrel down by means of the pulley. The appearance of a large longitudinal rent, made me at first conceive that the experiment was lost, and the barrel destroyed: The barrel was visibly swelled, and in swelling had burst the crust of smooth oxide with which it was surrounded; at the same time, no exudation of metal had happened, and all was sound. The metals were thrown out with more suddenness and violence than in any former experiment, but the rod remained in its place, being secured by a cord. The upper pyrometer gave 27°, the lower 23°. The contents of the inner tube had lost 1.5 *per cent.* The upper end of the little lump of chalk, was rounded and glazed by fusion; and the letter which I have been in the habit of cutting on these small pieces, in order to trace the degree of action upon them, was thus quite obliterated. On the lower end of the same lump, the letter is still visible. Both the lump and the rammed chalk were in a good semitransparent state, shining a little in the fracture, but with no good facettes, and no where appearing to have acted on the tube. The last circumstance is of consequence, since it seems to shew, that this very remarkable action of heat, under compression, was performed without the assistance of the substance of the tube, by which, in many other experiments, a considerable additional fusibility has been communicated to the carbonate.

These

These experiments, and many others made about the same ime, with the same success, clearly prove the efficacy of water in assisting the compression; and results approaching to hose in quality, obtained, in some cases, by means of a very mall air-tube, shew that the influence of water on this occasion has been merely mechanical.

The water increased the pressure, and its effect was merely mechanical.

During the following summer and autumn 1803, I was occupied with a different branch of this subject, which I shall soon have occasion to mention.

In the early part of last year, 1804, I again resumed the ut of experiment lately described, having in view principally to accomplish absolute compression, in complete imitation of the natural process. In this pursuit, I did not confine myself to water, but made use of various other volatile substances, in order to assist compression; namely, carbonate of ammonia, nitrate of ammonia, gunpowder, and paper impregnated with nitre. With these I obtained some good results, but none such as to induce me to prefer any of these compressors to water. Indeed, I am convinced, that water is superior to them all. I found, in several experiments, made with a simple air-tube, without any artificial compressor, in which a very low red-heat had been applied, that the carbonate lost one or one and a half *per cent.* Now, as this must have happened in a temperature scarcely capable of inflaming gunpowder, it is clear, that such loss would not have been prevented by its presence: whereas water, beginning far below redness to assume a gaseous form, will effectually resist any calcination, in low as well as in high heats. And as the quantity of water can very easily be regulated by weight, its employment for this purpose seems liable o no objection.

Other volatile bodies used for compression; but water is the best.

On the 2d of January 1804, I made an experiment with marble and chalk, with the addition of 1.1 grain of water. I imed at a low heat, and the pyrometer, though a little broen, seemed clearly to indicate 22°. Unluckily, the muzzle f the large tube, which was closed as usual with chalk, was laced uppermost, and exposed to the strongest heat. I found rounded by fusion, and in a frothy state. The little tube ame out very clean, and was so nearly of the same weight as hen put in, that its contents had lost but 0.074 *per cent.* of e weight of the original carbonate. The marble was but

Experiment in which under the same pressure chalk was fused by an high heat, and another part converted into limestone at a lower.

feebly

feebly agglutinated, but the chalk was in a state of firm limestone, though it must have undergone a heat under 22°, or that of melting silver. This experiment is certainly a most remarkable one, since a heat has been applied, in which the chalk has been changed to a hard limestone, with a loss less than the 1000dth part of its weight, (exactly $\frac{1}{1151}$); while, under the same circumstances of pressure, though probably with more heat, some of the same substance had been brought to fusion. What loss of weight this fused part sustained, cannot be known.

Similar experiments.

On the 4th of January, a similar experiment was made, likewise with 1.1 grain of water. The discharge of the metal was accompanied with a flash of flame. The pyrometer indicated 26°. The little tube came out quite clean. Its contents had been reduced from 14.53 to 14.46, difference 0.07 grains, being 0.47 *per cent.* on the original carbonate, less than one two-hundredth part of the original weight, (exactly $\frac{1}{212}$). The chalk was in a state of firm saline marble, but with no unusual qualities.

The carbonate should be dried.

These two last experiments are rendered still more interesting, by another set which I made soon after, which shewed, that one essential precaution in a point of such nicety had been neglected, in not previously drying the carbonate. In several trials made in the latter end of the same month, I found, that chalk exposed to a heat above that of boiling water, but quite short of redness, lost 0.34 *per cent.*; and in another similar trial, 0.46 *per cent.* Now, this loss of weight equals within 0.01 *per cent.* the loss in the last-mentioned experiment, that being 0.47; and far surpasses that of the last but one, which was but 0.074. There is good reason, therefore, to believe, that had the carbonate, in these two last experiments, been previously dried, it would have been found during compression to have undergone no loss.

Explanation of some apparent anomalies.

The result of many of the experiments lately mentioned, seems fully to explain the perplexing discordance between my experiments with porcelain tubes, and those made in barrels of iron. With the porcelain tubes, I never could succeed in a heat above 28°, or even quite up to it; yet the results were often excellent. Whereas, the iron-barrels have currently stood firm in heats of 41° or 51°, and have reached even to 70°

or

or 80° without injury. At the same time, the results, even in those high heats, were often inferior, in point of fusion, to those obtained by low heats in porcelain. The reason of this now plainly appears. In the iron-barrels it has always been considered as necessary to use an air-tube, in consequence of which, some of the carbonic acid has been separated from the earthy basis by internal calcination: what carbonic acid remained, has been more forcibly attracted, according to Mr. Berthollet's principle, and, of course, more easily compressed, than when of quantity sufficient to saturate the lime: but, owing to the diminished quantity of the acid, the compound has become less fusible than in the natural state, and, of course, has undergone a higher heat with less effect. The introduction of water, by furnishing a reacting force, has produced a state of things similar to that in the porcelain tubes; the carbonate sustaining little or no loss of weight, and the compound retaining its fusibility in low heats*.

In the early part of 1804, some experiments were made with barrels, which I wished to try, with a view to another series of experiments. The results were too interesting to be passed over; for, though the carbonic acid in them was far from being completely constrained, they afforded some of the finest examples I had obtained, of the fusion of the carbonate, and of its union with silex.

Experiment. Fusion of a portion of carbonate (from shell) and mutual attraction of the carbonate and silex of the tube.

On the 13th of February, an experiment was made with pounded oyster-shell, in a heat of 33°, without any water being introduced to assist compression. The loss was apparently of 12 *per cent*. The substance of the shell had evidently been in viscid fusion: it was porous, semitransparent, shining in surface and fracture; in most parts with the gloss of fusion, in

* The retentive power here ascribed to the porcelain tubes, seems not to accord with what was formerly mentioned, of the carbonic acid having been driven through the substance of the tube. But the loss by this means has probably been so small, that the native properties of the carbonate have not been sensibly changed. Or, perhaps, this penetrability may not be so universal as I have been induced to think, by having met with it in all the cases which I tried. In this doubt, I strenuously recommend a further examination of this subject to gentlemen who have easy access to such porcelains as that of Dresden or of Seve.

 many

many others with facettes of crystallization. The little tube had been set with its muzzle upwards; over it, as usual, lay a fragment of porcelain, and on that a round mass of chalk. At the contact of the porcelain and the chalk, they had run together, and the chalk had been evidently in a very soft state; for, resting with its weight on the porcelain, this last had been pressed into the substance of the chalk, deeper than its own breadth, a rim of chalk being visible without the surface of the porcelain; just as when the round end of a knife is pressed upon a piece of soft butter. The carbonate had spread very much on the inside of the tube, and had risen round its lip, as some salts rise from their solution in water. In this manner, a small quantity of the carbonate had reached the outer tube, and had adhered to it. The black colour frequently mentioned as accompanying the union of the carbonates with the porcelain, is here very remarkable.

Similar experiment.

On the 26th of February, I made an experiment, in which the carbonate was not weighed, and no foreign substance was introduced to assist the compression. The temperature was 46°. The pyrometer had been affected by the contact of a piece of chalk, with which it had united; and some of the carbonate must have penetrated the substance of the pyrometer, since this last had visibly yielded to pressure, as appeared by a swelling near the contact. I observed in these experiments, that the carbonate had a powerful action on the tubes of Cornish clay, more than on the pounded silex. Perhaps it has a peculiar affinity for argil, and this may lead to important consequences. The chalk had visibly first shrunk upon itself, so as to be detached from the sides, and had then begun to run by successive portions, so as still to leave a pillar in the middle, very irregularly worn away; indicating a successive liquefaction, like that of ice, not the yielding of a mass softening all at once.

Similar experiment. Detail of appearances.

On the 28th of February, I made an experiment with oyster-shell unweighed, finely ground, and passed through the closest sieves. The pyrometer gave 46°. The piece of chalk below it had been so soft, as to sink to the depth of half an inch into the mouth of the iron air-tube, taking its impression completely. A small part of this lump was contaminated with iron, but the rest was in a fine state. The tube had a rent in it,

it, through which the carbonate, united with the matter of the tube, had flowed in two or three places. The shell had shrunk upon itself, so as to stand detached from the sides, and bore very strong marks of fusion. The external surface was quite smooth, and shining like an enamel. The internal part consisted of a mixture of large bubbles and solid parts: the inside of the bubbles had a lustre much superior to that of the outside, and equal to that of glass. The general mass was semitransparent; but small parts were visible by the lens, which were completely transparent and colourless. In several places this smooth surface had crystallized, so as to present brilliant facettes, steadily shining in certain aspects. I observed one of these facettes on the inside of an air-bubble, in which it interrupted the spherical form as if the little sphere had been pressed inwards at that spot, by the contact of a plane surface. In some chalk near the mouth of the large tube, which lay upon a stratum of silex, another very interesting circumstance occured. Connected with its lower end, a substance was visible, which had undoubtedly resulted from the union of the carbonate with the silex. This substance was white and semitransparent, and bore the appearance of chalcedony. The mass of chalk having attached itself to that above it, had shrunk upwards, leaving an interval between it and the silex, and carrying some of the compound up with it. From thence this last had been in the act of dropping in a viscid state of fusion, as evidently appeared when the specimen was entire; having a stalactite and stalagmite corresponding accurately to each other. Unluckily I broke off the stalactite, but the stalagmite continues entire, in the form of a little cone. This new substance effervesced in acid, but not briskly. I watched its entire solution; a set of light clouds remained undissolved, and probably some jelly was formed; for I observed, that a series of air-bubbles remained in the form of the fragment, and moved together without any visible connection; thus seeming to indicate a chemical union between the silex and the carbonate The shell, fused in the experiment, dissolved entirely in the acid, with violent effervescence.

In the three last experiments, and in several others made at the same time, the carbonate had not been weighed; but no water being introduced to assist the compression, it is probable there was much loss by internal calcination; and owing doubtless

doubtless to this, the carbonates have crumbled almost entirely to dust, while the compounds which they had formed with silex remain entire.

Similar experiment. Resemblance to Cipolline marble.

On the 13th of March, I made a similar experiment, in which, besides some pounded oyster-shell, I introduced a mixture of chalk, with 10 *per cent.* of silex intermixed, and ground together in a mortar with water, in a state of cream, and then well dried. The contents of the tube when opened, were discharged with such violence, that the tube was broken to pieces; but I found a lump of chalk, then in a state of white marble, welded to the compound; which last, in its fracture, shewed that irregular black colour, interspersed roughly through a crystalline mass, that belongs to the alpine marbles, particularly to the kind called at Rome *Cipolline.* It was very hard and firm; I think unusually so. It effervesced constantly to the last atom, in diluted nitric acid, but much more sluggishly than the marble made of pure chalk. A cloudiness appeared pervading all the liquid. When the effervescence was over, a series of bubbles continued during the whole day in the acid, without any disposition to burst, or rise to the surface. After standing all next day and night, they maintained their station; and the solution being stirred, was found to be entirely agglutinated into a transparent jelly, breaking with sharp angles. This experiment affords a direct and positive proof of a chemical union having taken place between the carbonate and silex.

(To be continued.)

III.

Observations on the Dispersion of the Light of Lamps by Means of Shades of unpolished Glass, Silk, &c.; with a Description of a new Lamp. By BENJAMIN COUNT OF RUMFORD. *Read at the National Institute of France, March* 20, 1806.

To Mr. NICHOLSON.

SIR,

I SEND you for your Journal, the following translation of a very ingenious paper of Count Rumford's, which was read at

the Institute on Monday last. Count Rumford, at whose desire I send it, has taken the trouble to revise and correct the translation.

I am,

Paris, Your very humble servant,

27th March, 1806. W. A. Cadell.

No. 17, *Rue de Varenne.*

Amongst the necessaries of life may be reckoned *heat* and *light*; and each of them composes so considerable an article of expence, that every improvement that tends to facilitate their production, or to economise their consumption, is deserving of attention. Introduction.

Having made, at different periods, a great number of experiments on the production of light in the combustion of inflammable bodies, and on its distribution; and having lately contrived a lamp which, on trial, has been found to answer very well, I have resolved to submit to this learned assembly some of the results of my researches on this important subject. An improved lamp,

The lamp, which I have the honour of presenting to the Institute, has nothing new in the essential part of it, that is, in the form of the wick; for, after the ingenious discovery of the circular wick by M. Argand, it does not appear to me probable that the economy of oil in the production of light can be carried much farther: When this lamp is in good order it gives no perceptible smoke, nor smell; and hence, I think, we may conclude that the combustion of the oil is complete, and, consequently, that the quantity of light is at its maximum. But there still remains much to be done to improve the general form of lamps, in regard to their elegance and convenience, and above all, to distribute their light in a more advantageous and agreeable manner. —not essentially new.

If the facility with which the eye distinguishes objects depended solely on the intensity of the light by which they are illuminated, the scientific distribution of light would be less important; but that is far from being the case. We are able to see, very distinctly, with intensities of light which are extremely diffierent; provided that the eye has had the time to conform itself to the quantity of light present, and that that quantity remains invariable. Vision is well performed by light, at very different intensities.

It

It is well known that we can read printed characters of a moderate size, both by the light of the full moon, and by that of the sun at mid-day; the intensity, however, of the light, in the first case, is, to that in the second, as 1 to 300,000; but when the eye passes rapidly from a strong light, to one that is more feeble, or vice versa, we can distinguish nothing at first; and, when these changes succeed each other rapidly, they become extremely fatiguing to the eyes.

Distinctness depends very much on the simplicity of the shadows of bodies.

The facility with which we distinguish an enlightened object depends on its shadow. When shadows are simple, they are necessarily well defined, and we see distinctly; but when the light comes in several directions, several shadows are formed of the same object, which are so blended together as to render them confused and ill defined; and in that case we see indistinctly, even in the midst of a great glare of light. Hence we may conclude, that a considerable economy must result from a judicious distribution of the light employed in lighting a room. But this saving of expence, considerable as it would be, is however an object of much less importance than the advantage which must result in respect to the pleasantness of the light, and the preservation of the eyes.

The hurtful effects of the direct light of lamps upon the eye.

If every sudden change in the intensity of the light that falls upon the eyes be hurtful to them, the direct rays coming from the flame of an Argand lamp must fatigue them very much, and even deprive them of the faculty of distinguishing easily objects which are placed near that dazzling source of brightness. It is impossible indeed to view the flame of one of these lamps near at hand without suffering excessive pain, and even at a distance it is always hurtful and unpleasant to the eye. It is well known how much we are dazzled and almost blinded on coming into a room lighted by several of these lamps burning without shades, and placed so low that the eye cannot avoid them.

—remedied by semitransparent shade

With a view to soften the light of these lamps, shades have been contrived, formed of materials whose transparency is more or less imperfect; for instance, large cylinders, or spheres of crape, gauze, or roughened glass. This contrivance is very useful, and deserves to be more generally adopted; it is even of so great importance that we cannot take too much pains to improve it, and recommend it to the public.

Wi

What has hindered these shades from being more generally employed is probably an opinion, that they must necessarily occasion a great loss of light. I hope to be able to shew that that opinion is not well founded.

The use of shades has been limited from a notion that they darken;

The following simple experiment was made some years ago, with a view to determine nearly the quantity of light which is lost in passing through a roughened glass.

Two wax candles, of equal size, lighted, and burning with the same degree of intensity, were placed in two vertical cylinders of fine glass, pretty thin, six inches in diameter and six inches in height, the one of smooth and the other of roughened glass; these two cylinders being placed at the same height, on two tables, at the distance of eight feet from each other, in a room where there was no other light than that emitted by the candles, I presented to the two candles, placed in their cylinders, a sheet of white paper, at the distance of sixteen feet from each of them, and I interposed before the paper, at the distance of two inches from its surface, a small wooden cylinder in a vertical position, which projected two shadows on the paper.

but experiment shews that roughened glass intercepts no more light than clear glass.

I was much surprised to find these shadows very nearly of the same intensity. This result shewed me that the quantity of light lost in passing through a roughened glass is much less than I at first supposed; but, on reflection, I saw that there was nothing in the result of the experiment which did not admit of an easy explanation.

The cause is manifest;

Although roughened glass appears opaque, it is by no means so. In the operation of roughening its surface, which from being smooth becomes furrowed, and broken in every direction, it at last presents an uninterrupted collection of asperities of every different form. Individually they are almost invisible to the naked eye, on account of their smallness; their sides are however smooth and shining, as is easy to perceive on examining them with a microscope. It is evident that the light which falls upon the smooth surface of one of these little prominent points must penetrate the glass with the same ease (when the angle of incidence is the same) as it would penetrate the plane surface of a large polished plate of the same sort of glass; and that having passed through the surface, the ray must pursue its course in the substance of the glass, and pass out on the other side in the same manner in one case as in the other.

for the minute cavities of rough glass are polished.

These small surfaces only disperse the rays.

When a pencil of paralell rays falls perpendicularly upon a plate of well-polished glass, they pass through the glass without any perceptible change of direction; but when the pencil falls upon a plate of roughened glass, the rays of which it is composed are dispersed, and the cylindrical pencil is transformed into a cone. The ultimate course of each ray depends on the refractions that it has undergone in entering and issuing out of the glass, and these refractions are determined by the angles of incidence, and the respective inclinations of the refracting surfaces on each side of the glass at the point where the ray enters and at that where it goes out.

A clear glass globe surrounding a lamp is scarcely seen; if the glass be roughened it emits light from every part.

If the flame of a lamp be placed in the center of a globe of fine glass, well polished, the rays issuing from it will traverse the sides of the globe without undergoing any perceptible change, either in their intensity or their direction; and the flame will be seen so distinctly through the globe that this last might even escape observation. But if, instead of a globe of polished glass we employ a globe of roughened glass, in that case, the rays emitted by the flame will be dispersed by the glass in such a manner that each visible point of the surface of the globe will become a radiant cone, and consequently the globe will appear luminous, diffusing light from its surface in every direction.

The light is thus softened and very little lost,

From this explanation of the phenomena we see that a shade of fine glass roughened, when it is used with a view to disperse and soften the too vivid light of a lamp, does not occasion any considerable loss of light. This loss would even be imperceptible, or not greater with a shade of roughened glass than with one of the same kind of glass polished and transparent, notwithstanding the great dispersion of the light, were it not for the reflections which some of the rays suffer before they quit the shade.

by internal reflections.

It is well known that when a ray of light falls upon a polished surface of glass (or other substance), at a very small angle of incidence, it is necessarily reflected; and as the sides of the asperities of the roughened glass must present themselves to the rays which proceed from the lamp at angles of all possible magnitudes, there must necessarily be some whose inclination is sufficient to reflect some of the rays that fall upon them; and as that may occur at both surfaces of the shade, it

is

is possible that a ray may be obliged to pass and repass in the glass from one side of the shade to the other several times before it be able to escape into the room.

If the glass were perfectly transparent, the light would be little, or perhaps not at all diminished by these repeated reflections and passages; but we know that even the finest glass is very far from being perfectly transparent.

When crape, gauze, or other substances are employed to make shades for the purpose of masking the flame of a lamp, the loss of light will be more or less considerable in proportion to the greater or less degree of transparency of the solid parts of the substance employed. But without engaging in the very delicate enquiry concerning the degree of transparence of the molecules or small solid particles of the substances to be employed in making shades, we may determine, by simple experiments, with ease and even precision, what are the substances to be preferred for that purpose. We have only to procure shades of the same form and size, made of the different substances to be examined, and to compare them, by pairs, by means of two argand lamps, made to burn with the same degree of vivacity, and of a simple photometer, which can be constructed at a very small expence.

Shades of crape gauze, &c. will intercept light in proportion to their opacity

The photometer which I used in my experiments on the comparative quantities of light produced in the combustion of wax, tallow, and different kinds of oil, and of the same kind of oil burned in an argand lamp and in a common lamp,* would serve perfectly well for the experiments in question; but as that instrument is somewhat complicated, I shall propose another more simple, which I have employed since with success. Its construction is as follows:—

The loss may be ascertained by the photometer,

In the middle of the upper surface of a wooden cube of 8 inches in diameter, composed of boards, covered with black paper, there is fixed vertically a small board, 4 inches in breadth, 6 inches in height, and half an inch in thickness, covered on one side with white paper. In the middle of this white side there is traced, with pen and ink, a slender black line, from the top to the bottom, which divides the surface into two equal parts.

or by a simpler instrument here described.

* See Phil. Transact. for 1794, and my Philosophical Papers, Vol. i. page 270.

Before this white surface, at the distance of $2\frac{1}{10}$ inches, are placed two little pillars of wood, painted black, 4 inches in height and half an inch in diameter. These little pillars, which are cylindrical, are placed at the distance of $3\frac{2}{10}$ inches asunder; and they are firmly fixed in two holes formed to receive them in the upper surface of the cube. They are at equal distances from the black vertical line which marks the middle of the white surface of the photometer, that is to say, at the distance of 3 inches (English measure) from that vertical line.

ethod of employing that apparatus.

This little instrument is employed in the following manner. Having placed, in a dark room, three little tables, at the distance of 7 or 8 feet from each other, so as to occupy the three angles of an equilateral triangle, the photometer is placed on one of these tables, and the two lamps upon the other two; taking care that the flames of the lamps and the middle of the white surface of the photometer are of the same height, or in the same horizontal plane.

The observer being seated before the photometer, with his back turned towards the lamps, he presents the photometer to the two lamps, in such a manner that the direct rays from their flames fall upon the white surface of that instrument at equal angles of incidence, or in such a direction that the two internal shadows formed by the two pillars may touch each other without being blended together, at the black vertical line in the middle of that face. As the two external shadows fall without the surface of the photometer, they are of course not seen.

When the photometer is placed, the distances of the lamps are verified, and brought to a perfect equality, and when that is done, the lamps are made to burn with the same degree of vivacity, which is easily done by elevating a little one of the wicks, or lowering the other: this must be performed by an assistant, whilst the observer keeps his eyes constantly fixed upon the shadows.

The equality of the quantities of light which the lamps emit is announced by the perfect equality in the densities of the two shadows which are formed in the middle of the white face of the photometer. This is evident, because each shadow being enlightened by the direct rays of the opposite lamp, if one of the lamps gives more light than the other, the shadow which it enlightens must of course be more enlightened, and consequently

sequently less dark than that enlightened by the weaker lamp.

If, instead of establishing an equality in the quantity of light emitted by the two lamps, we would ascertain the relative quantities of light that they emit when their flames are unequal, they must be placed on two tables before the photometer, and after having brought the shadows into contact with each other, we must remove the stronger lamp until the intensity of its light at the vertical field of the photometer be diminished by the increase of its distance, till a perfect equality is established between the densities of the two shadows; and then we measure exactly the distance of each lamp from the photometer.— The squares of the distances will be as the quantities of light emitted by the lamps.

In order to exclude the light reflected from the sides of the room and other surrounding bodies, with a view to render the shadows more distinct, and their comparison more easy, the photometer may be placed in a quadrangular box, open in the front, forming a kind of centry-box, 15 or 16 inches in height and 10 or 12 inches in width and depth, constructed of boards, or even of pasteboard, and lined within and without with black paper.

Valuable results may be had by experiments upon shades.

The experiments are simple and easy that may be made with this little apparatus for determining the different substances that may be employed in constructing shades to soften the light of lamps; and as this enquiry must lead to very important improvements, both with respect to economy and to elegance and comfort, I recommend these researches to all those who are occupied in the improvement of lamps. These experiments may be made in the following way:—

Instructions for making such experiments.

Having prepared two shades, of the same form and dimensions, that are to be compared together, we must begin by placing the two lamps at the same distance in front of the photometer, and causing them to burn with the same degree of brightness, and then masking the flames of the lamps by the two shades, we must examine anew the shadows. If these shadows are of equal densities, we may conclude that the two shades emit equal quantities of light; if the densities of the shadows are different, then the shade that enlightens the shadow which is the least dense is that which emits the most light; and

and in order to determine with precision the relative quantities of light emitted by the two shades, we must remove the lamp which bears the shade that emits the most light until the equality of the shadows be re-established, and then, measuring the distances of the lamps from the photometer, the quantities of light will be as the squares of these distances.

If we wish to know how much light is absorbed and lost in employing any given shade, we must operate in the following manner:—Having placed the two lamps, without their shades, at equal distances in front of the photometer, and having equalised the flames of the lamps in the manner already described, we place the shade that is to be tried upon one of the lamps, and the equality of the shadows is instantly destroyed. In order to re-establish this equality, we remove the lamp that is without a shade, and when it re-established we measure the distances of the two lamps from the photometer. The quantity of light emitted by one of these lamps without a shade is to that emitted by the same lamp with the shade, as the square of the distance of the lamp that burns without a shade is to the square of the distance of that masked by the shade.

The power of a shade to soften and disperse light, and that of intercepting light, are different qualities.

The object in view in using a shade being to disperse the rays of a too dazzling flame, without destroying them, it is evident that the less the flame of a lamp is apparent through the sides of a shade, the quantity of light emitted being the same, the better it will answer its purpose. But as we always see, more or less distinctly, the brilliant flame of an argand's lamp through the shade which masks it, it is evident that a considerable part of the light emitted by a lamp thus masked does not proceed from the shade, but, passing directly through the sides of the shade, it comes from the flame in straight lines.

It is light, coming from the flame to the eyes in straight lines, which a shade is destined to disperse and to soften; and as it is certain that two shades of different materials may have an equal power in softening the direct rays of the flame of a lamp, and that nevertheless the total quantities of light that they emit may be very different, it is necessary to pay attention to this remarkable circumstance in the choice of shades.

The shades to be compared should therefore be examined, first in regard to their power of masking and softening the direct rays of the flame of a lamp, and afterwards in regard to the

the quantity of light that they distribute in a room. The first point seems susceptible of being pretty well determined by the simple inspection; but if we would employ more precision, we may make use of the following method:—

Having placed, at equal distances, two lamps burning with the same degree of vivacity, in front of the photometer, we mask them with the two shades (made of different materials), which are to be compared, and we place between each shade and the photometer, at the distance of about an inch from the shade, a disk of thick pasteboard, perforated in the middle by a circular hole, one inch in diameter. The diameter of this disk must be sufficiently large to mask completely the shade, and the center of the circular hole must be in a straight line drawn from the centre of the flame to the center of the vertical face of the photometer. Instructions for ascertaining the power of dispersing or softening the light,

We see that, in this state of things, it is almost solely the direct rays coming from the flames of the lamps in straight lines through the substance of the shades that fall upon the photometer; and that, on measuring the relative intensities of these rays on each side, by means of the shadows and the distances, we can determine not only which of the shades fulfils best its principal object, that of preserving the eyes, but likewise the proportion in which one of the flames is more softened than the other. We may also determine, by easy experiments and calculations, the proportion which exists, in any given case, between the quantity of light that passes directly from the flame in straight lines through the sides of the shade, and that which is dispersed by the shade and appears to issue from the shade itself. It would be too long to describe in this place all these experiments, and several others that might be made with the photometer, to complete the researches necessary for the improvement of lamps; but these details are the less necessary, as the experiments will not fail to present themselves to those who shall have made some progress in these enquiries. and of generally illuminating.

I shall finish my observations upon shades for lamps by some remarks on the size that may be given to them. And first, it is evident that the diameter of a shade should be greater in proportion as the flame which it is intended to mask is greater and more bright; for if a shade be small, the light which Concerning the size of shades.

which it emits may be sufficiently strong to hurt the eyes, especially when viewed at a small distance.

The size and intensity of the flame being the same, the intensity of the light emitted by the surface of a shade that may it will be as that surface, and consequently as the square of the diameter of the shade inversely.

The larger the shade the less luminous it will appear when looked at.

If the intensity of the light emitted by a shade four inches in diameter be equal to four, it will be reduced to one by doubling the diameter of the shade, and that without any change in the total quantity of light which is diffused in the room. This shews the advantage to the eyes that will result from the use of shades of large dimensions.

Small shades appear dazzling.

The small spherical shades of roughened glass which are sometimes employed for lamps, have been found to emit a light too dazzling to the eyes. In order to remedy this inconvenience, all that is necessary is to make the shade larger. If these globes are more dazzling than globes of crape or gauze of the same dimensions, that circumstance proves no more than that roughened glass absorbs less light than these silk stuffs do; and from this we may conclude that the solid parts of silk are less transparent than those of glass, and consequently that this substance is less fit to be used in making shades for lamps than glass.

Great advantage of glazing windows with roughened glass when the illumination is very obliquely received, as in courts, alleys, &c.

I will just mention here a circumstance respecting roughened glass, which although not immediately connected with the subject of this paper, appears nevertheless sufficiently important to deserve attention. It often happens, in great towns, that a room has no other light than what it receives by windows which look into a small court, surrounded on all sides by high buildings: in these cases the room would be more copiously and better lighted by panes of roughened glass than by transparent panes. The rays of day-light, which descend almost perpendicularly from above into the court, fall upon the panes at so small an angle of incidence that, when the exterior surface of the glass is polished, they are in great part thrown off by reflection, and do not get into the room; and even those which, not being reflected, pass through the pane, as they fall directly upon the floor, where they are almost all absorbed, the objects in the room are very little enlightened; but when the pane is roughened, the asperities of the

glass presenting to the descending rays surfaces less inclined, more of the rays enter the glass, and passing through it upwards in various directions, light is diffused in all the parts of the room. And it is not solely for windows which look into small courts that it is useful to employ roughened glass; it may be used with much advantage in all cases where windows look against a high wall, at a small distance, and especially if the wall be of a dark colour. But to return to my subject.

Account of the lamp.

Without enlarging farther at this time on the construction of shades to be used for masking and softening the too dazzling lustre of lamps, I shall give an account of the new lamp which I have lately caused to be constructed, and which I have the honour of presenting to this learned society.

This lamp, which is destined to be suspended in the middle of a room, was particularly intended to light a dining-room, but it may likewise serve to light a drawing-room, or a billiard-table. The following are the details of its construction:—

A hollow hoop of tin, painted white, $12\frac{8}{10}$ English inches in diameter within, 16 inches in diameter without, and $\frac{8}{10}$ of an inch deep, suspended in a horizontal position, serves as a reservoir for the oil. In the centre of this circular reservoir there are three cylinders, or beaks, which inclose three circular wicks of the usual form and size. These three vertical cylinders, which touch each other, are soldered together, and connected with the reservoir by means of three oblique tubes $\frac{2}{10}$ of an inch square, through which the oil flows to the wicks from the reservoir.

In order to catch the oil which occasionally drops from these three cylinders, there is a cup, made of tin $4\frac{1}{2}$ inches in diameter above at its opening, and 1 inch in depth in the middle, which is placed at the distance of $\frac{1}{4}$ of an inch under the lower ends of the three cylinders.

Each of these cylinders is furnished with a chimney, or tube of glass, and they may be lighted either all three together, or two, or one only, according to the quantity of light that is wanted.

This lamp is suspended by means of a hoop of brass, gilt, $16\frac{8}{10}$ inches in diameter and $1\frac{1}{4}$ inch wide, having a little horizontal projection at its lower edge, internally, on which the

Description of a lamp by Count Rumford.

the circular reservoir of the lamp rests. To this brass hoop are fixed three arrows, of gilt brass, at equal distances from each other. These arrows, which are 6 inches in length and 1/8 of an inch in diameter, are in a horizontal position, on the outer side of the hoop, and in the direction of three radii drawn from its centre.

To these three arrows, at the distance of 3 inches from the hoop, are fastened the ends of three chains of gilt copper, each of 28 inches in length, by which the hoop that receives the lamp is suspended.

These arrows serve the purpose of separating the chains from each other, in order that the lamp may be removed occasionally, and replaced, without deranging the chains.

For a lamp with four wicks, which serves to light a large drawing-room, the gilded hoop that receives the lamp has six arrows, to which are attached six chains; but in order to be able to remove and replace the lamp, there is one of the chains which, being attached to its arrow by a small hook, it is detached occasionally and laid aside, in order to allow a passage for the lamp.

The gilded hoop which receives the lamp is ornamented with pendants of cristal; and from the lower edge of the hoop, immediately behind the cristal ornaments, there descends a hoop of white crape, of the same diameter as the hoop of brass, and 4½ inches in breadth, which serves to disperse and soften the direct rays of the flames of the lamp.

To reflect a part of the rays that mount towards the ceiling, in order to destroy the shadows that might be formed under the lamp, there is a conical reflector of white crape, which, resting on the three tubes that conduct the oil from the reservoir to the wicks, surrounds and conceals the tubes of glass that contain the flames.

This reflector is 12½ inches in diameter below, 5½ inches in diameter at its opening above, and 6 inches high.

The chief difficulty to be overcome in the construction of this lamp was to contain the oil in the reservoir in such a manner that it should not be in danger of being spilled in taking the lamp out of the hoop, in which it is suspended, in carrying it from one place to another, and in replacing it. Several attempts had already been made, by different persons, to construct

struct suspended lamps with circular horizontal reservoirs, but none of them had been attended with success. That which I now propose is simple in its construction, and appears to me to answer perfectly well.

Description of a lamp by Count Rumford.

This reservoir is closed above, so as to form a hollow hoop, and it has three openings on its upper surface, equidistant from each other. These openings, which serve for pouring the oil into the reservoir, have each $\frac{7}{10}$ of an inch in diameter, and are hermetically closed by three stoppers of brass, ground with emery. In the axis of each of these stoppers there is a small hole, $\frac{1}{10}$ of an inch in diameter, which is occasionally closed by a small screw furnished with a collar of leather.

When the reservoir is filled with oil, the three stoppers are put in their places, and the small holes are then closed by means of the three screws. In this state of things, as the air cannot enter into the reservoir by the opening at its upper surface, the lamp may be transported, and even inclined considerably, without any danger of spilling the oil. As soon as the lamp is placed in its hoop (where care is taken to suspend it in a horizontal position), the communication must be opened between the air of the atmosphere and the upper surface of the oil in the reservoir, which is done by unscrewing a few turns the small screws which are in the axis of the stoppers. The oil then resumes its natural level, and afterwards passes freely into the cylinders that contain the wicks in order to feed the flames.

That it may not be necessary to take out the screw entirely when a passage for the air is opened by means of the vertical holes in the axis of the stoppers, these screws, which are half an inch in length, are not complete, being reduced on one side to the half of their diameters, in their whole length, except about $\frac{1}{10}$ of an inch at the top near the collar of leather.

When the screw is unscrewed two turns, the part of the screw that still remains in the hole not being entire, a free passage is necessarily opened into the interior of the reservoir.

It would be possible to fill the reservoir of this lamp by one opening only, which would require only one stopper and one screw; but I have found by experience that it is inconvenient, and that it is much better to fill the reservoir by three holes, in the manner above described; for in that case the air gets

Description of a lamp by Count Rumford.

out of the reservoir easily, and the oil enters without any obstacle.

When this lamp is filled with oil the reservoir must be firmly placed in a horizontal situation, upon a stand made on purpose to support it during the operation, and the three stoppers must be taken out; the reservoir being filled, care is taken to replace the stoppers, and to close the little holes by the screws before it is taken from its stand.

These little holes must not be opened till the lamp is suspended in its place in the gilt hoop, and at rest; and attention should always be paid to close them before lifting the lamp to take it from its hoop. These precautions are absolutely necessary in order to avoid the risk of spilling the oil.

When this lamp is suspended at a proper elevation above the middle of a round table, large enough for placing conveniently ten or twelve persons, in a room 24 feet long, by 20 feet wide, and 15 high, not only the table, but likewise the whole room is completely lighted, without the least visible shadow being produced in any part, and without any person at table, or in the other parts of the room, being incommoded by the direct rays from the three flames which are united at the centre of the lamp. The diameter of the hoop of crape which masks these flames is so great that the light which it emits from its surface is very soft, although it receives the direct rays of the three flames.

As the light which this lamp diffuses in a room proceeds from one single source, the shadows of the enlightened objects are of course simple, and well defined, a circumstance which certainly contributes much to the ease with which we distinguish objects, as well as to the pleasantness of the illumination, and the preservation of the eyes.

In order to light the table of a dining-room or study, of 5 or 6 feet in diameter, a small lamp with one wick will be sufficient, and instead of suspending it from the cieling, it may be placed on a pedestal, at the height of 12 or 15 inches above the table. For a lamp with one wick, intended to burn 8 or 10 hours, the circular reservoir for the oil may be made 6 inches in diameter within, 1 inch wide, and $\frac{6}{10}$ of an inch deep. The conical reflector of crape, or roughened glass, for this lamp should be 8 inches in diameter below, 2 inches in dia-

meter

meter at the top at its opening, and 5 inches in height. The hoop of crape which surrounds the lamp should be 8 inches in diameter and 8½ inches in breadth. If it be desired to have more light upon the table, and less in the room, the conical reflector which covers the lamp above may be made of a thicker stuff, or even of tin, painted white within, and painted and varnished without.

Description of a lamp by Count Rumford.

To light a large drawing-room, or dining-room, a suspended lamp with six wicks may be used, with a reservoir of oil 18 inches in diameter within, 2½ inches in breadth, and $\frac{8}{10}$ of an inch in depth, surrounded by a hoop of crape 6 inches broad.

Such a lamp, hung at the height of 8 or 9 feet in the middle of a large room, would be found to diffuse a very gentle and agreeable light.

It is almost superfluous to observe, that the general form of this lamp is simple and elegant; and that it is susceptible of being easily ornamented, a circumstance which is of real importance in this age of refinement, and even in every age in which the sentiment of beauty has any influence on the manners and happiness of mankind.

N.B. The openings of the tubes above, which contain the wicks, are situated one quarter of an inch above the level of the oil in the reservoir, when the reservoir is full.

PLATE I.

Fig. 1.—Horizontal projection.

d. Hoop that serves for reservoir of oil.

e. Tubes that convey the oil from the reservoir to the cylinders.

f. Cylinders.

a. Brass stoppers, with their screws.

g. Brass hoop which serves to receive the lamp.

h. Arrows attached to the brass hoop: to these arrows the chains are fixed.

Fig. 2.—Vertical projection.

d. Hollow hoop that serves for reservoir of oil.

e. Tubes that convey the oil from the reservoir to the cylinders.

f. Cylinders, with their chimneys.

a. Brass stoppers, with their screws.

g. Brass hoop which serves to receive the lamp.
h. Arrows attached to the brass hoop.
i. Cup that receives the oil which may fall from the cylinders.
k. Cristal pendants attached to the brass hoop.
l. Hoop of white crape attached to the lower edge of the brass hoop immediately behind the pendants.
m. Reflector of white crape, which rests on the tubes *e*.

Fig. 3.—On a larger scale.

a. Section of the brass stopper with the small screw in its axis: the screw is represented as open to give admission to the air into the reservoir.
b. Collar of leather which, when the screw is closed, presses upon *c*, and excludes the air of the atmosphere from the reservoir.
d. Section of a part of the hollow hoop which serves as a reservoir for the oil.

IV.

Facts towards a History of Tin. By Professor PROUST.*

Tin and Muriate of Ammonia.

Facts and observations on tin and its compounds.

THE muriate when heated with granulated tin yielded the following results:—

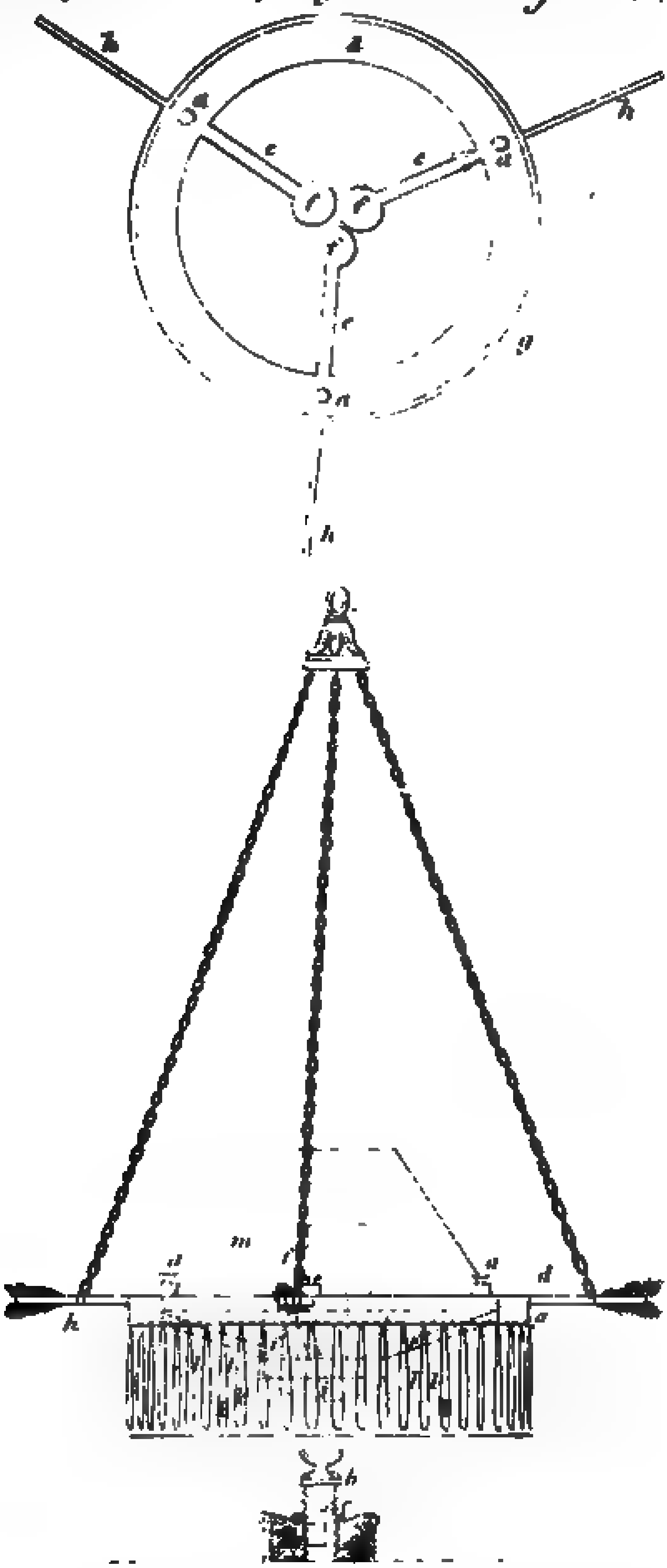
When the heat is on the point of evaporating the muriate, the tin acts upon the water of this salt, and decomposes it. The metal seizes its oxigen, and causes a disengagement of inflammable gas. One hundred ounces of muriate afford from eleven to twelve inches of gas: they may perhaps afford more; but the retort generally bursts before the operation is completed. This hydrogen has nothing in it remarkable.

At the close of the operation, a saline mass is found, composed of muriates of tin and ammonia, and of the granulated tin. The oxide of this muriate is only at the minimum; for it gives a purple colour with gold, and black with hydrosulphurated water, &c. If tin be merely boiled in a solution of sal ammoniac, a considerable portion will be dissolved.

* Journal de Physique, vol. lxi. p. 338.

Mosaic

Improved Lamp by Count Rumford



Mosaic Gold.

If a mixture of tin, sal ammoniac, and sulphur be heated together, it happens at the moment the latter is decomposed that the muriate of tin, which is formed, seizes part of its oxide, and transforms it into mosaic gold. But as the ammoniac, in proportion as it is set at liberty, saturates the new products, and disguises the results, we shall resume this experiment in a different manner. Facts and observations on tin and its compounds.

Muriate of Tin and Sulphur.

The muriate is first to be concentrated to the minimum, even till it congeals. The aqueous product of this by distillation contains muriate at the maximum. This happens because the latter is more volatile than the muriate at the minimum. Distillation is therefore a mean whereby the minor muriate may be purified from that whose oxidation has been increased by the atmosphere. The minor muriate is also volatile; but it requires a much higher temperature than the other: this is demonstrable by the process for making the fuming liquor. The muriate at the maximum is raised by a very gentle heat; whilst the muriate at a minimum remains in the retort: this is their difference.

Flowers of Sulphur are next to be cast into the congealed muriate, and heat gradually applied. In a few seconds volatile fuming muriate will pass over in considerable quantity. The excess of sulphur will fix about the neck of the retort, and at the bottom will appear a light mass of brilliant mosaic gold, and part spangle the dome of the vessel with gold-coloured flowers.

On an attentive examination of these products, it will be discovered,—

1, That the muriate of tin is divided into two parts.

2, That one of these is deprived of all its acid in favour of the other.

3, That it also parts with some of its oxigen, which raises the other to the quality of fuming volatile muriate.

4, That tin oxided to the minimum, combines with sulphur only in proportion to a certain reduction, which takes place in the quantity of its oxigen: this reduction may henceforth be considered as subject to a fixed degree, in common with

Facts and observations on tin and its compounds.

with all those which are determined by the affinities in general. The proof of this may be observed in the constant proportions of mosaïc gold: it exhibits a crystallisable volatile combination, as invariable in the whole of its characters as cinabar is, whatever may be the process from which it is obtained.

The following experiment offers a second proof of the diminution of oxidation, which takes place in the oxide at the minimum, before the production of mosaïc gold:—

Fifty parts of sulphur and one hundred of the grey oxide, or oxide at a minimum, deprived of water by a slight calcination, were heated in a defective retort. A moment arrived at which the mixture, though still at a very low temperature, entered into incandescence, and suddenly presented that phenomenon of ignition which is common to most metals when they combine with sulphur. After this appearance, the heat was raised till the mixture was faintly ignited, and it was continued till all the excess of sulphur was condensed in the neck of the retort. When cold, it was weighed, and was found to have lost 8 or 9 parts of its weight. The mosaïc gold remaining in the vessel weighed 120 or 121 parts.

Let us now examine these results:—

Sulphureous gas escapes from the retort, which accounts for the 8 or 9 parts lost of the weight; for nothing else escapes, as the excess of sulphur remains in the neck of the retort. Hence, if there were no formation of sulphureous acid, and consequently no loss of oxigen, the mosaïc gold obtained would be composed of 100 parts oxide + 20 of sulphur. But there is a deduction of oxigen: the mosaïc gold is therefore composed of oxide 100, — an unknown quantity of oxigen, 20 of sulphur, + a quantity of sulphur equal to that unknown quantity of oxigen. Mosaïc gold, therefore, is not a sulphurated oxide, in the degree hitherto imagined; or, in other words, a combination of sulphur with one or other of the two oxides of tin with which we are acquainted; but it is a sulphuret, whose oxide is fixed at a degree inferior to their constituting the minimum of oxidation of this metal: a constant term, I repeat; because, whenever the attributes of a compound present themselves without variation, whatever may be the process by which it has been obtained, the invariability in the proportion of its parts is always an inseparable consequence.

It

It remains, therefore, to ascertain what may be this new degree of oxidation, produced by affinity, exclusively to give existence to a singular combination, and to discover if it be capable of being exhibited separately, like those which we know do form the maximum and minimum of tin. I shall conclude this paragraph by observing that three operations, repeated with care, agreed to nearly half a grain in giving similar results.

Facts and observations on tin and its compounds.

The oxide at a maximum heated with sulphur produced an abundance of sulphureous gas, leaving mosaïc gold as a residuum. In this approximation, therefore, the metal abandoned all the oxigen comprised between 28 *per cent.* and the new term, inferior at 22, which we have just discovered.

If mosaïc gold be heated in a high temperature, the oxigen separates from the metal, combines with the sulphur, and escapes in sulphureous gas: but a part of the sulphur is retained from the oxigen by the metal itself: and the products are thus metallic sulphuret, sulphuric gas, and sulphuret of tin. Such are the new binary combinations produced by the ternary union of mosaïc gold, when urged by a strong temperature.

Bergmann, and after him Pelletier, were well convinced that mosaïc gold required for its formation a greater quantity of sulphur than the simple metallic sulphuret. For, besides the sulphureous gas already mentioned, a portion escapes entire on heating the mosaïc gold. It is a curious fact, that this metal, whose affinity to sulphur might be expected to decrease in proportion to the quantity of oxigen it contains, should be capable of attracting a much larger portion than pure tin.

If three parts of oxide at a maximum, and one part of mosaic gold be made red-hot, the latter will be decomposed. The sulphur contributes to disoxidate parts of the oxide; sulphureous gas is afforded, and after the operation a grey powder is found, being a mixture of oxide at a minimum, metallic sulphuret, and white oxide. Muriatic acid dissolves the grey oxide, and the metallic sulphuret with this produces sulphurated hydrogen. The oxide at a maximum, being much less soluble, is the last to dissolve. After decanting and adding fresh acid, this second solution differs from the foregoing in giving a yellow colour to hydro-sulpurated water, whilst the former gave a deep brown.

Pelletier, who observed so acutely, has suffered himself to be

Facts and observations on tin and its compounds.

be imposed on by some appearance with which I am unacquainted. He says that sulphuret of tin and cinnabar heated together, yield mosaïc gold. A result so contrary to principles, appeared incredible; I repeated the experiment, and found, that these two sulphurets heated, produced merely cinabar and sulphuret of tin; the one volatilised, the other moulded in the bottom of the retort.

All these facts sufficiently acquaint us with what takes place in the operation of converting tin into mosaïc gold. It would be useless to urge that the intervention of mercury is as superfluous in this preparation, as in that of fuming muriate of tin, as I have shewn, in 1801, in the "*Journal de Physique*," vol. lii.

Mosaïc Gold and Acids.

Sulphuret of tin is composed of metal 100, of sulphur 20. Of this Sago and Bergmann were assured: I also found the same proportions. Muriatic acid readily acts upon this sulphuret of tin at a minimum, sulphurated hydrogen, &c. But it is a singular circumstance that the same acid has not the least influence upon mosaïc gold; it merely clears it of metallic sulphuret, as has been remarked by Pelletier.

Nitric acid, which likewise easily destroys sulphuret, has as little power over mosaic gold: a fact not less extraordinary, when we recollect the facility with which tin and sulphur, under other circumstances, are acted upon by nitric acid.

To dissolve mosaïc gold, aqua-regia must be used, and it must have a long and continued boiling. The result is a kind of sulphate of tin at a maximum. It is decomposed by the heat, and after drawing over oil of vitriol, a residuum is obtained of spongy white oxide, which must be washed to cleanse it from acid. The edulcorating water contains not an atom of tin; sulphureous hydrogen discovers nothing in it, unless it be atoms of mercury, when the mosaïc gold of commerce has been used, arising from the small quantity of cinnabar sometimes found in it.

One hundred grains of saltpetre, and fifty of mosaïc gold, heated gradually in a small retort, exploded with much violence, and had nearly been attended with serious consequences to me.

Sul-

Sulphuret and Potash.

Facts and observations on tin and it's compounds.

Liquid potash has not the least action upon sulphuret of tin; but of antimony is affected quite otherwise under similar circumstances. Antimony, however, is far from possessing so great an affinity to oxigen as tin does. This diversity shews how cautious we ought to be in forming previous judgments in chemistry.

Mosaïc Gold and Potash.

Liquid potash, assisted by heat, dissolves mosaïc gold. The changes it undergoes are curious. As they tend to throw a light on the theory of oxidation, it may be useful to detail them; but to do this with clearness, it is indispensable, first, to speak of the hydro-sulphurets of tin, combinations which I have hitherto but imperfectly understood, and of which the denomination will stand in need of being improved.

Major Hydro-Sulphuret of Tin.

A current of sulphurated hydrogen is passed through any solution, of which the oxide is perfectly at a maximum; and a yellow precipitate will be obtained, to be collected, washed, and left to dry. To obtain more precipitate, it is proper to saturate the excess of acid in the solution; for when that predominates too much, the hydrogen with more difficulty attracts the oxide.

The precipitate possesses the following characters: Heated with marine acid, it dissolves with effervescence, yields abundance of sulphurated hydrogen, and is reduced to a simple muriatic solution, in which the oxide is always found at a maximum. This clear yellow precipitate, so long as it remains clear, is what we call *hydro-sulphuret major*: it augments the number of those combinations, which serve, in chemistry, to demonstrate the facility with which mere heat can vary the affinities. At an ordinary temperature, sulphurated hydrogen is an acid which takes the oxide from muriatic acid: but at the temperature of boiling water, the latter, in its turn, acts upon the sulphurated hydrogen, and resumes its oxide of tin.

Dry hydro-sulphuret of tin is of a dark-brown colour: it is vitreous in the fracture of its pieces, as are likewise the oxide major, the purple of cassius, and the native oxide. Potash readily dissolves it, and acids precipitate it without alteration.

Facts and observations on tin and its compounds.

If it be gradually heated, it affords water of new formation, and gives out sulphureous gas, a little free sulphur, and a residuum of very beautiful mosaïc gold.

These latter products distinctly shew, that the hydro-sulphuret cannot sustain a high temperature without a tendency to become simplified: that the tin, for example, communicates oxigen to the two principles of sulphurated hydrogen, retaining only such a proportion as the affinities render necessary for the new combination in which it becomes mosaïc gold; and, lastly, if the temperature be augmented, the mosaïc gold, abandoning this oxigen, passes into the state of metallic sulphuret, a combination still more simple than mosaïc gold.

Minor Hydro-Sulphuret of Tin.

If a saturated solution of tin at the minimum be treated in the way already described, a powder is obtained of the colour of coffee, or a little darker, which is to be washed in boiling water. This slight heat augmenting the attraction of the particles, enables the hydro-sulphuret to resist the action of the air, which otherwise is apt to change it from brown to yellow, even whilst on the filtre, that is to say, from minimum to maximum.

This hydro-sulphuret is distinguished from the preceding by the following qualities:

It is black, or appears so; will not dissolve in potash without changing its state; and yields no mosaïc gold by heat.

It possesses, in common with the foregoing, the property of dissolving with effervescence, of restoring the gas which saturated its base; and, consequently, of giving muriate of tin at a minimum, if muriatic acid be used.

If this hydro-sulphuret, when fresh, be heated with potash, it divides in two: one part of its base gives to the other all its oxigen, and is thus reduced to the state of simple metallic sulphuret. In this state it is collected together at the bottom of the vessel. The other part, raised by this addition to the maximum, attracts also the sulphurated hydrogen of the former, and thus becomes hydro-sulphuret major. The metallic sulphuret being thus separated, a yellow powder is precipitated by acids from the liquor, possessing all the characters described in the hydro-sulphuret of tin at a maximum. I have observed, in speaking of antimony, that its hydro-sulphuret, or kermes, treated with potash, can also yield sulphuret of antimony.

Black

Black hydro-sulphuret, heated in a retort, gave abundance of water and a little free sulphur, but no sulphureous gas, and was reduced to a pure and simple metallic sulphuret; that is to say, although the quantity of tin in this hydro-sulphuret be as 22 to 100, it does not stop at this inferior degree of oxidation, which would turn it into mosaïc gold. It should seem, that the hydrogen, being presented to the oxigen of the oxide in a more powerful degree than in hydro-sulphuret at a maximum, saturates and converts it entirely into water, leaving none with the metal, which, as we have seen, can never form mosaïc gold without a certain portion of oxigen. Facts and observations on tin and its compounds.

We shall now proceed to the changes effected in mosaïc gold by potash.

Mosaïc Gold and Potash.

Liquid potash, assisted by heat, quietly dissolves mosaïc gold, and assumes a greenish shade. From this solution acids separate a yellow powder, which is no longer mosaïc gold but hydro-sulphuret at the maximum: there is, therefore, a decomposition of water; the base of the mosaic gold deprives it of oxigen, to raise itself to the maximum of oxidation, whilst the hydrogen, on the other hand, combining with the sulphur, constitutes sulphurated hydrogen, and the mosaïc gold thus becomes transformed into hydro-sulphurated oxide major; or, in other words, into hydro-sulphuret of tin major: in fact, this precipitate possesses none of the properties of mosaïc gold; muriatic acid dissolves it, disengages the sulphurated hydrogen, and reduces it to a simple solution of muriate, whose basis is at a maximum.

This reminds us of that decomposition of water, which accompanies the transformation of sulphur of antimony into kermes. The antimony is oxided at the expence of the water, which it decomposes; whilst its sulphur is hydrogened, and furnishes the antimonial oxide with the requisite saturating acid. There is, however, between antimony and tin this difference: that though the latter is raised suddenly to its maximum in potash, antimony never passes its minimum in changing into kermes. Indeed, it is still more surprising to observe that sulphuret of tin, whose affinity to oxigen appears far

Facts and observations on tin and its compounds.

far greater than that of mosaïc gold, cannot decompose water like it.

But mosaïc gold is not singular in undergoing this inversion. For example, if oxide of tin at the minimum be heated with potash and sulphur, the oxide will be suddenly raised to the maximum, and changed into hydro-sulphuret major.

If muriate of tin minor be poured into potash, exempt from sulphurated hydrogen, it produces a yellowish precipitate, inclining to fawn, which is nothing else than hydro-sulphuret major. Oxide of tin at the minimum has, therefore, a peculiar disposition to decompose water, and to be suroxidated at its expence. Thus mosaïc gold cannot be had by the humid way. Pelletier, who went no farther than the precipitation of the muriate in sulphate of potash, thought he had obtained mosaïc gold, because his precipitate, when heated in a retort, was converted into that substance; but it did not occur to him then, that what he was heating was not, as it should have been, a composition capable of resisting acids; in a word, it was not mosaïc gold.

If all liquid sulphurets were hydrogened, as Berthollet imagines, the precipitates which they give with muriate at a minimum, would be very much mixed with black hydro-sulphuret; and, consequently, of a very deep colour; but nothing is less general.

When the precipitate is very yellow, capable of complete solution in pot-ash, and the solution does not turn brown when mixed with hydro-sulphurated water; the conclusion must be that there are simple sulphurets of potash, as well as compound ones.

But we must not forget that no liquid sulphuret is strictly without a little hydrogen, as I have demonstrated; it is this which clouds the yellow colour of the hydro-sulphuret major, and gives it a drab coloured hue; but these small portions of hydrogen cannot be considered as necessary component parts of the sulphurets; nor as mediums without which the sulphur could not be suspended in the alcali? I cannot admit this. Put diluted sulphuret of potash into three glasses, and add to two of them a little hydro-sulphuret of potash, in unequal proportions; then let a few drops of muriate of tin

tin minor fall into each, and three very different shades will immediately be perceived, which perfectly confirms all that I have here affirmed.

Facts and observations on tin and its compounds.

Mosaïc gold, then, décomposes the water in presence of sulphuret of potash, as has been just shown; but it also decomposes it in the midst of hydro-sulphuret of potash, one of the most disoxidating compositions known. Heat mosaïc gold in hydro-sulphuret, and it will be dissolved : add to the solution an acid, the precipitate will be yellow, and exhibit all the properties of hydro-sulphuret of tin major: that is to say, sulphurated hydrogen, alone or combined, can never deprive tin of its tendency to decompose water, in order to arrive at the maximum of oxidation.

Muriate at the Maximum and Tin.

If hydrogen, assisted by the affinities which sulphur adds to those which it possesses, cannot lower the oxidation of tin, it will be conceived that hydrogen alone is still less likely to effect it; and, indeed, if thin plates of tin be heated in a solution of tin at the maximum (such as the diluted fuming muriate, or residuum of muriatic ether, an old sulphate, &c.) the oxide at a maximum separates in white flakes, which become vitreous in drying, and, in a word, possess all the properties of which we already have said so much. This is a mean of restoring the integrity of solutions which have been changed by the atmosphere. During this solution, a decomposition of water, and disengagement of hydrogen take place. This hydrogen, which under similar circumstances would lower the oxidation of iron, has not the same power over that of tin; zinc itself precipitates the oxide of tin, and the hydrogen, procured in such great abundance, has no greater effect upon this oxide.

All these Experiments prove, that the oxide of tin in passing from a minimum to a maximum decreases in solubility, and follows the same law as iron, manganese, cobalt, and many other metals; they also show why it is that acids have so little action upon the native oxides of this metal, and that potash, on the contrary, has so great an aptitude to dissolve them, as has been remarked by Morveaux, viz. that native, oxide

Facts and observations on tin and its compounds.

oxide is also at 28 in 100. In this oxide, whose fragments are equally vitreous with those of the artificial, the condensation is so great, that when heated with sulphur, it yields but slowly to disoxidation; the process must be repeated two or three times, during which it emits sulphureous gas, and is at last changed into mosaïc gold.

White crystals ought, undoubtedly, to be replaced among ores of tin, from which they have been separated for want of proper examination. It is true that tungstein has been frequently taken for the white oxide; but this last, though rare, does not less positively exist. Among a collection of minerals sent me from the mines of Monteray, in Gallicia, were three white crystals, opaque, and quite disfigured by rolling about, which I at first took to be tungstein; but finding that, after remaining in muriatic acid for a twelvemonth, they still remained unchanged, I examined them again, and found they were pure oxide; these are the same which I changed into mosaïc gold by sulphur. The grey and brown crystals also change into this substance, but with more difficulty; their mosaïc gold is contaminated with sulphuret of iron; it may be discovered by muriatic acid; it also retains some sand and small fragments of undecomposed crystals.

A phenomenon not less interesting to the sight than the judgment may be observed in the solution of indigo in potash, prepared by the medium of the oxide at the minimum. Put indigo, oxide, and liquid potash into a bottle, and stop the mouth quite close; let it be well shaken from time to time; and when the indigo has entirely disappeared and the liquor become of an orange-yellow colour, proceed to the following experiment:

Pour cold water into one glass; boiling water into a second; and hydro-sulphurated water into a third: then pour a few drops of the indigo solution into each; the water in the first glass will immediately become blue; that in the second, a beautiful orange-yellow; and that in the third, will be similar to the second. In all this we may perceive the influence of the atmospheric oxigen. The indigo being disoxidated in the solution instantly attracts the oxigen commonly suspended by cold water, and resumes its primitive colour; whilst the boiling water, being deprived of its atmospheric air, fails to produces a similar phenomenon. In the hydro-sulphurated water,

no

no change is perceived, because this water no longer contains oxigen.

Facts and observations on tin and its compounds.

A linen or cotton rag, previously wetted with boiling water, if dipped into the second glass comes out of a yellow colour, which changes successively to green, and then to blue, with which it is finally firmly dyed. If the contents of the second glass while still yellow be thrown into a large bell-shaped vessel, and whirled round, the liquor will pass rapidly from yellow to perfect blue. The indigo having recovered its native colour, becomes insoluble and settles; in like manner, if a few drops of oxigenated muriatic acid be put into the solution whilst yellow, the blue colour will be instantly restored; but more of the acid will destroy it again. These effects confirm more and more the ideas of modern chemists respecting indigo.—In India and the islands indigo is not drawn from the vegetable juices which contain it, so much as it is precipitated by oxigen; and in Europe it is only fit for the purpose of dyeing in proportion as this oxigen is destroyed. The effects of the woad vat though so different in their appearance from those produced by the disoxidating minerals, are nevertheless subject to the same theory. The fermentation of the green fecula of the woad, or of bran, or madder, &c. disengages a portion of hydrogen, which attacks and disoxidates the indigo, and restores its green colour. After frequent opportunities of observing the process in the vat, I am persuaded that any other green plants rich in fecula, such as cabbages, and all cruciferous plants in general, would produce similar effects, and might be advantageously employed, particularly where woad cannot conveniently be obtained.

It may be interesting to commerce, and to manufacturers to be informed that 100lbs. of linen, well scoured require 6¼lbs. of indigo, to dye it of a turquoise blue, the deepest tint that can be given; this I have obtained from an experiment made some time ago, with great care, in a wood vat.

I could wish to give in this place a set of experiments made upon the scarlet, with solution of tin, by sulphuric acid, sea salt, and saltpetre, to avoid the use of aquafortis; but I wish to correct some particular parts, which I have not leisure just now to do. I can, however, assert that sulphuric acid, and even the salt, which both change scarlet to violet, appear to be

 no

Facts and observations on tin and its compounds.

no obstacles to the success of the scarlet dye. I have a number of patterns, of the most perfect dye: the least of these patterns is 12 inches square, which I mention lest they should be confounded with small scraps dyed in a wine glass.

Oxide of tin at the maximum is very soluble in potash: this solution easily crystallizes. The crystals appeared to me to be lenticular, adhering to each other without order: they taste like potash; dissolve in water, where they lose part of their oxide; become dense in a retort, yield water, ignite without melting, and preserve their shape. Beyond this I have not observed any thing remarkable.

Madrid, April, 1805.

V.

Experiments and Observations respecting the Manner in which the Gases are afforded in Water by Galvanism, and various essential Points of Theory. *By* H. B. K.

To Mr. NICHOLSON.

SIR,

Introductory observations.

SOME respectable chemists having expressed a great desire that I should give in your Journal my experiments, which form my opinion that the gases obtained in Galvanism vary from different causes, as it is an opinion that militates so directly against the Lavoiserian theory; therefore they say the experiments which lead to it, ought to be given to the public, and not rest upon a bare assertion.

I must make these general observations, that I give the name of pile to all Galvanic apparatus, even when they are formed of troughs; and also, that the same general names are given to the wires, calling them the silver wire, and the zinc wire, whatever metals formed the pile.

The Galvanic pile yields hidrogen and oxigen in water with gold wires—no gas with animal fibres—but the gas as before, if the fibres have ter-

Having constructed a pile, I found that it yielded hydrogen and oxigen gases with gold wires, and which were the common gases usually formed, and that they detonated. I then, instead of the gold wires, used either animal or vegetable substances; but I found they generated no gases. Then it would appear, that the metals are necessary to this phenomenon as

as well as the Galvanic fluid. Then instead of letting these animal and vegetable fibres enter the water that was to be acted upon, I placed two very small and short pieces of gold wire immediately in contact with the water, and then united to them the animal fibres that were connected with the plates of the pile, but there were scarce any gases produced; but I found as I lengthened these gold wires, in the same proportion, I produced gases. That being ascertained, I endeavoured to discover whether the silver or the zinc wires were more necessary to forming the gases. The silver wire was made of gold, the usual length, without any animal fibre; and the zinc wire was a very small piece of gold wire, placed in the water, united to the pile by animal fibres. Gases were equally produced, the same as if two long gold wires were used, at both the zinc and silver ends of the pile.

minations of gold wire, and more the longer the wire.

The quantities of wire and fibre at each end need not be equal.

I next endeavoured to ascertain, whether one metallic wire was better than another; and I found that they all gave gases; but the most calcinable wires in the greatest proportion. I also found, by examining the gases, that the easy calcinable wires produced the greatest proportion of inflammable gases, and platina wires the most of oxigen and azote gases. I next tried charcoal wires, and I found that they formed more gases than the animal or vegetable fibres, but less than the metals, and that the silver charcoal formed by much a greater proportion than the zinc charcoal; and, as Mr. Davy also found, that they formed neither hydrocarbonate, nor carbonic gases; but I found them gases of less inflammability than are produced from the metals, as well as less in quantity; and also a more considerable quantity of azote gas than the metallic wires produced, for there was a proportion of azote gas in the gases, whatever metallic wires were made use of; but gold and platina wires, yielded the most, but not equal to what was produced by the charcoal wires. It clearly appears, then, that metals, or charcoal, are necessary in forming the gases.

The most oxidable wires give the most gas and most inflam:—platina most of oxigen and azote.

Charcoal wires less than metals, and the silver ends[?] they afford most azote.

The next consideration was to find what changes these wires had undergone from producing the gases. A thin iron wire, which had for a long time been used for the silver wire, the galvanic fluid, in consequence, passing through it: upon examination it was found to have lost its ductility, and it appeared to shiver in pieces when struck with the hammer, something

The wire and charcoal were less combustible after this process.

thing like cast iron : its fibres appeared like it. It afforded less hydrogen gas, with a solution of the vitriolic acid and water, than the same weight of wire that had not been used in the galvanic process : and I should have supposed, that it would have burned with less inflammation, if it had been burned in oxigen gas : two slips of charcoal, that had been long used as galvanic wire, had lost greatly their combustibility ; and when applied to the calces of metals, produced a less proportion of them, than the same quantity of charcoal would have done, that had not been galvanised. The power of the pile was by no means so strong, as to suppose there had been any combustion in any of the wires ; but evidently their combustible principle or property had been extracted by the galvanic fluid, producing gases. It is clearly so, from this self evident demonstration, namely, these gases are well known to be capable of combustion. To prove this doctrine, I took incombustible bodies, as the animal fibre, well washed from all its combustible matter, for there was only the white muscular fibre left, and used it instead of the metallic, or charcoal wires, and they produced no gases, neither from the silver nor zinc sides. But when, instead of water being placed between the muscular fibres, I placed a solution of potash, I found the solution very soon nitrated. Now it must appear, that the acid which undoubtedly the galvanic fluid possesses, instead of having the inflammable substance of the wires to unite itself to, so as to form gases, united itself to the potash neutralizing it. I think these are such self evident truths, that they cannot possibly be otherwise explained.

Arguments against the Lavoisierian theory of Galvanic effects.

The Lavoisieran explanation of these phenomena, upon the composition of water, is directly contradicted by every fact. If the wires acted by aiding the galvanic fluid in decompounding the water, we cannot rationally suppose that the zinc, being the calcinable wire would produce the oxigen gas : for this theory says, that when metals are calcined by water, the metal seizes its oxigen, and its hydrogen is set free as gas ; and its advocates have been pushed to this extraordinary explanation, that the galvanic fluid seizes upon the hydrogen of the water, leaving the oxigen free ; but then, in this case, it would do the same when the animal fibre was used instead of the metal : but it is not so ; the animal fibre forms no gas.—

Besides

Besides, we are forced upon this highly improbable opinion, that the galvanic fluid can carry this hydrogen through even the body of a man; for the phenomena take place if he forms a part of the circuit. But this circulation is directly contradicted; for it appears clear, from my experiments, that the fluid passes from the silver wire to the zinc wire; for when the animal fibre was placed at the silver side, and only connected with a small portion of metallic wire, to unite it to the water, no gas was produced; but when placed upon the zinc side, gas was produced. The zinc side only requiring a metallic body, ever so small, just to receive the fluid out of the water; if after that it had the muscular fibre united to it, it makes no difference in the production of the gases. It appears to operate by hindering the galvanic fluid which was united to the combustible part of the silver metal, from passing through it, as by that union, it had lost that tenuity or penetrability, so as to be admitted through the close pores of the metal, and the metal may also operate by its combustible matter at its point, which is in the water, uniting to the galvanic fluid and form gas. I found that when the discs were filled with ammonia, there was a greater proportion of hydrogen gas; and when they were filled with acids, more of oxigen formed at the wires: also, the larger the plates of the pile, the less of gases were produced; and I think there were more of the oxigen and azote kind in proportion; and also, the galvanic fluid had a less tearing sensation to those who were electrified by it, and evidently gave them a less shock, even when the plates were so large as to produce the combustion of metals, and when so large, they produced the most nitric acid. That tearing sensation of the pile, seems to be from the fluid being united with the combustible part of the metals, and, in consequence, circulates with less ease through the animal body; and, therefore, gives that tearing sensation. It is from this that the fluid requires water to assist it in entering and passing through the skin of animals, as the hands requiring to be moist when you touch the galvanic wire. But the pure electric fluid requires no such aid; and this is the only difference in those two fluids.—Therefore, I should think that the galvanic fluid is more adapted to remove obstructions in diseases; the one possessing so much less of combustible matter than the other.

Facts and observations in explanation of the theory of Galvanism.

That

Facts and observations in explanation of the theory of Galvanism.

That there is an acid similar to the marine in the Galvanic fluid, appears from the zinc wire, when it is silver, having some *luna cornea* upon it, even though the water that is placed between the wires is pure distilled water.

The action of the pile seems to be from two causes; the first, from two metals being united, which have different saturations of the electric fluid; therefore, by being united, the saturation of each is disturbed, producing a motion in the fluid, and the pile being formed of water in the discs, the water being not so good a conductor of the Galvanic fluid, there is an obstruction which makes an accumulation, and it accumulates to such a pitch, as to overcome the obstruction, and then, when it is overcome, there is a strong current produced like a river's obstruction, producing streams. But then the principle of the electric fluid being so repellant, the particles, when accumulated, repel each other with such power, as to give great velocity to its motion. It has this great repellant power, that it can tear buildings to pieces, when its passage is obstructed, aiding its velocity and power.

I am now describing the simple pile, with nothing but pure water and pure metals; but there is also a calcination of the zinc metal, particularly when active bodies are placed in the discs, which actively calcines the zinc metal, and by that means sets a quantity of its electric fire free. The fact that you mention, Mr. Nicholson, that metals by having the electric fluid pass through them become lighter, must add strength to my opinion, that it is the combustible part of them, which the electric fluid receives from them; for if you will chemically examine the metals after the process, they will be found to have lost this matter.

I hope I have given a regular chain of facts, which it will be found difficult to misconstrue; I shall not examine that doctrine, by an extensive examination of a variety of different facts, it being too wide a field for your journal; but just take those which appeared in your last number, and see whether they are explained by the French theory, in that simple and easy manner, as my explanations are made; doing it in a cursory way, for fear of making my paper too long. I shall first notice Mr. Northmore's additional experiments, which I think very valuable, and likely to be aiding in elucidating the truth

truth, when exempt from the shackles of theory. Upon condensing nitrogen gas upon lime, he procured the nitrate of lime. In this experiment then, he procured the nitric acid, which he could not do in his former experiments, and there was no oxigen here to unite with the nitrogen. This directly confirms my opinion hinted at in my last, that this nitrogen gas was formed of the nitric acid, and the combustible animal matter; for Dr. Priestley only procured from the nitric acid and muscular fibre, nitrous gas, but when he exposed the ingredients to heat, he procured less nitrous gas, and when the acid was much diluted with water, and heat applied, he procured azote (nitrogen.) See his Experiments, vol. ii. beginning at p. 147. Also to shew what effect heat has upon these nitrous gases, Dr. Priestley, vol. iii. p. 328, in heating the nitrous oxide in malleable iron, says the bulk of the air was encreased, and become all phlogisticated air (nitrogen). Now when nitrogen was compressed on lime there was no oxigen to form it into the nitric acid, but the fire which the lime possessed set fire to the combustible matter it was united to, which appears from its violent explosion."

Fact and observations in explanation of the theory of Galvanism.

Experiment 9. He also compressed nitrogen gas upon the gaseous oxide of carbone, and nitrous acid was formed: now in this experiment there was no oxigen, but if he had compressed nitrogen gas upon the carbonic acid gas, he would have formed no nitric acid as I found.

His observations that nitrogen is the cause of explosions, are not just; for the strongest gunpowder is made of the ox-muriate of potash, instead of nitre, also phosphorus and sulphur explode when melted under water; besides many other examples in chemistry.

Mr. Northmore's experiments on the compressions of the oxiginated muriatic acid gas, are also very valuable, it becomes more concentrated, which adds, as he observes, to its pungency, and its volatility. But what will appear extraordinary to the advocates of the Lavoisieran theory,—when I compressed either atmospheric air or oxigen gas upon the oxigenated muriatic acid gas, they became injured, decompounding each other; the same effect as nitrous gas would have had and the oxigenated muriatic gas became the liquid muriatic acid. That compressing hydrogen upon the oxigenated mu-

riatic

Facts and observations on explanation of the theory of Galvanism.

riatic gas instead of its becoming weaker, according to their theory, the hydrogen should have united to the oxmuriatic oxigen and formed water, but it became a stronger, more pungent and volatile gas, destroying vegetable colours more actively, just the reverse of what this French theory teaches. It appears clearly that the muriatic acid becomes a gas from inflammable matter, also that its volatility, pungency, and its power of destroying vegetable colours, proceed from this combustible matter, and that this combustible matter of this oxmuriatic gas, when united with oxigen gas, produces an active fermentation, setting loose a great quantity of free fire, the same as nitrous gas and oxigen gas do, the one mixture forming the nitrous acid, and the other the muriatic acid. This accords with the doctrine of our forefathers, that combustible matter makes bodies become volatile, and it appears extraordinary, how far we have departed from this *clear, simple and obvious doctrine.* But I am afraid I have made the communication long enough, therefore you shall have the remainder in the next, with your permission.

I am, Sir,

Yours, &c.

H. B. K.

London, April 17, 1806.

VI.

A Chemical Examination of the Bark of the White Willow and of the Root of the Herb Bennet, compared with Quinquina; considered in a medical Point of View. By M. BOUILLON LAGRANGE,* *read before the Society of Medicine at Paris.*

Inquiry from the chemical properties of bitter vegetables, how far they may be of medical value.

IN the memoir which I read, last Floreal, before the class of Physical and Mathematical Sciences of the National Institute, on tannin and gallic acid, I mentioned, that in pursuing my researches among several vegetables, called bitter, wherein the tannin was supposed to reside, I had observed properties in some, which led me to examine them in a medical point of view. Prior to the communication of my experiments and of the reflections I had made upon them, I wished to ascertain how far the art of healing might derive

* Annales de Chimie, vol. liv. p. 287.

advantage

advantage from the two substances which are the objects of this note. I know that a slight success is not sufficient to fix the opinions of physicians; that it is useful to multiply facts, and that it remains for practitioners to decide upon so important an object. These considerations have determined me to present my labours to the society of medicine, in the persuasion that I should no where find men more enlightened, or more impartial, than are those who compose it.

Chemical examinations can only afford probability.

Though the chemical analysis may not lead us to sure results as to the application to be made of any medicine, yet it ought at least to enlighten the physician, and give him a kind of security.

Such is the object I have in view; and should my conjectures, founded on chemical principles, prove successful; medicine will not only have the advantage of rendering indigenous vegetables useful, but it will be no longer tributary to foreigners, who frequently send us the mere refuse, or articles which they would not themselves make use of.

Mention of some vegetables, &c.

They who devote themselves to the art of medicine healing have already some knowledge of the medicinal properties of the barks of the willow and chesnut trees, and of the root of the herb bennet. It is known that these substances are employed in some parts of Germany; and many members of this society have used them beneficially, particularly our colleagues Desessart, Coste, Willemet, &c. We want, therefore, only repeated and well attested facts. Far be from me any idea of empiricism; no one can hold it in greater detestation, or wish more strongly for its extirpation; but I believe if the society were to direct its attention towards a great number of indigenous vegetables, it would discover in some properties no less certain than those of exotics; and the facility of obtaining them would generally cause them to be preferred. The means of acquiring this knowledge are simple: let comparative essays be made; abandoning all idea of routine, which commonly confines the art to its state of infancy. It must be confessed that we are almost always deceived in the effect, expected from a medicine. The cause is sought in remote discussions; but it is near us: who does not know that for several years there has existed in commerce a great number of barks all sold under the appellation of quinquina.

The materia medica, but particularly quinquina, very often adulterated.

Let us, for a moment direct our attention to the apothecaries in the departments, particularly such as are at a distance from large cities: they purchase, without suspicion, these barks, persuaded that they are buying quinquina; they make use of them; the operation cannot answer their intention, which is attributed to the disorder. You are better acquainted than I am with the consequences resulting from the use of a bad article; why not therefore seek for means to check the evil, and to throw light upon so pernicious an abuse? Though we should not entirely succeed, the intention would at least be entitled to favor.

White willow bark, and root of the herb bennet.

It is long since the white willow and root of the herb bennet were ranked among vegetables proper for tanning; they do indeed, possess qualities similar to those of oak bark; but as much as they differ from this substance in this character, by so much do they approximate to quinquina in medicinal properties.

Of the Bark of Willow.

Examination of willow bark

The bark of young branches appear to me to be preferable; they should be used dry and broken. The water wherein this substance has been boiled, acquires a deep yellow tinge, bordering on red, which becomes turbid as the liquor cools.

When several decoctions have been made, the last are always the most coloured.

Its aqueous decoction.

This *decoctum* has a bitter and very rough taste. It feebly reddens tincture of turnsole; is abundantly precipitated by the *solutum* of glue, and by the carbonates of potash and ammoniac.

Habitudes of the decoction, with various agents.

Acetate of potash and muriate of ammoniac cause but a slight precipitation: indeed, with the muriate, it is scarcely perceptible.

If carbonate of potash be added at the time of making the decoction, the liquor assumes a deeper colour. This change seems to result from a disengagement of carbonic acid, which leaving the potash disengaged, causes it to act as an alcali upon the colouring matter of the bark, and upon a portion of the resin dissolved by the water; for the liquor no longer becomes turbid in cooling. These phenomena have been observed in the decoction of quinquina, by many chemists.

Lime

Lime water poured into the *decoctum* of willow bark, throws down a precipitate of a clear blue colour, which afterwards becomes fawn.

Sulphate of iron gives a dark green precipitate; if the decoction be very concentrated, it passes to black, particularly in the latter decoctions.

Many other metallic salts are also decomposed, such as nitrates of mercury, of silver, acetate of lead, sulphate o copper, and antimonial tartrite of pot-ash (tartar emetic.)

Alcohol precipitates flakes but little coloured, whilst the supernatant liquor is highly tinged.

Decoction of willow bark affords a fine flaky extract like quinquina.

By evaporating the *decoctum* to the consistency of syrup, and afterwards drying it on plates, an extract is obtained, dry, brilliant, separating in scales, of a beautiful red colour, rather deep, of a very bitter, acerb flavour, and possessing all the characteristics of the dry extract of quinquina, except that it attracts very little of humidity from the atmosphere.

Alcoholic tincture of willow bark.

The alcoholic tincture of willow bark is of a yellowish green colour, very bitter to the taste, and its transparency is disturbed by water.

The phænomena produced in the *decoctum* by the addition of *solutum* of glue and sulphate of iron are also seen in the alcoholic tincture.

Lime water forms in it a blueish precipitate; which proves that the bark contains a small quantity of gallic acid soluble in alcohol.

Evaporation of the alcohol leaves a brilliant substance, of a deep yellow colour, and very bitter; it liquifies in a gentle heat, and if thrown upon hot coals, sends forth a thick aromatic smoke.

All the experiments on willow bark are similar to those on quinquina.

On considering all these products, we readily recognise their similarity to those obtained from quinquina. But, it may be asked, are the quantities the same? Perhaps we might answer in the affirmative; but I thought it unnecessary to make calculations of the respective quantities, as they are so very variable, even in the same species. Besides, the difficulty of making such computations in vegetable and animal compositions is well known; I even believe it to be impossible to obtain similar results in repeating experiments of this kind; and though I have not mentioned the other constituent parts of

 this

this substance, which may be discovered by its complete analysis, it cannot present any uncertainty in regard to its properties. It appeared to me most essential to verify the predominant parts, those which physicians, of all times have acknowledged to possess the real and distinguished properties.

Of the Root of the Herb Bennet. (Geum urbanum, LIN.)

The roots of the herb bennet. (Geum urbanum.)

As there are several species of the herb bennet, I have mentioned the botanical name of that which should be preferred for medical purposes.

It is pretended that the word *bennet* is derived from *benedictum*, (blessed, holy); a name given to this plant by the ancients, on account of the great virtues they attributed to it.

Aqueous decoction of bennet root.

Water wherein bennet root, dried and bruised, has been boiled, acquires a deep brown colour, yielding an aromatic odour: its transparency is disturbed in cooling much more than the willow decoction. In this it resembles more nearly the decoction of quinquina. It is bitter and very acerb to the taste, and feebly reddens the tincture of turnsole.

Habitudes of the decoction with various agents.

Solution of glue causes it to throw down a very abundant precipitate; and the supernatant liquor changes to blue, on the addition of sulphate of iron.

Lime water and water of barytes, when poured into the *decoctum*, cause a flakey precipitate, of a reddish colour, bordering upon violet.

Solid caustic potash proves it contains azote; the quantity of ammoniac disengaged from it is pretty considerable, particularly if the decoction be concentrated. The liquor then assumes a red brown colour.

Carbonates of pot-ash, of ammoniac, and acetate of potash, added to the *decoctum* of bennet root, produce very abundant precipitates.

Muriate and oxalate of ammoniac cause but slight deposits.

Sulphate of iron is precipitated of a beautiful blue colour, the supernatant liquor always preserves this tint, but not so deep; it undergoes no change on the addition of *solutum* of glue.

Several other metallic solutions are also decomposed by it such as nitrates of silver and of mercury, sulphate of copper and acetate of lead.

Th

The deposit caused by antimonial tartrite of potash is so abundant, that there is reason to believe all the metallic salt is decomposed. The supernatant liquor is without colour: hydrosulphuret of pot-ash, added in whatever proportion, causes no red precipitate.

Habitudes of the decoction with various agents.

The liquor, separated from the deposit, and filtered, no longer possesses its bitter nor acerb flavor; it reddens tincture of turnsole more deeply than the solution of tartar emetic. It still gives a precipitate with sulphate of iron, but of a green colour instead of blue; and is not affected with *solutum* of glue.

From these experiments, it may be concluded, that the extractive colouring, resinous, and tannin matter is what causes the acerb and bitter flavor, and combines with the oxide of antimony; and that the substance which remains in the liquor, and gives a green colour with sulphate of iron, is a particular acid.

M. Vauquelin attributes this effect in quinquina, rhubarb, and root of calaguala (the latter of which he has just examined), to the resin contained in these substances: but, I think, that the green colour with sulphate of iron, may be attributed to a modification of the gallic acid.

This acid, so modified, exists in a number of vegetables, which contain tannin, as is proved in my researches on this substance: it is found in the catecher, the arnica, and many other vegetables, ranked among tanning matters.

The extract obtained by evaporating the *decoctum* of bennet root, is so analogous in its characteristics, to those of quinquina, that much experience is necessary to enable us to distinguish one from the other.

Extract of bennet root.

If lime be thrown into a concentrated solution of this extract, a disengagement of ammonia takes.

Alcohol also acts upon this root, and received from it a brownish tint, but not quite so deep as that which it acquires from good quinquina. Its taste is bitter and acrid, water disturbs its transparency, and it reddens the tincture of turnsole.

Alcoholic tincture of bennet root.

Lime water causes a more abundant precipitation than with the alcoholic tincture of quinquina, a circumstance which proves this root to contain more tannin and gallic acid, but a little less resin than the true quinquina.

The solution of tartar emetic, is equally decomposed by this tincture. Sulphate of iron also gives from it a fine black precipitate

cipitate, whose colour may be rendered more intense, by the addition of a few drops of oxigenated muriatic acid.

Presumption. The willow, bark, and bennet root, resemble quinquina.

The experiments above reported on willow bark and bennet root, prove the identity of these substances with quinquina of the best quality.

A simple comparison will determine our ideas on this subject.

It would be nugatory here to detail the comparative experiments made upon quinquina and the barks sold under that name; the latter are so far from possessing its characteristic and chemical properties, that too much care cannot be taken to avoid them; and I am at a loss to conceive why means have not been adopted to prevent their sale as medicinal articles.

Statement of the facts observed in quinquina.

The following are the most striking phenomena which I have observed in quinquina:

The decoction precipitates glue, is decomposed by alcaline carbonates, renders the emetic solution turbid, and gives a green precipitate with sulphate of iron.

Decoctions of white willow bark, and of bennet root, present similar phenomena, except that bennet root yields a blue precipitate with sulphate of iron.

The alcoholic tincture of quinquina, differs from those of willow and bennet root, only in possessing a deeper colour.

Aqueous and dry extract of quinquina seems to me to present similar characters with willow and bennet: that of willow, however, attracts less moisture from the air.

It has a little more resin than the two drugs before examined.

It is evident, therefore, that the only difference consists in a triple more of resin, which varies according to the species of quinquina, and the method of making the extract. What is now called in commerce, good quinquina, differs but very little from these two substances, particularly bennet root.

We may conclude, that these indigenous vegetables contain, like quinquina, chiefly tannin, a colouring extractive matter, resin, and an acid, which I suppose to be a modified gallic acid in willow, quinquina, and the other substances above-mentioned, whilst it is gallic acid in the root of the herb bennet.

This comparison is not a regular analysis.

It may be observed, by the foregoing exposition, that my object has not been to enter into a regular analysis of these

two substances, which could have rendered no service to the art of healing; but to ascertain by comparative experiments, whether the properties already attributed to willow bark and bennet root are well founded, in order to induce my colleagues to apply them in practice. Should the society think this object worthy its attention, I would recommend that, besides the appointment of a committee to examine and report, it should engage its members to employ these two substances, and to communicate to it their observations. Several physicians have already prescribed the decoction, in the manner following:

Prescription of the willow bark or bennet root.

Take root of the herb bennet, or bark of white willow, dried and bruised, one ounce, boil it in a pint and a half (*trois chopines*) of water, to the reduction of 12 ounces.

Add muriate of ammonia from half a drachm to a drachm, and syrup of orange peel, one ounce. A glass, (probably two ounces) to be taken every hour.

I do not know if either of these substances have been used either in the powder, or with opiates, or by infusion in wine, or by the alcoholic tincture mixed with wine. It would likewise be interesting to ascertain what effects would result from the external application of the decoction, or of its other preparations, in cases where quinquina is usually prescribed.

The root is perhaps preferable. Facts and observations.

Were I permitted to deliver an opinion upon the medicinal virtues of the herb bennet, I should be disposed to ascribe the febrifuge quality to it in a greater degree than to the willow bark; for there are few substances whose chemical characters have more analogy to those of quinquina: the same observation may be made respecting the bark of the Indian chesnut-tree, (marronier d'Inde)*.

* The Society of Medicine, after hearing this Memoir read, appointed Messrs. Lafisse, Emennot, Double, Deguise, and Desfigennettes, to make comparative trials of the administration of white willow bark, root of the herb bennet, and quinquina. They were expressly commissioned to examine, with a view to verify, the febrifuge, tonic, and even antiseptic qualities, which Stoll, Cullen, Will, Gunz, Buchave, and other Danish physicians, have attributed to these substances, even in preference to quinquina, according to some of their observations.

(*Note of M. Sedillot, Secretary General to the Society of Medicine.*)

Explanation

VII.

Explanation of Timekeepers, constructed by the late MR. JOHN ARNOLD; *for which a Reward of £3000 was given by the Board of Longitude to his Son* MR. J. R. ARNOLD. *Extracted from the Account delivered by the latter to the Commissioners.**

The measuring parts in a timepiece are the balance spring, the balance, and the escapement.

I CONSIDER the chronometrical part of the timekeeper to be confined altogether to the balance spring, the balance, and the escapement. The other parts are no more than a good horizontal movement, which may be of any dimensions from two inches and a half in diameter, to five or more, and of proportionable depth, and may be constructed to go a day, a week, a month, or even a year (though the last may not be quite so well) at the option of the maker.

Suspension of the balaece on its spring.

[About three pages next following are employed on a description of the train and construction of the box timekeeper; in which there is no singularity asserted, except that the spring of the balance, which is helical, is made to exert a power endways or edgewise, to such a degree, as very nearly to support the weight of the balance when quiescent, and actually to cause its upper pivot to press against the potence, when the re-action of the spring is near its maximum. By this means, as the writer observes, a considerable degree of friction is avoided.]

Of the Balance Spring, with the Mode of rendering it Isochronal, or of adjusting the long and short Arcs of Vibration of the Balance. The Terms long Arcs, and short Arcs, large Arcs, and small Arcs, are used indifferently.

Balance spring: helical, and of steel or gold;

The balance spring may be made of steel wire hardened and tempered, of steel wire hard rolled, or of gold wire alloyed with copper. Steel wire hardened and tempered is the most

* Dated March 5, 1805. The words of the writer are retained, and the paper no otherwise shortened than by omitting what refers to the movement. *N.*

most elastic—then gold, and lastly, steel wire hard drawn. Springs composed of either the above substances, if the materials be good, will answer the purpose. The quantity of copper alloy put to the gold, has been found to answer in the proportion of from one eighth to a quarter, and many other proportions may probably do as well. The form of the spring is helical, or cylindrical, except for a portion of the turn at each end, where it is curved in, and fastened at an equal distance between its centre and circumference, see Plate II. Fig. 2. Were not those turns to be curved inwards, but left of the same diameter with the others, the spring would not have its present easy, concentric motion, but on the contrary, would jolt, wabble, and be distorted. Whether the balance vibrates an arc of 230 degrees from its point of rest in its forward direction, and re-vibrates 230 degrees in its backward direction, making together 460 degrees, the cylindrical figure of the spring is still preserved.

—the ends are turned in.

Upon the length of this spring depends the isochronism of the vibrations of the balance, and in every spring of a sufficient length, there is a place where all the vibrations, long, short, and intermediate, will be performed in equal times.

Isochronism depends on the length.

When the timekeeper is first set going, and always immediately after cleaning and putting into good order; the main spring pulling with all its force, the oil applied to the pivots clean and good, and every part performing its functions to the greatest advantage; the balance may vibrate from 180 to 230 degrees from the point of rest, according to the power of the main spring, and the relative weight of the balance. The balance also re-vibrates on the other side of the point of rest nearly the same arc, but here we only reckon the vibration on one side.

Semi-vibrations from 180° to 230°.

From continual exertion, the main spring will undergo some diminution of its original power, and very great resistance will be created from the thickening of the oil, and from the accumulation of dirt, so that at the end of a long voyage, suppose three or four years, the arc of vibration of the balance will gradually decrease from 230 to probably 130 degrees, and so on, till in time it will come to rest. From which it must be evident that if the different arcs from 230 to 130 are not all performed in equal times, a great irregu-

They fall off.

arity

tract or become smaller by cold, and instead of compensating the error of the spring, it would add to it.)

Adjustment of the balance by moving the pieces at the ends of the arms.

The balls *gg*, being made of equal weight, may be placed at the end of the taps at *c*, and if the timekeeper, being in a situation where the thermometer will rise to 100 degrees or more, should go faster than when placed in another situation where the thermometer will fall to 32 degrees or lower, it is a proof that the expansion pieces do too much, and that the balls are too heavy. Supposing this to be the case, screw the balls up close to the ends of the expansion pieces at *d*, and their effect will be less; because, notwithstanding the same degree of heat will occasion the expansion pieces to move inwards, the same quantity, or to describe the same angle from *c*, yet the balls will move through less space at *d* than at *c*. For it is evident, that if they could slide up to the ends of the expansion pieces, next to the arms of the balance, they would not move at all, or, at least, their motion could not be discovered by any effect that it would produce. If the timekeeper still gains in heat, reduce the balls, and screw them home again to *d*. In the next trial, should it lose in heat more than in cold, contrary to what it did before, it is a proof that the expansion pieces do not do enough, and the balls must be unscrewed toward the ends of the taps at *c*, until it keeps the same time in heat as in cold. If the balls being at the ends do not do enough, and the timekeeper still loses in heat, increase their size until the adjustment is brought within the compass of the length of the taps, where there is generally room sufficient to correct for a minute of difference in heat and cold per day. By screwing the balls up and down, it may be soon seen how much of error two or three turns will correct in a given time, and by that means discover their proper situation.

Of Positions, or the Mode of adjusting the Timekeeper to go alike, or nearly so, in different Positions.

Method of adjusting the balance so as to make equal vibrations in all positions.

The long and short vibrations being adjusted, and also the heat and cold, I shall next shew how to adjust the different positions. Let us suppose that the two mean time screws *hh*, Plate III, Fig. 1, when the balance is at rest, stand at those points

points where the hours 12 and 6 are marked upon the dial plate, and that the two side screws *ff*, stand at those points where the hours 9 and 3 are marked. If the timekeeper should go faster with the hour 12 highest (or vertical) than with the hour 6 highest, screw in the screw *h* a little at the hour 6, and unscrew the opposite screw at the hour 12, the same quantity, if it should lose most in that position, do just the contrary. The same rule is to be observed with respect to the hours 9 and 3, by the two side screws *ff*. It may however happen that the balance will not preponderate at either of these four points, or that the screws may not be sufficiently powerful to effect the purpose. In this case for the positions 12 and 6, by unscrewing a little one of the balls gg, and screwing in the other, we may succeed; but this method should not be practised in superior timekeepers, because by so doing, it will occasion one expansion piece to act more than it ought to do, and the other less, and destroy that equality of expansion, of weight, and of distance, which the very word *Balance* informs us ought to be preserved. To remedy this inconvenience, another method has been contrived, by which the balance may be rendered of equal weight while the balls, the screws, and every opposite part, are at equal distances from the centre. Let the balance be made with a light ring *aa* (as in Fig. 1, Pl. II.) within the expansion pieces. Let there be three light equal weights *kkk*, which by a screw in each may be fixed upon any part of the ring; then having adjusted the long and short vibrations, and the heat and cold, and having the mean time screws at equal distances from the centre, and the balls at equal distances upon the taps (there will be no occasion for side screws), try the timekeeper in different positions, and in a very few trials, by moving the weights upon different parts of the ring the positions may be adjusted very accurately. The weights may be brought all to the same part, and the balance made to preponderate in any given point, and none of the other adjustments will be affected by it, and the weights, upon whatever parts of the circle they may be, will still remain at an equal distance from the centre. Having adjusted for long and short vibrations, heat and cold, and positions, it remains only to regulate for mean time. Should the timekeeper gain, increase the diameter of the balance by drawing

The rule here given seems to be,—shift the centre of gravity towards that part which when highest gives the quickest rate.

out

out an equal quantity of the two mean time screws *hh*, and should it lose, decrease the diameter, by screwing in an equal quantity of the same screws. This adjustment does not affect for that heat and cold, because these screws are unconnected with the expansion pieces, nor will they affect the positions, if they are both turned the same quantity, and the taps of the same thread.

Of the Escapement.

Description and effects of the detached escapement as constructed by Arnold.

Fig. 5, represents the escapement wheel, [supposed to be urged to motion by the train] the teeth of which are of a cycloidal shape, and whose upper surface towards the extremity presents to the view a triangular form, two sides of which are described by right lines, and the other by a cycloidal curve, which is the principal part of action.

In this plan the whole of the escapement wheel is thrown open to view; *BBd* the escapement or locking spring, screwed fast by its end *C* to the pillar *D*, and extending from *C* to *d* in the direction *CBNBd*. The centre of motion of this spring is between *C* and *N*, the part *NBd* being more substantial than the part *CBN*, and into which part between *N* and *B* is fixed the locking piece *a*, the locking piece or locking pallat, whose acting surface is a jewel (see also Fig. 4,) placed between *N* and *B* and opposite the end of an adjusting screw *F*, which pallat descending from the escapement spring, locks upon the interior angle of the tooth 2, and upon every tooth in succession, suspending the motion of the escapement wheel for a time, and leaving the balance to vibrate without interruption from any part of the machinery. It is to be observed, that the triangular parts of the teeth of the wheel *AAA*, the wheel being hollowed or sunk, are raised above the periphery of the wheel to meet the locking piece *a*, so that upon viewing the wheel edgewise, see Fig. 5, the teeth will appear broader than the edge of the periphery *b*. In Fig. 3. the locking pallat *a* being in contact with the tooth 2, is not so well distinguished as in Figures 6, 7, and 8, where it appears very plainly over the periphery *b*, of the wheel in the interval between the teeth 1 and 2.

Fig. 4, gives a view of the escapement spring reversed, and Fig. 9 explains upon a large scale the figure of the locking piece, which is angular, adjoining that part of the straight edge

edge where the locking is effected. Was this angular part to be left square, like the opposite end, it might strike against the interior angle of the tooth, as the escapement spring returns to its place against the adjusting screw, after having unlocked or discharged the wheel, but by being of this figure it clears itself.

Description and effects of the detached escapement as constructed by Arnold.

Ne the discharging or unlocking spring, which is attached to the escapement or locking spring at *N*, and passes under the adjusting screw *F*, a little beyond the end *d* of the locking spring. This discharging spring is made very slight and delicate.

F the adjusting screw supported by the pillar *g*, whose end is opposite to the locking piece or pallat *a*, on the contrary side of the escapement or locking spring, and by which the locking piece or pallat *a*, may be more or less advanced upon any tooth of the wheel; the escapement spring *BBd* always pressing the locking pallat *a*, against the end of the screw *F*, except at the time of unlocking the wheel.

o, The unlocking, discharging or small pallat, whose part of action is a jewel. This pallat when the balance is in motion, presses against the end of the discharging spring at *e*, and passing on in a direction from *e* to *d*, carries with it for a short space, the discharging spring *Ne*, and also the locking spring *BBd*, moving them both at the same time, and in so doing carries the locking piece *a*, from off the interior angle of the tooth 2, (or any other tooth which may come into that situation) and leaves the wheel at liberty to impart its power to the impelling pallat. But when the balance returns, and the unlocking pallat *o* repasses the discharging spring *Ne* in a direction from *d* to *e*, it does not in the least disturb the locking spring *BBd*, nor consequently the locking piece or pallat *a*, but moves only, and for a short space, the unlocking spring *Ne*. *HHH* The impelling or large pallat, whose part of action is at the angle *m*, where a jewel is placed. Upon the exterior of this angle the pallat receives its impulse from the cycloidal part of the tooth of the escapement wheel. The circumference of the pallat is incomplete from a portion being cut away to make room for the action of the teeth of the wheel.

X. is a circular hole under the periphery *b*, of the escapement wheel, over the centre of which the tooth 2 appears, and

Description and effects of the detatched escapement as constructed by Arnold.

and the locking piece or pallat *a*. The two springs *BM* and *Ne* pass over it. This hole is made through the brass plate *QQQQ*, through which by inverting the escapement the manner in which the locking piece *a* holds the tooth 2 (or any other tooth in succession) of the escapement wheel may be seen.

The impelling pallat *HHHH* is supposed to be vibrating freely from *r* to *S*; here it is perfectly detatched, or at liberty from the escapement wheel; as will be seen by observing that the unlocking pallat *o* is not in contact with the discharging spring *Ne*, nor are either of the teeth 3 or 4 of the wheel in contact with the impelling pallat. The balance or the impelling pallat (for they are both upon the same axis) vibrating from *r* to *S*, the discharging pallat *o* comes in contact with the discharging spring *Ne*, see Fig. 6, (and from that instant it is not detached or free, but in the act of escaping) and moving it in the direction from *e* to *d*, takes the locking pallat *a* from off the interior angle of the tooth 2, and sets the escapement wheel at liberty for the tooth 3 to act upon the angle *m* of the impelling pallat. Here the tooth 2 will be seen to have passed the locking piece *a*, and the tooth 1 to approach it. The tooth 3 pressing the impelling pallat from *r* to *S*, and continuing to do so as in Fig. 7, where the centre of the escapement wheel, the angle *m*, of the impelling pallat, and the centre *G* of both pallats form a straight line. This action of the wheel upon the pallat continuing as in Fig. 8, where the point of the tooth 3 is about to quit the angle *m* of the impelling pallat, the tooth 2 approaching nearer to the circumference of the same pallat, and the tooth 1 advancing toward the locking piece or pallat *a*, against which it falls, and is held fast, as soon as the end of the tooth 3 quits the impelling pallat. Here the act of escaping ends, and the impelling pallat is again detatched or unconnected with the wheel, and moves in free vibration as in Fig. 5, for a certain number of degrees, until it is returned by the power of the balance spring and repasses from *S* to *r*, still independent of the escapement wheel or of any thing else, except the very little resistance which is encountered by the discharging pallat *o*, in repassing the discharging spring *Ne*, which it does without disturbing the locking piece *a*, or consequently the escapement wheel, and con-

continuing for a certain number of degrees, is again returned by the balance spring from r to S, when resuming its situation, it is prepared to act as before.

Description and effects of the detatched escapement as constructed by Arnold.

It may be proper to remark that the action of the cycloidal tooth upon the impelling pallat is always the same, in the beginning as in Fig. 6, in the middle as in Fig. 7, and at the end as in Fig. 8, impelling the pallat with the same quantity of surface in action at all times, and at all times equidistant from the axis of the pallat. This however depends upon the tooth having the true figure.

The proper shape of the cycloid is found in the following manner. Having a plate of smooth metal, fix upon it a piece of brass the size of the intended escapement wheel, which call the false wheel. Then take another piece of brass the size of the intended pallat, which call the false pallat. On the circumference of the false pallat fix a fine steel point, and then rolling the false pallat upon the circumference of the false wheel, the steel point will describe a line on the plate, which will be the proper curve, in which shape the tooth must be cut by an engine. The larger the pallat in proportion to the escapement wheel, the less sudden the cycloidal curve will be, and the smaller the pallat the more sudden; so that an escapement wheel which has 15 teeth, with a pallat of a proportionable diameter, will have its teeth of a very different shape to those in a wheel which has only 12 teeth, because, in one case, the pallat is half the size, and in the other it is little more than one third.

The size of the pallat depends upon the number of teeth in the escapement wheel. The radius of the pallat should be equal to the distance between any two teeth of the wheel, and then their relative motion will be equal. If the wheel has twelve teeth, the radius of the pallat will be thirty degrees, measured on the diameter of the wheel, and its diameter sixty degrees, measured in the same manner, which will make it half the size of the wheel. If it has thirteen teeth the pallat will in diameter measure fifty-five degrees and a half. If fourteen teeth, fifty-one degrees and a half; and if fifteen teeth, which is the number generally applied to pocket time-keepers, it will be forty-eight degrees.

Quickness of the train of the timekeepers.

The Marine Timekeeper, which has been here described, is

Description and effects of the detatched escapement as constructed by Arnold.

is made to beat half seconds, the balance making 240 vibrations both ways in a minute. For if the balance wh[illegible] has 15 teeth, the fourth wheel 80 teeth, and the balan[illegible] pinion 10 teeth, there will be 120 beats, or half seconds, i[illegible] one minute.

It is also made with the escapement wheel of 12 teet[illegible] the balance pinion having 7, and the fourth wheel 70; con[illegible]sequently there will be 120 beats, or half seconds, in o[illegible] minute as before. It has been already remarked that th[illegible] pallat for 12 teeth must be half the diameter of the wheel[illegible] and for 15 teeth five-twelfths, or fifty degrees.

The pocket timekeepers, that they may not be disturbed by motion, have what is called a quicker train, the second hand making 150 beats upon the dial, or 5 beats in tw[illegible] seconds. The escapement wheel has 15 teeth, the balanc[illegible] pinion 8 teeth, and the fourth wheel 80, consequently ther[illegible] will be 150 beats in one minute. The pallat being 50 degree[illegible] in diameter, measured upon the diameter of the balanc[illegible] wheel.

No mention has been made of the numbers of the teet[illegible] in the other wheels and pinions, as they are of little or n[illegible] importance, and may be varied considerably.

VIII.

An Essay on the Cohesion of Fluids. By Thomas Young, M. D. For. Sec. R.S.*

I. *General Principles.*

General principles of the cohesion of fluids.

It has already been asserted, by Mr. Monge and others, th[illegible] the phenomena of capillary tubes are referable to the cohesiv[illegible] attraction of the superficial particles only of the fluids em[illegible]ployed, and that the surfaces must consequently be form[illegible] into curves of the nature of lintearia, which are supposed to b[illegible] the results of a uniform tension of a surface, resisting the pre[illegible]sure of a fluid, either uniform, or varying according to a gi[illegible] law. Seguer, who appears to have been the first that mainta[illegible]

* Philos. Trans. 1805.

a sim[illegible]

From the Model of Mr Arnolds Escapement

Fig 10

Fig 7

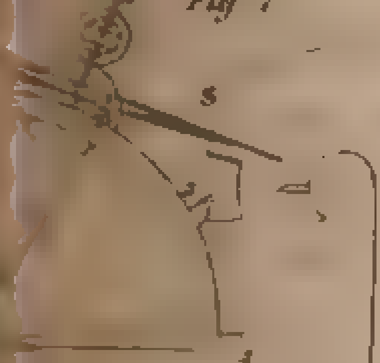


Fig 6

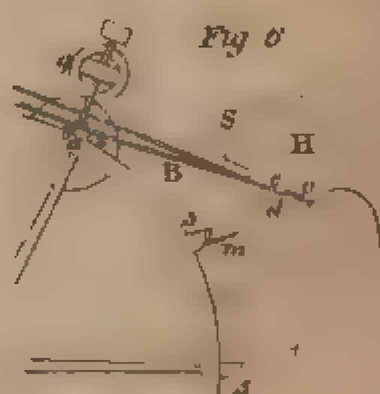


Fig 2

Fig 8

Fig 9

Fig 3

Fig 4

a similar opinion, has shown in what manner the principle may be deduced from the doctrine of attraction, but his demonstration is complicated, and not perfectly satisfactory; and in applying the law to the forms of drops, he has neglected to consider the very material effects of the double curvature, which is evidently the cause of the want of a perfect coincidence of his experiments with his theory. Since the time of Segner, little has been done in investigating accurarely and in detail the various consequences of the principle.

General principles of the cohesion of fluids.

It will perhaps be most agreeable to the experimental philosopher, although less consistent with the strict course of logical argument, to proceed in the first place to the comparison of this theory with the phenomena, and to inquire afterwards for its foundation in the ultimate properties of matter. But it is necessary to premise one observation, which appears to be new, and which is equally consistent with theory and with experiment; that is, that for each combination of a solid and a fluid, there is an appropriate angle of contact between the surfaces of the fluid, exposed to the air, and to the solid. This angle, for glass and water, and in all cases where a solid is perfectly wetted by a fluid, is evanescent: for glass and mercury, it is about 140° in common temperatures, and when the mercury is moderately clean.

II. *Form of the Surface of a Fluid.*

Form of the surface of a fluid, as modified by the cohesion of its parts, &c.

It is well known, and it results immediately from the composition of forces, that where a line is equally distended, the force that it exerts, in a direction perpendicular to its own, is directly at its curvature; and the same is true of a surface of simple curvature; but where the curvature is double, each curvature has its appropriate effect, and the joint force must be as the sum of the curvatures in any two perpendicular directions. For this sum is equal, whatever pair of perpendicular directions may be employed, as is easily shown by calculating the versed sines of two equal arcs taken at right angles in the surface. Now when the surface of a fluid is convex externally, its tention is produced by the pressure of the particles of the fluid within it, arising from their own weight, or from that of the surrounding fluid; but when the surface is concave, the tension is employed in counteracting the pressure of the

Form of the surface of a fluid, as modified by the cohesion of its parts, &c.

the atmosphere, or, where the atmosphere is excluded, the equivalent pressure arising from the weight of the particles suspended from it by means of their cohesion, in the same manner as, when water is supported by the atmospheric pressure in an inverted vessel, the outside of the vessel sustains a hydrostatic pressure proportionate to the height; and this pressure must remain unaltered, when the water, having been sufficiently boiled, is made to retain its situation for a certain time by its cohesion only, in an exhausted receiver. When, therefore, the surface of the fluid is terminated by two right lines, and has only a simple curvature, the curvature must be every where as the ordinate; and where it has a double curvature, the sum of the curvatures in the different directions must be as the ordinate. In the first case, the curve may be constructed by approximation, if we divide the height at which it is either horizontal or vertical into a number of small portions, and taking the radius of each portion proportional to the reciprocal of the height of its middle point or below the general surface of the fluid, go on to add portions of circles joining each other, until they have completed as much of the curve as is required. In the second case, it is only necessary to consider the curve derived from a circular basis, which is a solid of revolution; and the centre of that circle of curvature, which is perpendicular to the section formed by a plane passing through the axis, is in the axis itself, consequently in the point where the normal of the curve intersects the axis: we must therefore here make the sum of this curvature, and that of the generating curve, always proportional to the ordinate. This may be done mechanically, by beginning at the vertex, where the two curvatures are equal, then, for each succeeding portion, finding the radius of curvature by deducting the proper reciprocal of the normal, at the beginning of the portion, from the ordinate, and taking the reciprocal of the remainder. In this case the analysis leads to fluxional equations of the second order, which appear to afford no solution by means hitherto discovered; but the cases of simple curvature may be more easily subjected to calculation.

III. *Analysis of the simplest Forms.*

On the simplest forms of the surface of a fluid.

Supposing the curve to be described with an equable angular

lar velocity, its fluxion, being directly as the radius of curvature, will be inversely as the ordinate, and the rectangle contained by the ordinate and the fluxion of the curve will be a constant quantity; but this rectangle is to the fluxion of the area, as the radius to the cosine of the angle formed by the curve with its fluxion; and the fluxion of the area varying as the cosine, the *area* itself will vary as the sine of this angle, and *will be equal to the rectangle contained by the initial ordinate, and the sine corresponding to each point of the curve in the initial circle of curvature.* Hence it follows first, that *the whole area included by the or dinates wherethe curve is vertical and where it is horizontal, is equal to the rectangle contained by the ordinate and the radius of curvature;* and, secondly, that the area on the convex side of the curve, between the vertical tangent and the least ordinate, is equal to the whole area on the concave side of the curve between the same tangent and the greatest ordinate. On the simple forms of the surface of a fluid.

In order to find the ordinate corresponding to a given angular direction, we must consider that the fluxion of the ordinate at the vertical part, is equal to the fluxion of the circle of curvature there, that, in other places, it varies as the radius of curvature and the sine of the angle formed with the horizon conjointly, or as the ordinate inversely, and directly as the sine of elevation; therefore the fluxion of the ordinate multiplied by the ordinate is equal to the fluxion of any circle of curvature multiplied by its corresponding height, and by the sine, and divided by the radius: but the fluxion of the circle multiplied by the sine and divided by the tadius is equal to the fluxion of the versed sine; therefore the ordinate multiplied by its fluxion is equal to the initial height multiplied by the fluxion of the versed sine in the corresponding circle of curvature; *and the square of the ordinate is equal to the rectangle contained by the initial height and twice the versed sine, increased by a constant quantity.* Now at the highest point of the curve, the versed sine becomes equal to the diameter, and the square of the initial height to the rectangle contained by the initial height and twice the diameter, with the constant quantity: the constant quantity is therefore equal to the rectangle contained by the initial height and its difference from twice he diameter: *this constant quantity is the square of*

Of the simplest forms of the surface of a fluid.

of the least ordinate, and the ordinate is every where a mean proportional between the greatest height and the same height diminished by twice the versed sine of the angular depression in the corresponding circle of curvature. Again, *at the vertical point the square of the ordinate is equal to the square of the greatest height diminished by the rectangle contained by this height and the diameter of the correcting circle of curvature*, a rectangle which is constant for every fluid, and which may be called *the appropriate rectangle: deducting this rectangle from the square of the ordinate at the vertical point, we have the least ordinate*; which consequently vanishes *when the square of the ordinate at the vertical point is equal to the appropriate rectangle; the horizontal surface becoming in this case an asymptote to the curve, and the square of the greatest ordinate being equal to twice the appropriate rectangle, and the greatest ordinate to twice the diameter of the corresponding circle of curvature*; so that, *if we suppose a circle to be described, having this ordinate for a diameter, the chord of the angular elevation in this circle will be always equal to the ordinate at each point, and the ordinate will vary as the sine of half the angle of elevation whenever the curve has an asymptote.* Mr. Fuss has demonstrated, in the third volume of the *Acta Petropolitana*, some properties of the arch of equilibrium under the pressure of a fluid, which is the same as one species of the curves here considered. The series given by Euler in the second part of the same volume, for the elastic curve, may also be applied to these curves.

IV. *Application to the Elevation of particular Fluids.*

Application of the doctrine to particular fluids.

The simplest phenomena, which afford us data for determining the fundamental properties of the superficial cohesion of fluids, are their elevation and depression between plates and in capillary tubes, and their adhesion to the surfaces of solids which are raised in a horizontal situation to a certain height above the general surface of the fluids. When the distance of a pair of plates, or the diameter of a tube, is very minute, the curvature may be considered as uniform, and the appropriate rectangle may readily be deduced from the elevation, recollecting that the curvature in a capillary tube is double, and the height therefore twice as great as between two plates.

In

Application of the doctrine to particular fluids.

In the case of the elevation of a fluid in contact with a horizontal surface, the ordinate may be determined from the weight required to produce a separation; and the appropriate rectangle may be found in this manner also, the angle of contact being properly considered, in this as well as in the former case. It will appear that these experiments by no means exhibit an immediate measure of the mutual attraction of the solid and fluid, as some authors have supposed.

Sir Isaac Newton asserts, in his Queries, that water ascends between two plates of glass at the distance of one hundredth of an inch, to the height of about one inch; the product of the distance and the height being about .01; but this appears to be much too little. In the best experiment of Musschenbroek, with a tube, half of the product was .0195; in several of Weitbrecht, apparently very accurate, 0214. In Monge's experiments on plates, the product was 2.6 or 2.7 lines, about .0210. Mr. Atwood says that for tubes, the product is .0530, half of which is .0265. Untill more accurate experiments shall have been made, we may be contented to assume .02 for the rectangle appropriate to water, and .04 for the product of the height in a tube by its bore. Hence, when the curve becomes infinite, its greatest ordinate is .2, and the height of the vertical portion, or the height of ascent against a single vertical plane .14, or nearly one-seventh of an inch.

Now when a horizontal surface is raised from a vessel of water, the surface of the water is formed into a lintearia to which the solid is a tangent at its highest point, and if the solid be still further raised, the water will separate: the surface of the water, being horizontal at the point of contact, cannot add to the weight tending to depress the solid, which is therefore simply the hydrostatic pressure of a column of water equal in height to the elevation, in this case one-fifth of an inch, and standing on the given surface. The weight of such a column will be 50 grains and a half for each square inch; and in Taylor's well known experiment the weight required was 50 grains. But when the solid employed is small, the curvature of the horizontal section of the water, which is convex externally, will tend to counteract the vertical curvature, and to diminish the height of separation; thus if a disc of an inch in diameter were employed, the curvature in this direction would

Application of the doctrine to particular fluids.

would perhaps be equivalent to the pressure of about one hundredth of an inch, and might reduce the height from .2 to about 19, and the weight in the same proportion. There is, however, as great a diversity in the results of different experiments on the force required to elevate a solid from the surface of a fluid, as in those of the experiments in capillary tubes: and, indeed, the the sources of error appear to be here more numerous. Mr. Achard found that a disc of glass, one inch and a half French in diameter, required, at 69° of Fahrenheit, a weight of 91 French grains to raise it from the surface of the water; this is only 37 English grains for each square inch; at 44½° the force was $\frac{1}{14}$ greater, or 39 grains and a half; the difference being $\frac{1}{341}$ for each degree of Fahrenheit. It might be inferred from these experiments, that the height of ascent in a tube of a given bore, which varies in the duplicate ratio of the height of adhesion, is diminished about $\frac{1}{180}$ for every degree of Fahrenheit that the temperature is raised above 50°; there was, however, probably some considerable source of error in Achard's experiments, for I find that this diminution does not exceed $\frac{1}{1000}$. The experiments of Mr. Dutor make the quantity of water raised equal to 44.1 grains for each square inch. Mr. Achard found the force of adhesion of fulfuric acid to glass, at 69° of Fahrenheit, 1.26, that of water being 1, hence the height was as 69 to 1, and its square as .47 to 1, which is the corresponding proportion for the ascent of the acid in a capillary tube, and which does not very materially differ from the proportion of 395 to 1, assigned by Barruel for this ascent, Musschenbroek found it .8 to 1, but his acid was probably weak. For alcohol the adhesion was as .593, the height as .715, and its square as .510; the observed proportion in a tube, according to an experiment of Musschenbroek, was about .550, according to Carré from .400 to .440. The experiments on sulfuric ether do not agree quite so well, but its quality is liable to very considerable variations. Dutour found the adhesion of alcohol .58, that of water being 1.

With respect to mercury, it has been shown by Professor Casbois of Metz, and by others, that its depression in tubes of glass depends on the imperfection of the contact, and that when it has been boiled in the tube often enough to expel all foreign

foreign particles, the surface may even become concave instead of convex, and the depression be converted into an elevation. But in barometers, constructed according to the usual methods, the angle of the mercury will be found to differ little from 140°; and in other experiments, when proper precautions are taken, the inclination will be nearly the same. The determination of this angle is necessary for finding the appropriate rectangle for the curvature of the surface of mercury, together with the observations of the quantity of depression in tubes of a given diameter. The table published by Mr. Cavendish from the experiments of his father, Lord Charles Cavendish, appears to be best suited for this purpose. I have constructed a diagram, according to the principles already laid down, for each case, and I find that the rectangle which agrees best with the phenomena is .01. The mean depression is always .015, divided by the diameter of the tube: and in tubes less than half an inch in diameter, the curve is very nearly elliptic, and the central depression in the tube of a barometer may be found by deducting from the corresponding mean depression the square root of one-thousandth part of its diameter. There is reason to suspect a slight inaccuracy towards the middle of Lord Charles Cavendish's Table, from a comparison with the calculated mean depression, as well as from the results of the mechanical construction. The ellipsis approaching nearest to the curve may be determined by the solution of a biquadratic equation.

Application of the doctrine to particular fluids.

Diameter in inches.	Grains in an inch. C.	Mean depression by calculation. Y.	Central depression by observation. C.	Central depression by formula. Y.	Central depression by diagram. Y.	Marginal depression by diagram. Y.
.6	972	.025	.005	(.001)	.005	.066
.5	675	.030	.007	.008	.007	.067
.4	432	.037	.015	.017	.012	.069
.35	331	.043	.025	.024	.017	.072
.30	243	.050	.036	.033	.027	.079
.25	169	.060	.050	.044	.038	.086
.20	108	.075	.067	.061	.056	.096
.15	61	.100	.092	.088	.085	.116
.10	27	.150	.140	.140	.140	.161

The square root of the rectangle .01, or .1, is the ordinate where the curve would become vertical if it were continued; but in order to find the height at which it adheres to a vertical surface,

Application of the doctrine to particular fluids.

surface, we must diminish this ordinate in the proportion of the sine of 25° to the sine of 45°, and it will become .06, for the actual depression in this case. The elevation of the mercury that adheres to the lower horizontal surface of a piece of glass, and the thickness at which a quantity of mercury will stand when spread out on glass, supposing the angle of contact still 140°, are found, by taking the proportion of the sines of 20° and of 70° to the sine of 45°, and are therefore .0484 and .1330 respectively. If, instead of glass, we employed any surface capable of being wetted by mercury, the height of elevation would be .141, and this is the limit of thickness of a wide surface of mercury supported by a substance wholly incapable of attracting it. Now the hydrostatic pressure of column of mercury .0484 in thickness on a disc of one inch diameter would be 131 grains; to this the surrounding elevation of the fluid will add about 11 grains for each inch of the circumference, with some deduction for the effect of the contrary curvature of the horizontal section, tending to diminish the height; and the apparent cohesion thus exhibited will be about 160 grains, which is a little more than four times as great as the apparent cohesion of glass and water. With a disc 11 lines in diameter Mr. Dutour found it 194 French grains, which is equivalent to 132 English grains, instead of 160, for an inch, a result which is sufficient to confirm the principles of the calculation. The depth of a quantity of mercury standing on glass I have found by actual observation, to agree precisely with this calculation. Segner says that the depth was .1358, both on glass and on paper: the difference is very trifling, but this measure is somewhat too great for glass, and too small for paper, since it appears from Dutour's experiments, that the atraction of paper to mercury is extremely weak.

If a disc of a substance capable of being wetted by mercury, an inch in diameter, were raised from its surface in a position perfectly horizontal, the apparent cohesion should be 381 grains, taking .141 as the height: and for a French circular inch, 433 grains, or 528 French grains. Now, in the experiments of Morveau, the cohesion of a circular inch of gold to the surface of mercury appeared to be 446 grains, of silver 429, of tin 418, of lead 397, of bismuth 372, of zinc 204, of copper 142, of metallic antimony 126, of iron 115, of cobalt 8: and this order is the same with that in which the metals

Application of the doctrine to particular fluids.

tals are most easily amalgamated with mercury. It is probable that such an amalgamation actually took place in some of the experiments, and affected their results, for the process of amalgamation may often be observed to begin almost at the instant of contact of silver with mercury; and the want of perfect horizontality appears in slight degree to have affected them all. A deviation of one-fiftieth of an inch would be sufficient to have produced the difference between 446 grains and 528; and it is not impossible that all the differences, as far down as bismuth, may have been accidental. But if we suppose the gold only to have been perfectly wetted by the mercury, and all the other numbers to be in due proportions, we may find the appropriate angle for each substance by deducting from 180°, twice the angle of which the sine is to the radius as the apparent cohesion of each to 446 grains; that is, for gold .1, for silver about 97, for tin .95, for lead .90, for bismuth .85 for zinc. .46, for copper .32, for antimony .29, for iron 26, and for cobalt .02, neglecting the surrounding elevation, which has less effect in proportion as the surface employed is larger. Gellert found the depression of melted lead in a tube of glass multiplied by the bore equal to about .0054.

It would perhaps be possible to pursue these principles so far as to determine in many cases the circumstances under which a drop of any fluid would detach itself from a given surface. But it is sufficient to infer, from the law of the superficial cohesion of fluids, that the linear dimensions of similar drops depending from a horizontal surface must vary precisely in the same ratio as the heights of ascent of the respective fluids against a vertical surface, or as the square root of the heights of ascent in a given tube; hence the magnitudes of similar drops of different fluids must vary as the cubes of the square roots of the heights of ascent in a tube. I have measured the heights of ascent of water and of diluted spirit of wine in the same tube, and I found them nearly as 100 to 64: a drop of water falling from a large sphere of glass weighed 1.8 grains, a drop of the spirit of wine about .85, instead of .82, which is nearly the weight that would be inferred from the consideration of the heights of ascent, combined with that of the specific gravities. We may form a conjecture respecting the probable magnitude of a drop by

inquiring

Application of the doctrine to particular fluids.

inquiring what must be the circumference of the fluid, that would support by its cohesion the weight of a hemisphere depending from it: this must be the same as that of a tube, in which the fluid would rise to the height of one-third of its diameter; and the square of the diameter must be three times as great as the appropriate product; or, for water .12; whence the diameter would be .35, or a little more than one-third of an inch, and the weight of the hemisphere would be 2.8 grains. If more water were added internally, the cohesion would be overcome, and the drop would no longer be suspended, but it is not easy to calculate what precise quantity of water would be separated with it. The form of a bubble of air rising in water is determined by the cohesion of the internal surface of the water exactly in the same manner as the form of a drop of water in the air. The delay of a bubble of air at the bottom of a vessel appears to be occasioned by a deficiency of the pressure of the water between the air and the vessel; it is nearly analogous to the experiment of making a piece of wood remain immersed in water, when perfectly in contact with the bottom of the vessel containing it. This experiment succeeds however far more readily with mercury, since the capillary cohesion of the mercury prevents its insinuating itself under the wood.

V. *Of apparent Attractions and Repulsions.*

On the apparent attractions and repulsions of floating bodies.

The apparent attraction of two floating bodies, round both of which the fluid is raised by cohesive attraction, is produced by the excess of the atmospheric pressure on the remote sides of the solids above its pressure on their neighbouring sides: or, if the experiments are performed in a vacuum, by the equivalent hydrostatic pressure or suction derived from the weight and immediate cohesion of the intervening fluid. This force varies ultimately in the inverse ratio of the square of the distance; for, if two plates approach each other, the height of the fluid that rises between them is increased in the simple inverse ratio of the distance; and the mean action, or negative pressure, of the fluid on each particle of the surface is also increased in the same ratio. When the floating bodies are both surrounded by a depression, the same law prevails, and its demonstration is still more simple and obvious. The repul-

On the apparent attractions and repulsions of floating bodies.

repulsion of a wet and a dry body does not appear to follow the same proportion: for it by no means approaches to infinity upon the supposition of perfect contact; its maximum is measured by half the sum of the elevation and depression on the remote sides of the substances, and as the distance increases, this maximum is only diminished by a quantity, which is initially as the square of the distance. The figures of the solids concerned modify also sometimes the law of attraction, so that, for bodies surrounded by a depression, there is sometimes a maximum, beyond which the force again diminishes: and it is hence that a light body floating on mercury, in a vessel little larger than itself, is held in a stable equilibrium without touching the sides. The reason of this will become apparent, when we examine the direction of the surface necessarily assumed by the mercury in order to preserve the appropriate angle of contact, the tension acting with less force when the surface attaches itself to the angular termination of the float in a direction less horizontal.

The apparent attraction produced between solids by the interposition of a fluid does not depend on their being partially immersed in it; on the contrary, its effects are still more powerfully exhibited in other situations; and, when the cohesion between two solids is increased and extended by the intervention of a drop of water or of oil, the superficial cohesion of these fluids is fully sufficient to explain the additional effect. When wholly immersed in water, the cohesion between two pieces of glass is little or not at all greater than when dry; but if a small portion only of a fluid be interposed, the curved surface, that it exposes to the air, will evidently be capable of resisting as great a force as it would support from the pressure of the column of fluid that it is capable of sustaining in a vertical situation; and in order to apply this force, we must employ in the separation of the plates, as great a force as is equivalent to the pressure of a column appropriate to their distance. Morveau found that two discs of glass, 3 inches French in diameter, at the distance of one-tenth of a line, appeared to cohere with a force of 4719 grains, which is equivalent to the pressure of a column 23 lines in height: hence the product of the height and the distance of the plates is 2.3 lines instead of 2.65, which was the result of Monge's experiments on

On the apparent attractions and repulsions of floating bodies.

on the actual ascent of water. The difference is much smaller than the difference of the various experiments on the ascent of fluids; and it may easily have arisen from a want of perfect parallelism in the plates; for there is no force tending to preserve this parallelism. The error, in the extreme case of the plates coming into contact at one point, may reduce the apparent cohesion to one half.

The same theory is sufficient to explain the law of the force by which a drop is attracted towards the junction of two plates inclined to each other, and which is found to vary in the inverse ratio of the square of the distance; whence it was inferred by Newton that the primitive force of cohesion varies in the simple inverse ratio of the distance, while other experiments lead us to suppose that cohesive forces in general vary in the direct ratio of the distance. But the difficulty is removed by considering the state of the marginal surface of the drop. If the plates were parallel, the capillary action would be equal on both sides of the drop: but when they are inclined, the curvature of the surface at the thinnest part requires a force proportionate to the appropriate height to counteract it; and this force is greater than that which acts on the opposite side. But if the two plates are inclined to the horizon, the deficiency may be made up by the hydrostatic weight of the drop itself; and the same inclination will serve for a larger or a smaller drop at the same place. Now when the drop approaches to the line of contact, the difference of the appropriate heights for a small drop of a given diameter will increase as the square of the distance decreases; for the fluxion of the reciprocal of any quantity varies inversely as the square of that quantity: and, in order to preserve the equilibrium, the sine of the angle of elevation of the two plates must be nearly in the inverse ratio of the square of the distance of the drop from the line of contact, as it actually appears to have been in Hauksbee's experiments.

VI. *Physical Foundation of the Law of superficial Cohesion.*

Law of superficial cohesion.

We have now examined the principal phenomena which are reducible to the simple theory of the action of the superficial particles of a fluid. We are next to investigate the natural foundations upon which that theory appears ultimately to rest. We

Law of superficial cohesion.

We may suppose the particles of liquids, and probably those of solids also to possess that power of repulsion which has been demonstratively shown by Newton to exist in aeriform fluids, and which varies in the simple inverse ratio of the distance of the particles from each other. In airs and vapours this force appears to act uncontrolled; but in liquids, it is overcome by cohesive force, while the particles still retain a power of moving freely in all directions; and in solids the same cohesion is accompanied by a stronger or weaker resistance to all lateral motion, which is perfectly independent of the cohesive force, and which must be cautiously distinguished from it. It is simplest to suppose the force of cohesion nearly or perfectly constant in its magnitude, throughout the minute distance to which it extends, and owing its apparent diversity to the contrary action of the repulsive force, which varies with the distance. Now in the internal parts of a liquid these forces hold each other in a perfect equilibrium, the particles being brought so near that the repulsion becomes precisely equal to the cohesive force that urges them together: but whenever there is a curved or angular surface, it may be found by collecting the actions of the different particles, that the cohesion must necessarily prevail over the repulsion, and must urge the superficial parts inwards with a force proportionate to the curvature, and thus produce the effect of a uniform tension of the surface. For, if we consider the effect of any two particles in a curved line on a third at an equal distance beyond them, we shall find that the result of their equal attractive forces bisects the angle formed by the lines of direction; but that the result of their repulsive forces, one of which is twice as great as the other, divides it in the ratio of one to two, forming with the former result an angle equal to one-sixth of the whole; so that the addition of a third force is necessary in order to retain these two results in equilibrium; and this force must be in a constant ratio to the evanescent angle which is the measure of the curvature, the distance of the particles being constant. The same reasoning may be applied to all the particles which are within the influence of the cohesive force; and the conclusions are equally true if the cohesion is not precisely constant, but varies less rapidly than the repulsion.

VII.

VII. *Cohesive Attraction of Solids and Fluids.*

Cohesive attraction of solids and fluids.

When the attraction of the particles of a fluid for a solid is less than their attraction for each other, there will be an equilibrium of the superficial forces, if the surface of the fluid make with that of the solid a certain angle, the versed sine of which is to the diameter, as the mutual attraction of the fluid and solid particles is to the attraction of the particles of the fluid among each other. For, when the fluid is surrounded by a vacuum or by a gas, the cohesion of its superficial particles acts with full force in producing a pressure; but when it is any where in contact with a solid substance of the same attractive power with itself, the effects of this action must be as much destroyed as if it were an internal portion of the fluid. Thus, if we imagined a cube of water to have one of its halves congealed, without any other alteration of its properties, it is evident that its form and the equilibrium of the cohesive forces would remain undisturbed: the tendency of the new angular surface of the fluid water to contract would therefore be completely destroyed by the contact of a solid of equal attractive force. If the solid were of smaller attractive force, the tendency to contract would only be proportionate to the difference of the attractive forces or densities, the effect of as many of the attractive particles of the fluid being neutralized, as are equivalent to a solid of a like density or attractive power. For a similar reason, the tendency of a fluid to contract the sum of the surfaces of itself and a contiguous solid, will be simply as the density of the solid, or as the mutual attractive force of the solid and fluid. And it is indifferent whether we consider the pressure produced by these supposed superficial tensions, or the force acting in the direction of the surfaces to be compared.

(The conclusion in our next.)

A

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AND

THE ARTS.

JUNE, 1806.

ARTICLE I.

Letter of Inquiry from a Correspondent, respecting the spontaneous Recovery of the Edge in dull Razors laid aside for a time; and a Postscript, shewing that Lavoisier has no title to the Discovery of the Modern Theory of Oxidation: with a Reply and some Remarks by W. N.

To Mr. NICHOLSON.

SIR,

Edinburgh, 10*th May*, 1806.

HAVING often remarked that a razor which by use had lost its edge, and was laid by, recovered it again when newly strapped; I have been in the habit for some time past of putting my razor aside for a short time when it became dull, with the view of improving its power of cutting. I have asked some experienced hair-dressers who are much in the habit of shaving, if they had ever noticed the fact, and received an answer in the affirmative: and on inquiring a few days ago of a surgeons' instrument maker of this place, whether he could tell me the cause, he replied that the same question was more than once put to him by the late Dr. Black, but that he was unable to give any satisfactory answer. If you should deem the observa-

A dull razor recovers its edge by being laid aside for a time.

The same fact noticed by others, and by Dr. Black.

servation worth notice, and are able to give me an adequate explanation, I shall be much obliged by your insertion of this note in some future number. Can it be owing to any chemical action exerted on the fine edge of the metal by the oxigenous part of the atmosphere, whereby its quality is so changed as to cause it to yield more to the strap than it did before?

Question as to cause.

I am, Sir, your constant reader,

E. D.

The editor reminded of his promise respecting Lavoisiers claim to the theory of oxidation. See our vol. xiii. page 84.

P. S. I was pleased to see your annotations on the notice of the late celebrated Lavoisier's posthumous collection of memoirs; and hope you have not relinquished your intention of vindicating the just rights of our own philosophers against the unqualified claims of that author. I subjoin the following remarks which I lately met with on oxidation. By an experiment of Mr. Homberg's, four ounces of regulus of antimony being calcined by a burning-glass for an hour together, were found to have imbibed and fixed seven drams of the substance of light.* Here the increase of weight by calcination is decisively remarked; though it is attibuted to a wrong cause. But Dr. Hales says, "if fire was a distinct kind of body inherent in sulphur, as M. Homberg and Lemery imagine, then such bodies when ignited would rarefy and dilate all the circumambient air, whereas it is found by many of the preceding experiments that acid sulphureous fuel *attracts* and *condenses* a considerable part of the circumambient elastic air."† And again he says, speaking of the weight which lead acquires by calcination, "that there is good store of air added to the minium, I found by distilling first 1922 grains of lead, from whence I obtained only seven cubic inches of air; but from 1922 gains, which was a cubic inch of red lead, there arose in the like space of time thirty-four cubic inches of air. It was therefore doubtless this quantity of air in minium which burst the hermetrically sealed glasses of the excellent Mr. Boyle, when he heated the minium contained in them by a burning-glass: but the pious and learned Dr. Nieuwentyt attributes this effect wholly to the expansion of

Lavoisier was not the first who observed the increase of weight in oxidation of metals. Homberg observed it in antimony oxided by solar heat. Hales found by experiment that the gain is from absorbed air.

He separated the air again from the minium.

* Bp. Berkley's Sirius, par. 169.

† Beg. Statice, 4th edit. p. 285.—Ibid. 289.

"the

"the fire particles lodged in the minium." I think I remember to have seen a communication from Dr. Priestley in one of the early numbers of your 8vo. series, wherein he says, that when formerly in company with M. Lavoisier at Paris, he informed Mr. L. of his having obtained a very pure kind of air from red lead, which was what he afterwards named pure or dephlogisticated air.

With regard to *oxidation*, therefore, and its cause, M. Lavoisier did not discover; he only confirmed and extended the fact. With respect to the *theory of combustion*, I am sure you will do our countryman Hooke justice. I shall only observe that I do not think we possess any complete theory of that process; for all that we know is, that the pure part of the air combines with the body that is burnt, and forms various compounds according to the nature and composition of that body: the phenomena of flame as yet are unaccounted for. Nearly the same may be said of the claim M. Lavoisier makes to the *theory of respiration*. All the great facts tending to an explanation of that function have been the successive discoveries of Boyle, Lower, Black, Priestley, and Crawford: but nothing in the shape of a satisfactory theory has yet been produced. M. Lavoisier's claims, therefore, in this case, as far as concerns the *facts*, are notoriously the discoveries of others: and as to what regards the *theory*, it is unnecessary to contest with him the right to a thing which has no existence. The truth is, Sir, M. Lavoisier *discovered* very little, but he *improved* and *generalized* a great deal. No one will question or deny his high merits in that point of view, which certainly affords the surest proof of a philosophical genius, inasmuch as the person who constructs the building ranks higher than the mere workman who prepares the materials. But M. Lavoisier in assisting to prepare the materials was too apt to forget his fellow-labourers in the work: and it now appears that he lays claim to the construction of a building which has not yet been raised. An impartial and minute observation of the progress and evolution of any science, will convince us, that what may be called "great discoveries" are things of very rare occurrence; and that, perhaps, Newton is the only person who in the race of discovery has distanced all the rest of mankind.

Lavoisier did not discover, but only confirmed the fact.

The theory of combustion belongs to Hooke;

—supported by facts from Boyle and others.

Reply. W. N.

The fact that razors, &c. recover their edge by laying by, admitted.

That razors and other fine-edged tools when dull, do recover a certain degree of sharpness after being kept for a time, has, I believe, been frequently noticed. I think the fact can scarcely be considered as doubtful; but that it is requisite to strap the instrument in order to produce this acuteness, has not, that I know of, been before remarked.

The editor ascribes it to oxidation and the peculiar nature of steel. Some account of Damascus steel.

My former meditations on this phenomenon have induced me to ascribe it to oxidation modified by a peculiarity in the nature of steel. In the quarto series of this Journal, vol. i. p. 468, I gave a short account of a Damascus sabre, with observations and experiments on the method of making that kind of steel; which has a wavy or fibrous appearance on its surface, called the water, and is valued for its rough cutting edge and its tenacity. It was there explained that the water is produced by the action of an acid upon the metal; of which the minute aggregate parts are some iron and some steel; the former being more readily corroded than the latter. I likewise mentioned a method I had, much to my own profit and satisfaction, used for ascertaining the texture of steel; by applying diluted nitrous acid to its clear surface; the effect of which is to shew the respective veins or imperfect mixtures of iron in the steel,—because these appear of a lighter colour than the steel itself, which loads its solvent with plombage or carburets of iron. On the present occasion I must add, that I never found any specimen, even of the finest cast steel, which did not exhibit a very considerable mixture of parts, some more and some less steely.

Method of examining the uniform texture of steel by an acid.

All steel is probably the same as Damascus steel, but fine grained.

I strongly suspect that these facts may be applied to the case before us. I would assume the position, that the same irregularity which the acid shows us in steel, does also exist among its very minute and perhaps invisible integrant parts; or that all steel has the property of Damascus steel with regard to these parts. Whenever a razor is brought to a keen edge by grinding, setting, and strapping, the fretting powders or the substance on which it is thus rubbed convert it into a very fine toothed saw. By employing this edge against a soft substance the teeth are rubbed, down and the edge, though still thin, becomes much less notched than before: the razor is then dull, and requires to be

be revived by setting or strapping. Instead of this process, however, let it remain exposed to the effects of the atmosphere. If it be thus left for a sufficient time, it will become oxided or quite rusty: but before this happens, it is reasonable to admit that an invisible effect will be produced on its thinest part or edge; or in other words, that it will be corroded by the joint action of the water, carbonic acid, and oxigen of the atmosphere. We have seen in the sabre that the iron is corroded before the steel; and in one of fine Damascus structure the iron parts will be oxided and the steel parts left,—that is to say, the edge will again spontaneously become toothed, and the razor of consequence sharper than before.

—setting reproduces them: —but if laid aside the razor will rust, in time; particularly on the edge; —and that unequally, so as to reproduce teeth and restore the edge.

The use of strapping is to render the edge finer than that which comes from the hone. If we imagine the atmospheric oxidation or corrosion to give a somewhat course edge (which it must do if it revives an edge that was too dull for the strap), the use of the strap mentioned by E. D. will be to do the same in the present case. Or perhaps it may be of use to carry away the oxide with which the chemically formed notches will be clogged.

Strapping may render the edge finer, or clear away the oxide.

It is with great satisfaction that I find the delay respecting my promise in the place cited (xiii. p. 84.) so well compensated for in the postscript to E. D.'s letter. Confinement by illness, and the pressure of many objects of business by that means increased, were the causes of that delay. I fully agree with him that all our theories of chemistry, viz. of heat, combustion, and the affinities, are crude, hypothetical, and unworthy of the importance which is attached to them; though the discoveries of facts relating to each are such as do honour to the sagacity of the great men who have laboured in those several departments of research. I hope to be able shortly to state so much of the history as may extend the observations of my ingenious correspondent in settling the claim of M. Lavoisier to which he has attended. The mischiefs as well as the merits of generalizing are intitled to the serious attention of philosophers; and the integrity of any claimant, whose demands are not well founded, can only be vindicated by a supposition that he did not know the true owners of that which he may profess to have found. M. Lavoisier well knew the claims

Concerning the claims of Lavoisier.

All our theories of chemistry imperfect.

General analogies are as often detrimental as beneficial to science.

M. Lavoisier did attempt to obtain honor

from the discoveries of others. claims of Hales, Cavendish, Priestley, and some others, to whose discoveries he himself and his numerous party at Paris have by direct as well as indirect means endeavoured to establish a title in his own name.

II.

Observations and Experiments on Galvanism, the Precipitation of Metals by each other, and the Production of Muriatic Acid. By CHARLES SYLVESTER, *Esq.*

To Mr. NICHOLSON.

SIR,

Theory of Galvanism. IN October 1804, you did me the honour of inserting in your valuable Journal, some experiments and observations, tending to prove that galvanism was strictly a chemical process, depending upon a double affinity, existing between the water and the metal; the metal being supposed a compound electricity of metallic base, and the water a compound of oxigen and hydrogen.

Some remarks on the theory of H. B. K. I observe in your last Journal a new mode of theorizing on those appearances by Mr. H. B. K. As it is my intention in this paper to give you some new experiments which have for some time occupied my attention, I shall not at present examine Mr. H. B. K.'s hypothesis, having only had time to observe that his opinions are very singular and very liable to objection. I beg leave however just to contradict several assertions made by Mr. K. I have no objection to his convincing the world of the absurdity of modern chemistry; but I would recommend him to bring forward facts for that purpose, without which every opinion must fall into nothing.

Some errors in his facts pointed out. In page 224. No. 52, he says, that with water the wires were much calcined. Now it is right that Mr. K. should know that only one wire, viz. that which comes from the zinc end of the pile could be calcined; for invariably deoxidation would be going on at the opposite wire if the oxide of a metal were present. Mr. K. tells us that the wires are calcined by an acid; and to prove it, he introduces the same wires which were calcined with water, into a solution of potash, and asserts they were not in the least acted upon. This is certainly not true. The wire coming from the copper end of

of the pile, as in the experiment with water, is not acted upon; but the other is oxided in a much greater degree. With my large apparatus (100 plates 32 square,) when I made use of brass wires in a solution of potash, the liquid in the course of ten minuets became perfectly green with the copper of the wire, and was rendered at the same time turbid with the suspended oxide of zinc. I shall not at present say more on this subject, as I mean in a future communication to give some experiments on the action of galvanism upon bodies dissolved in water.

Mr. K., in his last paper, has made some assertions equally destitute of foundation. I am surprised at his giving himself the trouble of attempting to produce gas by using animal and vegetable substances instead of metal. The moisture contained in these bodies connected the liquid of the cell of the trough with the glass of water, making them the same as one vessel. If Mr. K. had looked into the cell where one end of this connecting substance was placed, he would have seen the gas upon the copper plate at the copper end, and the zinc oxided at the zinc end: so that the cells at the opposite end, the moist substances, and the water where these substances terminated, were the same as one vessel, while the terminating plates of the trough or pile performed the office of the metallic wires.

The experiments with animal fibre (instead of wire) which afford no gas, explained. The waters so connected are considered as one fluid.

Mr. K. asserts that he had more gas when the wires were longer. This I deny. He afterwards observes that an iron wire at the silver end of the pile "became brittle." If he made use of a trough, and one end of the wire was immersed in the liquid of the cell, it would be oxided as far as the liquid touched it, and might so far be brittle, but no farther.

Longer wires do not afford more gas, &c.

I have been lately engaged in a very interesting department of chemistry which has been very much neglected, viz. the precipitation of one metal in solution by another in its solid form. The simple action of the piece of metal upon the oxide in solution is insufficient to explain a number of very curious phenomena attendant on some of these processes. In the arbor dianæ we see the appearance of vegetation; and in the experiment with acetite of lead in a phial with a piece of zinc, we observe a bundle of fine filaments of metallic lead reaching to the bottom of the bottle. If a thin

Interesting experiments of precipitation of one metal by another.

The precipitation in the arbor dianæ takes place at the extremities of the branches.

Experiment of silver precipitated by zinc on the face of a glass plate in ramifications.

thin coat of a solution of nitrate of silver be laid upon a piece of plain glass, and in the centre of this be laid a bit of zinc wire, in a little time a beautiful tree of silver will appear as if growing from the wire. If the process be observed with a magnifying glass, the ramifications of silver will be seen to increase by the progressive reductions of silver at their farthest extremity from the zinc; a clear proof that the oxide of silver does not owe its reduction to the zinc, but to something which exists at the point where the increase is taking place, and which I shall prove by the following experiments to be dependant upon the principles of galvanism.

Variation of the last experiment. One half of a glass plate was coated with sol. of silver, the other half with diluted muriatic acid: zinc wire touched the latter fluid, and platina wire the former. When these wires were united at their ends, the silver tree grew from the platina, but ceased when they were disjoined. The act of precipitation was galvanic. Explanation: If silver had not been in the solution the platina would have developed hydrogen. This is supposed to have been transmitted from the place of oxidation to the place of reduction and precipitation.

In the above experiments with the glass plate I made this variety: I coated one half of the glass with nitrate of silver and the other with dilute muriatic acid, so as to touch each other. I then laid one end of a platina wire upon the nitrate of silver, while the other rested on the table, a piece of zinc wire was similarly situated upon that side of the glass covered with the dilute acid: upon uniting those ends of the wires upon the table, I soon had the satisfaction to see a beautiful tree of silver grow from the point of the platina wire. This effect ceased as soon as the ends of the wires were separated. If instead of nitrate of silver the whole of the glass had been covered with the dilute acid, bubbles of hydrogen would have been given out at the platina wire. In this experiment the water is decomposed, the oxigen combines with the zinc, while the hydrogen enters into some combination by which it is invisibly carried across the liquid to the platina wire, where the hydrogen is liberated in its gasseous form. It is easy to observe, that when the platina was in contact with the nitrate of silver, the hydrogen was employed in reducing the silver to its metallic form. When a piece of zinc simply is laid upon the coating of nitrate of silver, the zinc in the first instance reduces the oxide of silver immediately in contact with it: the silver and zinc have now become a galvanic combination, and the remainder of the process is carried on by means of galvanism. The zinc is now oxided by the oxigen of the water, and the silver is reduced by the hydrogen. In the experiment with zinc and the solution of lead, the same explanation holds good. As soon as the least portion of metallic lead is produced by the zinc, a decomposition

of

of water takes place; the oxigen uniting with the zinc, while the hydrogen is invisibly diffused through the liquid, reducing the oxigen of lead upon the metallic lead already found.

To prove that the whole of this effect can be produced by galvanism without the zinc touching the solution of lead, I made the following experiment:

Experiment by vessels in which the dilute acid was separated from a solution of lead by bladder.

The glass tube *AB*, Plate III. Fig. 1. had a piece of thin bladder tied at *B* so as to hold a liquid when filled; the tube was filled with acetite of lead; the vessel *D* was of zinc partly filled with dilute muriatic acid, the wire (passing from *C* through a cork at *A*, and coming down so near to the bladder as not to pierce it) was of pure platina. The bladder it is plain separates the zinc and the acid from the acetite of lead. The wire was for some time unconnected with the zinc vessel, but no change whatever took place in the tube; but as soon as the contact was made at *C*, metallic lead began to be formed at the point of the platina wire. I obtained in this way about six grains of metallic lead. When the tube was filled with the dilute acid instead of acetite of lead, hydrogen gas was given out at the platina wire; so that it seems the bladder itself could not prevent the passage of the combined hydrogen.*

The lead was precipitated as in the case before stated by platina.

When lead was not present hydrogen was developed.

In thus decomposing the metallic solutions the acid is set free.

When acetite of lead is decomposed by galvanism, the acid is set free, and I believe it is the case with all metallic solutions under the same operation. For the sake of not occupying too much of your room as well as my own limits, being confined, I must defer for a future communication more minute observations upon these experiments. By the favour of inserting the above in your next Journal, if you are not already provided with something more important, you will much oblige

Your most humble and obedient servant,

CHARLES SYLVESTER.

Sheffield, May 14*th*, 1806.

Pure water treated with platina wires in

P. S. I have just made the experiment announced by M. Pachiani on the discovery of the muriatic base, and repeated

* It is very probable that the electricity present on these occasions combines with the hydrogen when the oxigen combines with the zinc, and that it is invisibly transmitted through the liquid to the platina wire.

 with

the apparatus last mentioned, indicated muriatic acid and an alkali.

without success by the galvanic society at Paris. I made use of water which was not altered by nitrate of silver. I took a tube, secured at one end by a bladder, into which I introduced pure water. At the other end was a cork, through which a platina wire was passed nearly to the bottom of the tube. I then set the tube in a wine glass containing also pure water, into which I introduced another platina wire, the end of which came under the end of the tube, as near the bladder as possible. The wire in the tube was connected with the zinc end of the trough, that in the glass with the copper end; after the process had continued an hour, I put the liquid in the tube to the test of nitrate of silver; and when I had a sufficient precipitate to indentify the presence of muriatic acid, the liquid in the glass contained an alkali, which I suspect to be ammonia. I hope in my next to speak more fully to this. If, as Mr. Peel has asserted in the Philosophical Magazine, this alcali should be soda, what an important field will be opened to the chemist and the naturalist! If a galvanic trough of moderate size be capable of generating muriate of soda from water, the same effect must in some degree take place when two metals of different degrees of oxidability are immersed in this liquid. Hence the water used in the economy of life and in other situations, must be frequently undergoing this change. The muriate of soda, thus generated, is carried by the rivers into the ocean, from whence it cannot return by evaporation: need we then wonder at the saltness of the sea?

Query whether the bladder affected or caused these products? W. N.

III.

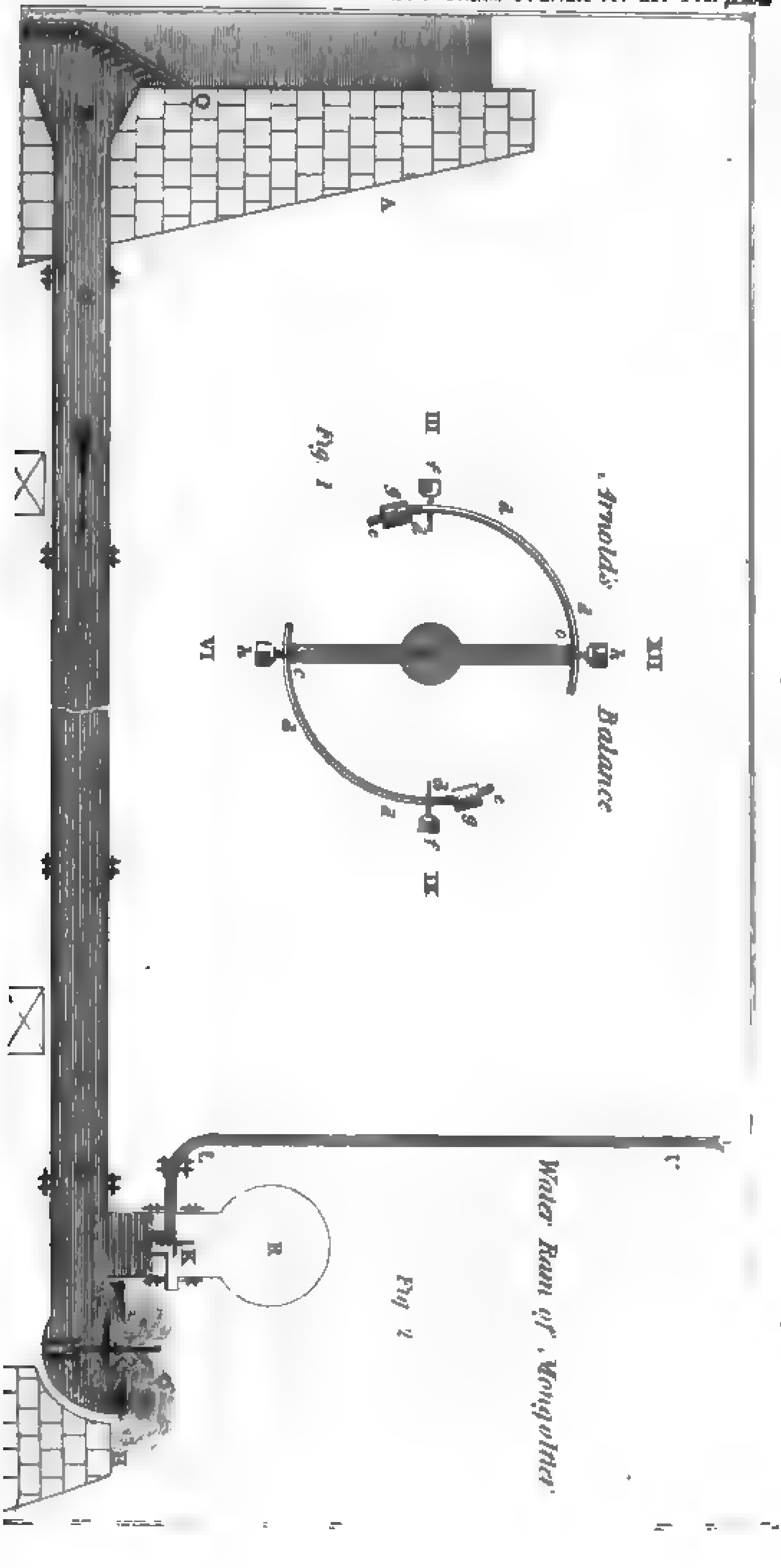
Of the Utility of the Water Ram; by M. Jos. MONGOLFIER, *Demonstrator to the Repository of Arts and Trades, Paris.** *With Remarks by* W. N.

The raising water by men or horses is expensive.

IT is frequently necessary to raise water to considerable heights when we are precluded from employing the strength of men or animals for this purpose, because these would be too expensive. Accordingly, recourse has been had to streams of water, or the effects of fire, to supply the power required.

* Translated from Sonini's Journal, No. V. Feb. 18[illegible], p. [illegible]1.

The


Arnold's Balance
Fig. 1
XII
VI
III
IX
Water Ram of Mongolfier
Fig. 2
A
R

The force of water has hitherto been applied only by means of wheels, or of pumps, more or less complicated in their construction; and, notwithstanding the improvements made in machines of this kind, the cost of their erection has been so great, and the supply they have furnished so small, that princes and great towns alone have been able to avail themselves of them. A private individual, even of considerable wealth, cannot afford an hydraulic machine: and agriculture, in which irrigation is so frequently necessary, has derived no assistance from such engines. Water may be employed as the moving power: but the machines are, in many instances, too expensive.

With regard to steam engines, as their construction will scarcely be deemed less complicated than that of hydraulic machines moved by wheels, and as fire is often more expensive than a stream of water as a first mover, they can be used for raising water with advantage only in large works, and where coal is plentiful. Steam engines too dear, unless for large works, and where coal is cheap.

These were the reflections which led me to seek after other means of applying the power with which we are furnished by nature in falls of water, for the purpose of raising, at little expence, a part of the same water to any height that may be required. Investigation of simpler means,

In this inquiry economy being my object, it may be presumed that I turned my attention entirely to the most simple means I could invent; for by simplicity alone could I hope to succeed. Consequently I was obliged to give up the system of wheels, and of pumps, in which so many have exerted their abilities to little purpose. because most economical.

What indeed can we expect from the common hydraulic machines? They display a complicated and of course expensive assemblage of materials of various kinds, the cost of which is frequently increased, as well as their effect injured by the strangeness of their figure. Is not the machine of Marly in its kind the *opus magnum* of mechanists, a kind of monstrosity, whether we consider the multitude of its parts, the immense sums of money it cost, or the small quantity of water it raises? Defects of hydraulic machines. Machine of Marly.

The better to avoid the defects inherent in the nature of these machines, I chose one founded on principles totally different, principles that completely exclude the defects I wished to avoid. An exhibition of these principles, and the The water engine of the author.

theory of the action of this machine, are not my object in the present paper; in which I mean only to make known their practical results, in such a way as to enable any one to form a sufficient judgment of the effects produced by this machine, and avail himself of the advantages it offers. A description of the machine itself, and an account of some of the trials made with it, will be sufficient to show, that its simplicity gives it a decided superiority over others, from the less original expence it demands, and the greater supply it affords.

Advantage of simplicity in machines.

This double advantage obtained at the same time is very evident. In fact, simplicity is the most estimable quality of a machine; at the same time that it renders it cheaper in the first cost, and to keep it in order. It is better, because every useless piece not only adds to the expence, but is detrimental to the end to be obtained. The simplicity of a machine is therefore an argument in its favour with men of sense; and we may reasonably expect greater effect from a simple engine than from one of greater complexity.

What is meant by the effect of a machine.

It may not, perhaps, be amiss to explain what I mean by the effect of an hydraulic machine, as it may lead to a more accurate judgment of the true value of my engine, called the Water Ram (Belier), and other machines.

I have premised, that for raising water we must employ some power which is furnished by different agents. The ram is worked by that part of the water that falls.

Power of a fall of water.

It is a simple fact, of which no one can be ignorant, that falling water has only a determinate power, which is proportionate to the mass of the water, and the height from which it falls: accordingly, if I would express the power I have to employ in numbers, I multiply the mass of the falling water* by

Simple means of estimating the moving power of water.

* The means we have of measuring accurately the quantity of water flowing from a spring or in a river, are not of a nature to be employed with facility by every one: but we may have recourse to approximations, the results of which will always come much nearer the truth than would be supposed from the notions commonly formed on such subjects, and will often be fully sufficient for my purpose. For example, if it be wished to be known how much water flows through an aperture under a certain pressure, it will

by the height it passes through, and the product of these two numbers is the expression of the power. Thus I may know with precision the quantity of the power I have at my disposal.

Author's use of the terms *power* and *effect*.

As I give the name of *power* to the product of the mass of water multiplied by the height of its fall, I likewise give the name of effect to the product of the mass of water raised, multiplied by the height to which it is carried. This is the effect or useful product of the machine.

The effect is never equal to the power producing it.

This second quantity can never be equal to the first, because in the motion of the machine a part of the power is always expended [in generating velocity, and] in overcoming friction, and in useless movements: but we may bring them considerably nearer to an equality, by diminishing, as much as possible, the last two impediments of which I have been speaking, and this I have in part effected in the water ram.

Ground of the comparative estimate of machines.

Thus to estimate the value of a machine, we must find the ratio of the power expended, or of the mass of water that falls multiplied by the height of the fall, to the effect produced, or the mass of water raised multiplied by the height of its ascent. The comparison of these two quantities gives the true measure of the excellence of the machine.

will be sufficient to measure the aperture, and the height from the middle of the aperture to the surface of the water. By multiplying the surface of the aperture by the velocity the water would acquire in falling from the surface to the middle of the aperture, and taking two-thirds of the product, we shall have nearly the quantity discharged. If we had a brook or a rivulet to measure where there was no fall, we may observe the height to which the water would rise against an obstacle opposed to the stream, such as a slip of wood two inches wide and half an inch thick, and calculating the velocity as in the preceding case, multiplying it by the surface of a section of the river perpendicular to the current, and this would give the mass of the flowing water.

The velocity of the water may also be better estimated by that of any floating body, following it a little time with a watch in the hand. If we have a small spring to measure, and a reservoir at our disposal, it would be a much more certain method to calculate the capacity of the reservoir, and observe the time it took to fill. (These last give rough estimates only. N.)

The

Machine of Marly.

The machine of Marly, when first constructed, appears to have produced $\frac{1}{24}$ of the power expended, so that $\frac{23}{24}$ of its power were (chiefly) misapplied. This misapplied power has been injurious to it, and the wear it has occasioned has reduced the effect to $\frac{1}{40}$, $\frac{1}{60}$, and at length even to $\frac{1}{200}$.*

Hydraulic machines in general lose 9-10ths of their power.

It would be difficult to exhibit the results of other similar hydraulic machines with wheels and pumps, because they are not common, and are often erected under circumstances where it is not easy to make the proper calculations; but it is certain that, with all possible care in the execution, they usually produce less than one-tenth of the power employed. They are not very productive therefore; but this is not the only reason for their being so rare. In fact they are not applicable to a number of falls of water, small in quantity, or of little height; and their expence includes not only the cost of the machinery, but of buildings to defend it from the weather, which renders them very costly.

Why they are not more common.

The effect of the water ram equal to 3-4ths of its power, and at least ½.

A water ram, made with care, may produce $\frac{75}{100}$ of its first power, as I have seen; but in general it gives only $\frac{60}{100}$. Indeed I engage only for $\frac{50}{100}$ or $\frac{1}{2}$: I mean, that if you would raise water by it 100 feet with a fall of 5, only a fortieth part of what falls will be raised to that height. If the fall were 10 feet, a twentieth part of the water would be raised: if the height were 200 feet, and the fall 5, only an eightieth part; and so in all other cases.

I shall hereafter relate the experiments from which I deduce these results.

Superiority of the water ram.

Thus we may admit, that the water ram generally gives $\frac{50}{100}$ of the power it expends. Its effect therefore is five times as much as that of the common hydraulic machines, under the same circumstances; yet it is far from being perfect, as appears from the rules I have laid down for judging of the merits of a machine. It may be rendered more effective, as I have already

* The part of the Seine that passes the machine at Marly is nearly equal to 300,000 cubic inches, and falls about four feet and half. The moving power therefore is $300{,}000 + 4{\cdot}5 = 1{,}350{,}000$. The quantity of water raised was 120 hydraulic inches, and the height 475 feet. The power produced therefore was $120 + 475 = 57{,}000$, or $\frac{1}{24}$ of that expended.

already observed; but the care that it would require to make it so, would occasion it to be too costly in some instances for the purpose for which it is intended: there are circumstances, however, in which it would be worth while to purchase water at this price.

I recommend, therefore, to all who have falls or streams of water at their disposal, the power of which may be employed for raising water, either for the supply of houses or manufactories, or for irrigations, to employ the *water ram*. There are many situations in which it may be of the greatest utility. The great expence of other machines has in some measure prevented on frequent occasions the use of water. Farmers have never even thought of applying it in husbandry; but in future they cannot avoid considering this operation as very easy, and calculating the great advantages they may derive from artificial irrigations. **Its use in husbandry.**

The construction of the water ram even enables them to raise turbid and muddy water, which is so useful as manure; and it requires but little care to prevent such water from doing the least harm to this machine. **Muddy water may be raised by it.**

The following is the description of a water ram. Plate III. *Fig*. 2. represents it placed before a dam *A*, constructed so as to confine the stream of water, and oblige it to pass through a cone *B*, adapted to a long tunnel *CD* of iron or copper, connected with the masonry of the dam, and placed on pieces of wood which support it throughout its whole length. **Description of the water ram.**

The depth of water above the cone *B* is supposed to be four feet, and the length of the tunnel *CD* twenty-four feet.

To the extremity of the tunnel *D* a piece of copper or iron, which I call the head of the ram, is fitted. In this head are two apertures; one, *O*, which the valve *G* shuts by rising; another, *I*, which the valve *K* closes by falling. The motions of these valves are guided by sockets through which the stems or axes passing through their centres are made to pass.

When the aperture *O* is open, the water issuing from it is dispersed on all sides. *I* is covered with a kind of bell, *R*, in the side of which is a hole, with a pipe *LU* fitted to it, which rises as high as the water is intended to be conveyed.

The piece of wood on which this machine is placed reaches under the head of the ram, and is fixed on a solid wall, which

is

is built at *H*, and receives the support of the head of the tarn, so as to render the whole of the machinery as firm as possible This is easily accomplished by laying *H* with very large stones, or by placing them against immovable obstacles.

Manner of its acting.—An horizontal column of water is suffered to run out thro' a valve, which shuts when a certain velocity is acquired. The momentum then drives a portion of water through another valve to the elevation required.

I shall now speak of the action of the machine.

If the valve *G* open, the water filling the long tunnel *BCDO* will issue out, and disperse itself with a velocity continually increasing; for this water will fall, and it is well known that falling bodies acquire an increase of velocity.

Now at a certain velocity the force of the current will raise the valve *G*, and the orifice *O* will be closed. The whole of the cylinder of water included in the long tunnel *BCDO* will then be suddenly stopped; but the active force with which it is impelled cannot be annihilated: this therefore will exert a pressure against the whole of the inside of the tunnel, and as this is very strong, the pressure will impel a certain quantity of water through the aperture *I*, which the valve, raised by the water itself, leaves open. Consequently water will enter into the bell *R*. The active force existing in the cylinder of water being thus expended, the pressure, to which the valve *G* had yielded, will cease to exist, and the elasticity of the metal, assisted by the weight of the valve, will cause it to descend again. The aperture *O* being then open, the water will begin again to flow, and these operations will be repeated as long as there is any water before the dam.

The effect is produced by alternations

This succession of actions, which has taken me so long to describe, passes in a very short space of time; frequently in less than half a second, sometimes in three seconds, according to the dimensions and conditions of the machine: but be the time what it may, all that I have said actually takes place, beside other events, that it is not necessary to mention here.

—from half a second to three.

It is obvious then, that every time the stopping valve *O* closes, a portion of the water which was in motion will pass into the bell. This will soon be sufficient in quantity for the extremity of the ascending pipe to be immersed in it, and then the air over the water will have no communication with the atmosphere. If the machine continue its action, the water entering into the reservoir will compress this air, and this air, reacting on the surface of the water, will force it to ascend in the pipe *LL'* to the greatest height that can be given to it. In

The water may be raised to any height.

In fact, were this pipe 1320 feet long, so that the pressure exerted by the water in it would be forty times that of the atmosphere, the air would be condensed by this pressure into a fortieth part of its usual bulk, and in consequence the ascending valve *K* could not be raised without a pressure at least equal to that of the air: but at the moment the stopping valve closes, the cylinder of water in motion cannot be stopped without imparting its active force to a portion of the water, which has no easier way of escape than by the aperture *I*; accordingly the valve *K* will be raised, and a portion of the water will enter into the reservoir. This, it is true, will be less, in proportion as the height of the ascent is greater; but it will nevertheless contain the greater part of the active force exerted by the motion of the cylinder of water *BCDO*. A quarter of a mile perpendicular, for instance.

The example I have taken of a pressure of 1320 feet has been verified by experiment, and all that could have been expected in it actually took place. This pressure is considerably greater than any that usually determines the height to which attempts have been made to raise water, so that the water ram is applicable to every possible case of ascent, a circumstance that could never be accomplished by wheels or pumps. This proved by experiment

This description must be sufficient to show, that the ram is liable to very little friction, and that in consequence it will last much longer than other machines. If the parts be made as strong as the calculations of the resistance requires, it may be presumed it will produce a very profitable effect for a great length of time without much alteration. The engine is liable to very little wear.

The following is an account of the two experiments, which I promised, to show the value of the ram.

Estimate of the expenditure and produce of the water ram constructed at the bleaching works of M. Turquet, near Senlis. Force expended and effect produced by water ram. Exp. I.

The ram is seven inches and half in diameter; the height of the head of water above the stoppage valve is three feet two inches.

The height to which the water is raised is fourteen feet two inches above the same point.

The water that descended in a hundred strokes of the ram, which took place in three minutes, was 1987 English quarts.

 The

Ten parts of water raised six by its fall. Height 11 feet.

The water raised ············· 269 English quarts,
The force expended therefore was $1987 \times 3\frac{1}{6} = 6293$.
That obtained, or the effect ······ $269 \times 14\frac{1}{6} = 3811$.

Consequently if I represent the first of these forces by 100, they will be in the proportion of 100 to 60. That is to say, the force received was $\frac{6}{10}$ of the whole the machine could employ.

An engine 7½ inches diam. raised 500hhds. a day above 11 feet.

In a second experiment, made somewhat differently, it was $\frac{61}{100}$ of the force applied.

This ram delivered at the height of fourteen feet two inches (say 11 feet) a stream of water equal to 20 hydraulic inches (pouces de fontanier), and in twenty-four hours working would raise near 130,000 quarts to that height.

Exp. II.

Estimate of the expenditure and produce of a water ram two inches in diameter, placed in my own garden, rue des Juifs, No. 122.

The fall of water, which I have procured artificially for want of a natural stream, is $7\frac{1}{2}$ feet.

By a smaller engine 10 parts raised 6½. Height 50 feet.

The height to which the water is raised is that of my house, or 50 feet.

The water expended in four minutes I find to be 315 quarts; that raised 30 quarts.

Accordingly the force employed is $315 \times 7\frac{1}{2} = 2362$.
That received ················ $50 \times 30 = 1500$.

This engine of 2 inches would raise 43 hhds. a day 50 feet high.

They are therefore in the ratio of 100 to 64. Consequently the produce is $\frac{64}{100}$ of the expenditure.

This ram therefore is capable of delivering 10,800 quarts of water a day, at the height of 50 feet, provided the current be the same throughout that period.

Another water ram for streams.

The figure of the ram I have given is adapted only to falls of water. I have contrived another for streams, but this necessarily costs more money, though it may frequently prove very useful. It would take up too much room to describe the construction of this ram at full length, besides it must be varied according to the situation; and I would undertake it only for persons who, having occasion for such a machine, should acquaint me with the rapidity and magnitude of their stream of water.

I furnish only the part, which I call the head of the ram, or that

that included between D and H, with exception of the ascending pipe. I make all the parts, that form this head, of a proper strength; but as the whole requires to be in due proportion, it is necessary to point this out to those who would make use of the ram, and to apprize them of the precautions requisite to the success of the machine.

The dam, as I have supposed at R, is a very common structure. Falls of water are applied to some purpose or other, almost every-where, and this arrangment is always requisite. If a person have not such a dam, and wish to avail himself of a rapid stream to procure a fall, one might be constructed in the common mode, without any difference on account of the ram, except a place for fixing the tunnel BD in the part B.

It is advisable to place a grating QT before the cone leading to the tunnel, to stop the greater part of the filth brought down by the current. This grating should not be very close, as it is intended to stop only the weeds and bits of wood that would be liable to prevent the valves from shutting perfectly. Mud and sand could be of little or no injury, because they are continually carried away by the stream O. A grating should be placed before the cone.

The body of the ram, BCD, should be throughout its whole length equal in diameter to the entrance D of the head of the ram. All irregularities, whether from swelling out or being narrowed in any part, should be carefully avoided, as they would greatly diminish the velocity of the water. The interior surface ought to be as smooth as possible, to occasion less friction. Body of the ram.

The tunnel should be made of iron, copper, or lead. I shall give the means of calculating its thickness below; but it ought to be capable of sustaining at least twice the pressure of the column of water ascending from the air reservoir.

The joints of the different pieces which form the body of the ram, must be made with great care, and strong. The slightest aperture would diminish the effect of the machine. There must be neither elbows nor alterations in the slope. With respect to the length of the tunnel, I will give proper directions to any one who wishes to have a water ram constructed.

I shall have given every necessary information respecting the

 fixing

fixing of a ram, when I have spoken of the pipe for conducting the water from the reservoir of compressed air fo the place required: for what I shall say of its thickness will be applicable to that of the body of the ram, of which I have promised to speak, with this difference only, that the tunnel ought to be nearly double the thickness of the ascending pipe, paying regard likewise to the proportion required by the difference of diameter.

To the head of the ram I add a piece of this tunnel, to which the rest may be soldered. This serves as a guide for the diameter, which ought to be the same, as well as for the thickness, if the tunnel be made of the same material as the head of the ram.

Rule for calculating the thickness of the pipes.

Should this not be the case, the following are the principles, on which the thickness of the material employed should be calculated. If we know the thickness of a pipe of given substance in a given experiment, and the height of the column of water, the pressure of which it bore without bursting, we may by this proportion the thickness of the pipe to be made to the pressure it will have to sustain, and the diameter given it.

For example, we know that a leaden pipe a twelfth of an inch thick and an inch in diameter has supported a column of water of 50 feet; and if we would know the thickness required to support a column of 100 feet with the same diameter, we must multiply the preceding thickness by the ratio of 100 to 50, that is by 2. If we would alter the diameter and not the height, we must multiply the preceding thickness of the ratio of the diameters; so that if the diameter were increased to 4 inches, the thickness must be $\frac{4}{12}$ of an inch. If the pressure and the diameter be both increased, the thickness must be increased in the ratio of both.

Nothing can be more simple than this calculation; and to avail ourselves of it we need only know the pressure that pipes of different substances have sustained with a given diameter. This the following table will show:

Table of the strength of pipes.

	feet.
A pipe $\frac{1}{12}$ of an inch thick and an inch in diameter, of copper, supported a column of water of ……	400.
A pipe of the same dimensions of brass of a good quality, about a fourth less, or ……………	300.

The

	feet.
The same of sheet lead	50.
A pipe of cast iron, two inches in diameter, and 1-3d of an inch thick, at least	500.
A pipe of elder, an inch and half in diameter and two inches thick	30 or 40.

I have seen pipes of this kind however constantly supporting the pressure of a column of water of 110 feet.

From these data it will be very easy to calculate the proper thickness of the ascending pipe according to its diameter, which should be the same throughout its whole length. If it be necessary to make turns in it, they should not be too sharp; for the water will move in them with more facility, the more gently the alterations in its direction are made. Ascending pipe.

It is obvious, that, when water is to be raised to a very great height, the ascending pipe does not require the same thickness throughout its whole length. The greatest thickness is requisite only for the lower part, the pressure diminishing as the column of water shortens; the thickness therefore may be gradually diminished also, calculating what is required at different heights according to the rule above given.

Having now given a sufficient description of this new machine, to make it known to those who may have occasion for such a one, it will be proper to speak of the expence attending its construction.

This, as may be supposed, is very variable, as affected by the circumstances of its situation. However we may say in general, that the expence is proportional, 1st, to the quantity of water to be raised; 2dly, to the height to which it is to be conveyed; 3dly, to the distance from the reservoir; 4thly, to the fall of water, as the expence will be greater in proportion as this is less. Expence of construction.

An example will give a more accurate idea of the expence of the machine. The object was, to raise to the height of 108 feet the greatest quantity of water possible with a fall of 4 feet 9 inches. For this purpose a ram of 4 inches diameter was employed, which expends nearly 11,500 cubic feet of water a day, and in that time raises nearly 240 cubic feet to the height required. The ascending pipe is 900 feet long. The ram properly One of 4 inches raises 147 gall. a day, 180 feet, with a fall of 4 feet 9 inches.

and cost £200.

properly so called cost 2400 livres [£100], and the other parts necessary to the machine about as much. Thus for £200 a quantity of water fully sufficient for all the demands of a large family, for cattle, and for watering a garden, is raised to the height of 108 feet.

Costs little to keep in order.

To keep the machine in order cannot cost much; for all that is required is the renewal of a few leathers, or pieces of trifling value, and in cleaning it after floods or frosts.

Tried in various places.

I believe I have given sufficient proofs of the utility of the water ram; and the facts I announce may be verified at several places, where they have been fixed both in France and abroad. Individuals therefore may now judge of the advantages they may derive from the machine.

Principles applicable to other purposes,

I have spoken only of one kind of water ram; but it is easy to conceive that the principles in which a machine so different from any hitherto known constructed, are capable of much more extensive application. For instance, water different from that which runs in the ram may be raised by it; that is to say, by means of a fall of water, we may draw water out of the bottom of a well or of a mine, as with any other engine; and from the simplicity of the ram it would have a great advantage over others generally employed for this purpose. But I shall not turn my attention to other applications of the principles of the water ram, till I have derived all the advantage possible from the application I wish to make of it to falls and streams of water.

—as raising water from mines or wells.

Remarks. W. N.

The water ram was proposed some years ago by Mongolfier,

The engine described in the preceeding memoir as invented by M. Mongolfier was the subject of much attention about nine years ago, and a patent was then taken out for it in this kingdom. That part of the contrivance which is due to this ingenious experimental philosopher (so well known by his inventions in aerostatics), consists in his causing the operation of raising water by its momentum, first acquired by falling through a pipe, to be performed without attendance.

—and executed in Cheshire, much earlier, at the seat of P. Egerton, Esq.

An engine of this kind, not acting spontaneously, was executed thirty-four years ago at Oulton in Cheshire, the seat of Philip Egerton, Esq., and is described by Whitehurst in the Philo-

Philosophical Transactions for 1775. It consists of a pipe $1\frac{1}{2}$ inch diameter and upwards of 200 yards long, proceeding horizontally from a spring eighteen or twenty feet above the level of the kitchen of the house, where a branch of the pipe terminates in a cock for supplying the domestic purposes of the family. The horizontal part of the pipe has a valve to admit the water, but prevents its return. After passing this valve it enters an air vessel similar to that described in the preceding account, and proceeds from thence by means of another pipe upwards to the brewery and other offices. Whenever the kitchen cock is opened to draw water, the whole column of 200 yards is set in motion, and upon shutting the cock, its momentum drives a portion of water through the valve into the air vessel, and thence upwards to the place of its destination.

Mr. Whitehurst does not say to what height it is thus conveyed, nor the quantity raised each time of opening and shutting the cock.

It is certainly of importance to know what may be the true value of the water ram; and as M. Mongolfier has left some parts of his account defective as to this object, I shall take the liberty of making some additional remarks. Introduction to some remarks.

P. 104. *this water will fall.*] It is not exactly the case that the water in the pipe moves by its fall. It is put into motion by the pressure of the head, and would acquire a velocity equal to that of a body that had fallen through the whole height from the surface of the flood (impediments of friction, eddy, or obstruction in the pipe excepted). I do not know that any one has examined the theory of this engine. The general proposition may be made thus, and I hope some of my correspondents will investigate it: The theory of the water ram has not been published.

Proposition.—Given the height of an head of water and the height to which a part of the fluid is required to be elevated by the water ram, it is required to determine the diameter and length of the pipe with the dimensions of the other parts of the engine, and also the number of strokes per minute which shall afford the greatest possible quantity of water by a (given) long-continued action. Proposition to be solved.

P. 105.

It is probable that the water ram would not be durable under high pressures.

P. 105. *the pipe* 1320 *feet long.*] This column of water which answers to more than 40 atmospheres, would, I apprehend, greatly exceed what in actual practice could be trusted either to the water ram or any other hydraulic engine. Bramah's pressure engine, described in our first volume of the quarto series, supports between two and three hundred atmospheres by piston work: but a sudden stroke with this action would soon destroy the apparatus. By suddenly checking the escape of the water in one of the small pressure engines which acted by a power of upwards of one hundred atmospheres, I found that the cylinder, which was one inch diameter and half an inch thick in metal, was soon hammered out so as to exhibit external marks of the violence, and no longer to fit its piston.

The author's estimate of its power defective.

In order to form a more correct notion of the actual effect of the author's engines compared with other hydraulic machines, it may be observed that the author seems to have supposed, in p. 102, that the effect of an engine falls short of the measure of power applied to it only because of friction and impediments. I have inserted the words *generate velocity* between brackets in the text, because the case in question relates to preponderance and not equilibrium. This indispensable deduction from the power in the best and simplest engines is at least one-fourth, in order to work at a proper rate. The author greatly undervalues the engines which raise water by wheel work; and by referring to the machine at Marly he has taken perhaps the least productive which could have been any where found. (See the description, with large plates, in Desaguliers' Lectures.) As an under-shot wheel engine, it has only half the power of an over-shot; and from the prodigious weight and complication of its parts, it is not in the least adapted to be taken as the representation of works of the kind.

He underrates other hydraulic engines.

The water ram has not more power than other engines,

—as it may raise three-fourths of what falls.

Though the water ram is deserving of notice for its simplicity and effect in many situations, some of which the inventor has mentioned, it does not from his facts appear to have more power than other engines. This statement, of between three-fourths and one-half of the power (page 102) being obtained in the effect, is very much to the credit of the engine, as it comes near the water pressure engine of Trevithick (Phil. Jour. v. i. p. 161.) and other good machines which are not loaded with needless parts. From general reasoning, I should not have expected so much.

The

The water ram of M. Turquet, taken by the medium rate of estimation (namely, according to Desaguliers*) is equal to one-third part of a man, or one-fifteenth part of an horse.

The two engines he made are on a very small scale; viz. one-third and one-eighth of a man.

The engine at the author's house works at the rate of one-eighth of a man's power; so that a man would require three hours to raise as much water as the engine raises in 24 hours.

Larger exhibitions desirable.

We must therefore consider these engines as models or small works capable of being multiplied: but we have not yet any practical results on a large scale.

The water ram appears to be a very ingenious contrivance, but it is by no means any new principle or extraordinary effect in hydraulics, as some writers have pretended.

IV.

Account of a Series of Experiments, shewing the Effects of Compression in modifying the Effects of Heat. By Sir JAMES HALL, *Bart. F.R.S. Edinburgh.*

(Continued from page 22.)

SECT. VI.—*Experiments made in Platina,—with Spar,—with Shells,—and with Carbonate of Lime of undoubted purity.*

Suspicion that the fusibility of the carbonates arose from impurity.

SINCE I had the honour of laying before this Society a short sketch of the foregoing experiments, on the 30th of August last (1804), many chemists and mineralogists of eminence have favoured me with some observations on the subject, and have suggested doubts which I am anxious to remove. It has been suggested, that the fusibility of the carbonates may have been the consequence of a mixture of other substances, either originally existing in the natural carbonate, or added to it by the contact of the porcelain tube.

If so this would not affect the inferences.

With regard to the first of these surmises, I beg leave to observe, that, granting this cause of fusion to have been the real one, a material point, perhaps all that is strictly necessary

* On men and horse powers, as computed by engineers. See Philos. Journal, ix. pp. 215,—217.

in order to maintain this part of the Huttonian Theory, was nevertheless gained. For granting that our carbonates were impure, and that their impurity rendered them fusible, still the same is true of almost every natural carbonate; so that our experiments were, in that respect, conformable to nature. And as to the other surmise, it has been shewn by comparing together a varied series of experiments, that the mutual action between the lime and the porcelain was occasioned entirely by the presence of the carbonic acid, since, when it was absent, no action of this kind took place. The fusion of our carbonates cannot, therefore, be ascribed to the porcelain.

Experiments projected to determine this point.

Being convinced, however, by many observations, that the fusibility of the carbonate did not depend upon impurity, I have exerted myself to remove, by fresh experiments, every doubt that has arisen on the subject. In order to guard against natural impurities, I have applied to such of my friends as have turned their attention to chemical analysis, (a branch of the science to which I have never attended, (to furnish me with carbonate of lime of undoubted purity). To obviate the contamination arising from the contact of the porcelain tubes, I determined to confine the subject of experiment in some substance which had no disposition to unite with the carbonate. I first tried charcoal, but found it very troublesome, owing to its irregular absorption of water and air.

Pure carbonate enclosed in platina rolled up.

I then turned my thoughts to the construction of tubes or cups of platina for that purpose. Being unable readily to procure proper solid vessels of this substance, I made use of thin laminated plates, formed into cups. My first method was to fold the plate exactly as we do blotting-paper to form a filter (Fig. 26. See the quarto Plate marked 3, in vol. xiii.); this produced a cup capable of holding the thinest liquid; and being covered with a lid, formed of a similar thin plate, bent at the edges, so as to overlap considerably (Fig. 28.), the carbonate it contained was secured on all sides from the contact of the porcelain tube within which it was placed. Another convenient device likewise occurred: I wrapt a piece of the plate of platina round a cylinder, so as to form a tube, each end of which was closed by a cover like that just described (Fig. 27. and 29.). In figure 26. and 27. these cups are represented upon a large scale, and in 28. and 29. nearly

nearly of their actual size). This last construction had the advantage of containing eight or nine grains of carbonate, whereas the other would only hold about a grain and a half. On the other hand, it was not fit to retain a thin liquid; but in most cases, that circumstance was of no consequence; and I foresaw that the carbonate could not thus escape without proving the main point under consideration, namely, their fusion.

The rest of the apparatus was arranged in all respects as formerly described, the same precautions being taken to defend the platina vessel as had been used with the inner tubes of porcelain,

In this manner I have made a number of experiments during this spring and summer, the result of which is highly satisfactory. They prove, in the first place, the propriety of the observations which led to this trial, by shewing, that the pure carbonate, thus defended from any contamination, is decidedly more refractory than chalk; since in many experiments, the chalk has been reduced to a state of marble, while the pure carbonate, confined in the platina vessel, has been but very feebly acted upon, having only acquired the induration of a sandstone.

The results proved that the pure carbonate is more refractory than chalk,

In other experiments, however, I have been more successful, having obtained some results, worthy, I think of the attention of this Society, and which I shall now submit to their inspection. The specimens are all inclosed, for safety, in glass tubes, and supported on little stands of wax, (Fig. 31. 32. 33). The specimens have, in general, been removed from the cup or tube of platina in which they were formed, these devices having the advantage of securing both the vessel and its contents, by enabling us to unwrap the folds without violence; whereas, in a solid cup or tube, it would have been difficult, after the experiment, to avoid the destruction either of the vessel or its contents, or both.

—but the results were interesting.

April 16, 1805.—An experiment was made with pure calcareous spar from St. Gothard, remarkably transparent, and having a strong double refraction. A temperature of 40° was applied; but owing to some accident, the weight was not known. The conical cup came out clean and entire, filled not quite to the brim with a yellowish-grey substance,

Pure calcareous spar underwent decided fusion.

having a shining surface, with longitudinal streaks, as we sometimes see on glass. This surface was here and there interrupted by little white tufts or protuberances, disposed irregularly. On the ledge of the cup, formed by the ends of the folded platina, were several globular drops like minute pearls, visible to the naked eye, the number of which amounted to sixteen. These seem to have been formed by the entire fusion of what carbonate happened to lie on the ledge, or had been entangled amongst the extremities of the folds, drawing itself together, and uniting in drops; as we see when any substance melts under the blowpipe. This result is preserved entire, without deranging the tube. I am sorry to find that it has begun to fall to decay, in consequence, no doubt, of too great a loss of its carbonic acid. But the globules do not seem as yet to have suffered any injury.

Similar experiment.

April 25.—The same spar was used, with two grains of water, and a heat of 33°. I have reason to suspect, however, that, in this and several other experiments made at this time, the metal into which the cradle was plunged, on first introduction into the barrel, had been too hot, so as to drive off the water. There was a loss of 6.4 *per cent.* The result lay in the cup without any appearance of frothing or swelling. The surface was of a clean white, but rough, having in one corner a space shining like glass. The cup being unwrapt, the substance was obtained sound and entire: where it had moulded itself on the platina, it had a small degree of lustre, with the irregular semitransparency of saline marble: when broken, it preserved that character more completely than in any result hitherto obtained; the fracture being very irregular and angular, and shining with facettes in various directions. I much regret that this beautiful specimen no longer exists, having crumbled entirely to pieces, notwithstanding all the care I took to enclose it with glass and wax.

Carbonate of lime purified by art was rendered crystalline and semitransparent by heat of 32°.

April 26.—An experiment was made with some carbonate of lime, purified by my friend Sir George Mackenzie. Two grains of water were introduced, but were lost, I suspect, as in the last case. The heat applied was 32°. The loss of weight was 10.6 *per cent.* Yet though made but one day after the last-mentioned specimen, it remains as fresh and entire as at first and promises to continue unchanged. The external surface,

surface, as seen on removing the lid of the conical cup, was found to shine all over like glass, except round the edges, which were fringed with a series of white and rough sphericles, one set of which advanced, at one spot, near to the centre. The shining surface was composed of planes, which formed obtuse angles together, and had their surface striated; the striæ bearing every appearance of a crystalline arrangement. When freed from the cup, as before, the substance moulded on the platina was found to have assumed a fine pearly surface. Some large air bubbles appeared, which had adhered to the cup, and were laid open by its removal, whose internal surface had a beautiful lustre, and was full of striæ like the outward surface. The mass is remarkable for semi-transparency, as seen particularly where the air-bubbles diminish its thickness: a small part of the mass being broken at one end, shews an internal saline structure.

A shell partly fused with little change of figure at 30°.

April 29.—A cup of platina was filled with several large pieces of a periwinkle* shell, the sharp point of the spiral being made to stand upright in the cup, (fig. 30.). A heat of 30° was applied, and no water was introduced. The carbonate lost no less than 16 *per cent.* The shell, particularly the sharp end of the periwinkle, retained its original shape in a great measure, so as to be quite discernible; but the whole was glazed over with a truly vitreous lustre. This glaze covered, at one place, a fragment of the shell which had been originally loose, and had welded the two together. All the angles are rounded by this vitrifaction; the space between the entire shell and the fragment being filled, and the angles of their meeting rounded, with this shining substance. The colour is a pale blue, contrasted, in the same little glass, with a natural piece of periwinkle, which is of a reddish-yellow. One of the fragments had adhered to the lid, and had been converted into a complete drop, of the size of a mustard-seed. It is fixed on the wax (at *b*), along with the other specimens of the experiment (fig. 32.). This result shews, as yet, no sign of decay, notwithstanding so great a loss of weight.

Repetition of the experiment at 34°. fusion.

The last experiment repeated on the same day, and prepared in the same manner, with large fragments of shell, and the point of the periwinkle standing up in the cup. A heat

* Turbo terebra, *Lin.*

heat of 34° was applied; a loss took place of 13° *per cent.* All the original form had disappeared, the carbonate lying in the cup as a complete liquid, with a concave surface, which did not shine, but was studded all over with the white sphericles or tufts, like those seen in the former results, without any space between them. When detached from the cup, the surface moulded on the platina, was white and pearly, with a slight gloss. The mass was quite solid; no vestige whatever appearing, of the original form of the fagments, (fig. 33.). A small piece, broken off near the apex of the cone, shewed the internal structure to be quite saline. In the act of arranging the specimen on its stand, another piece came off in a new direction, which presented to view the most perfect crystalline arrangement: the shining plane extended across the whole specimen, and was more than the tenth of an inch in all directions. This fracture, likewise, shewed the entire internal solidity of the mass. Unfortunately, this specimen has suffered much by the same decay to which all of them are subject which have lost any considerable weight. The part next the outward surface alone remains entire. I have never been able to explain, in a satisfactory manner, this difference of durability; the last-mentioned result having lost more in proportion to its weight than this.

Artificial pure carbonate of lime from Mr. Hatchett. It resisted the heat very much;

About the beginning of June, I received from Mr. Hatchett some pure carbonate of lime, which he was so good as to prepare, with a view to my experiments; and I have been constantly employed with it till within these few days.

My first experiments with this substance were peculiarly unfortunate, and it seemed to be less easily acted upon than any substance of the kind I had tried. Its extreme purity, no doubt, contributed much to this, though another circumstance had likewise had some effect. The powder, owing to a crystallization which had taken place on its precipitation, was very coarse, and little susceptible of close ramming; the particles, therefore, had less advantage than when a fine powder is used, in acting upon each other, and I did not choose to run any risk of contamination, by reducing the substance to a finer powder. Whatever be the cause, it is certain, that in many experiments in which the chalk was changed to marble, this substance remained in a loose and brittle

—was changed into marble,

brittle state, though consisting generally of clear and shining particles. I at last, however, succeeded in obtaining some very good results with this carbonate.

In an experiment made with it on the 18th of June, in a strong heat, I obtained a very firm mass with a saline fracture, moulded in several places on the platina, which was now used in the cylindrical form. On the 23d, in a similar experiment, the barrel failed, and the subject of experiment was found in an entire state of froth, proving its former fluidity. And in another experiment fused.

On the 25th, in a similar experiment, a heat of 64° was applied without any water within the barrel. The platina tube, (having been contaminated in a former experiment with some fusible metal), melted, and the carbonate retaining its cylindrical shape, had fallen through it, so as to touch the piece of porcelain which had been placed next to the platina tube. At the point of contact, the two had run together, as a hot iron runs when touched by sulphur. The carbonate itself was very transparent, resembling a piece of snow in the act of melting. Similar experiment.

On the 26th of June, I made an experiment with this carbonate, which afforded a beautiful result. One grain of water was introduced with great care; yet there was a loss of 6.5 *per cent.*, and the result has fallen to decay. The pyrometer indicated 45°. On the outside of the platina cylinder, and on one of the lids, were seen a set of globules, like pearls, as once before obtained, denoting perfect fusion. When the upper lid was removed, the substance was found to have sunk almost out of sight, and had assumed a form not easily described. (I have endeavoured to represent it in fig. 31. by an ideal section of the platina tube and its contents, made through the axis of the cylinder). The powder, first shrinking upon itself in the act of agglutination, had formed a cylindrical rod, a remnant of which *(a b c)* stood up in the middle of the tube. By the continued action of heat, the summit of the rod (at *a*) had been rounded in fusion, and the mass being now softened, had sunk by its weight, and spread below, so as to mould itself in the tube, and fill its lower part completely *(d f g e)*. At the same time, the viscid fluid adhering to the sides (at *e* and *d*), while the middle part Another experiment with perfect fusion.

part was sinking, had been in part left behind, and in part drawn out into a thin but tapering shape, united by a curved surface (at *b* and *c*) to the middle rod. When the platina tube was unwrapt, the thin edges (at *e* and *d*) were preserved all round, and in a state of beautiful semitransparency. (I have attempted to represent the entire specimen, as it stood on its cone of wax, in fig. 34.). The carbonate, where moulded on the platina, had a clean pearly whiteness, with a saline appearance externally, and in the sun, shone with facettes. Its surface was interrupted by a few scattered air-bubbles, which had lain against the tube. The intervening substance was unusually compact and hard under the knife. The whole surface (*e b a c d*, fig. 31.), and the inside of the air-bubbles, had a vitreous lustre. Thus, every thing denoted a state of viscid fluidity, like that of honey.

These last experiments seem to obviate every doubt that remained with respect to the fusibility of the purest carbonate, without the assistance of any foreign substance.

VII.

Force of the compression discussed.

Measurement of the Force required to constrain the Carbonic Acid.—Apparatus with the Muzzle of the Barrel upwards, and the weight acting by a long Lever.—Apparatus with the Muzzle downwards.—Apparatus with Weight acting directly on the barrel.—Comparison of various results.

In order to determine, within certain limits at least, what force had been exerted in the foregoing experiments, and what was necessary to ensure their success, I made a number of experiments, in a mode nearly allied to that followed by Count Rumford, in measuring the explosive force of gunpowder.

The apparatus consisted of a barrel of iron, into which the materials were put, and a cover was applied on the mouth and kept down by a loaded lever,

I began to use the following simple apparatus in June 1803. I took one of the barrels, made as above described, for the purpose of compression, having a bore of 0.75 of an inch*, and dressed its muzzle to a sharp edge. To this barrel was firmly screwed a collar of iron (*a a*, fig. 36. See quarto Plate IV. in this volume.) placed at a distance of about three inches from the muzzle, having two strong bars (*b b*) projecting

* This was the size of barrel used in all the following experiments, where the fact is not otherwise expressed.

at right angles to the barrel, and dressed square. The barrel, thus prepared, was introduced, with its breech downwards, into the vertical muffle (fig. 35.); its length being so adjusted, that its breech should be placed in the strongest heat; the two projecting bars above described, resting on two other bars (c c, fig. 35.) laid upon the furnace to receive them; one upon each side of the muffle. Into the barrel, so placed, was introduced a cradle, containing carbonate, with all the arrangements formerly mentioned; the rod connected with it being of such length, as just to lie within the muzzle of the barrel. The liquid metal was then poured in till it filled the barrel, and stood at the muzzle with a convex surface; a cylinder of iron, of about an inch in diameter, and half an inch thick, was laid on the muzzle (fig. 35. and 37.), and to it a compressing weight was instantly applied. This was first done by the pressure of a bar of iron (*d e*, fig. 35.), three feet in length, introduced loosely into a hole (*d*), made for the purpose in the wall against which the furnace stood; the distance between this hole and the barrel being one foot. A weight was then suspended at the extremity of the bar (*e*), and thus a compressing force was applied, equal to three times that weight. In the course of practice, a cylinder of lead was substituted for that of iron, and a piece of leather was placed between it and the muzzle of the barrel, which last being dressed to a pretty sharp edge, made an impression in the lead: to assist this effect, one smart blow of a hammer was struck upon the bar, directly over the barrel, as soon as the weight had been hung on.

—the fusible metal was kept fluid all the time.

It was essential, in this mode of operation, that the whole of the metal should continue in a liquid state during the action of heat; but when I was satisfied as to its intensity and duration, I congealed the metal, either by extinguishing the furnace entirely, or by pouring water on the barrel. As soon as the heat began to act, drops of metal were seen to force themselves between the barrel and the leather, following each other with more or less rapidity, according to circumstances. In some experiments, there was little exudation; but few of them were entirely free from it. To save the metal thus extruded, I placed a black-lead crucible, having its bottom perforated, round the barrel, and luted close to it, (fig. 37.);

 some

some sand being laid in this crucible, the metal was collected on its surface. On some occasions, a sound of ebullition was heard during the action of heat; but this was a certain sign of failure.

Results.

The results of the most important of these experiments, have been reduced to a common standard in the second table placed in the Appendix; to which reference is made by the following numbers.

Experiment under the pressure of about 600lb. on the round inch, or about 50 atmospheres.

No. 1.—On the 16th of June 1803, I made an experiment with these arrangements. I had tried to use a weight of 30lb. producing a pressure of 90 lb., but I found this not sufficient. I then hung on a weight of 1 cwt., or 112 lb.; by which a compressing force was applied of 3 cwt. or 336 lb. Very little metal was seen to escape, and no sound of ebullition was heard. The chalk in the body of the large tube was reduced to quicklime; but what lay in the inner tube was pretty firm, and effervesced to the last. One or two facettes, of good appearance, were likewise found. The contents of the small tube had lost but 2.6 *per cent.*; but there was a small visible intrusion of metal, and the result, by its appearance, indicated a greater loss. I considered this, however, as one point gained; that being the first tolerable compression accomplished by a determinate force. The pyrometer indicated 22°.

The experiment was repeated the same day, when a still smaller quantity of metal escaped at the muzzle; but the barrel had given way below, in the manner of those that have yielded for want of sufficient air. Even this result was satisfactory, by shewing that a mechanical power, capable of forcing some of the barrels, could now be commanded. The carbonate in the little tube had lost 20 *per cent.*; but part of it was in a hard and firm state, effervescing to the last.

Another experiment.

No. 2.—On the 21st June, I made an experiment with another barrel, with the same circumstances. I had left an empty space in the large tube, and had intended to introduce its muzzle downwards, meaning that space to answer as an air-tube; but it was inverted by mistake, and the tube entering with its muzzle upwards, the empty space had of course filled with metal, and thus the experiment was made without any included air. There was no pyrometer used; but the heat was guessed to be about 25° where the subject of experiment lay.

ky. The barrel, when opened, was found full of metal, and the cradle being laid flat on the table, a considerable quantity of metal ran from it, which had undoubtedly been lodged in the vacuity of the large tube. When cold, I found that vacuity still empty, with a plating of metal. The tube was very clean in appearance, and, when shaken, its contents were heard to rattle. Above the little tube, and the cylinder of chalk, I had put some borax and sand, with a little pure borax in the middle, and chalk over it. The metal had not penetrated beyond the borax and sand, by a good fortune peculiar to this experiment; the intrusion of metal in this mode of execution, being extremely troublesome. The button of chalk, was found in a state of clean white carbonate, and pretty hard, but without transparency. The little tube was perfectly clean. Its weight with its contents, seemed to have suffered no change from what it had been when first introduced. Attending, however, to the balance with scrupulous nicety, a small preponderance did appear on the side of the weight. This was done away by the addition of the hundredth of a grain to the scale in which the carbonate lay, and an addition of another hundredth produced in it a decided preponderance. Perhaps, had the tube, before its introduction, been examined with the same care, as great a difference might have been detected; and it seems as if there had been no loss, at least not more than one-hundredth of a grain, which on 10.95 grains, amount to 0.0912, say 0.1 *per cent*. The carbonate was loose in the little tube, and fell out by shaking. It had a yellow colour, and compact appearance, with a stony hardness under the knife, and a stony fracture; but with very slight facettes, and little or no transparency. In some parts of the specimen, a whitish colour seemed to indicate partial calcination. On examining the fracture, I perceived, with the magnifier, a small globule of metal, not visible to the naked eye, quite insulated and single. Possibly the substance may have contained others of the same sort, which may have compensated for a small loss, but there could not be many such, from the general clean appearance of the whole. In the fracture, I saw here and there small round holes, seeming, though imperfectly, to indicate a beginning of ebullition.

I made a number of experiments in the same manner, that is

Method of operating with the mouth of the

barrel downwards.

is to say, with the muzzle of the barrel upwards, in some which I obtained very satisfactory results; but it was only by chance that the substance escaped the contamination of the fusible metal; which induced me to think of another mode of applying the compressing weight with the muzzle of the barrel downwards, by which I expected to repeat, with a determinate weight, all the experiments formerly made in barrels closed by congealed metal; and that, by making use of an air-tube, the air, rising to the breech, would secure the contents of the tube from any contamination. In this view, the barrel was introduced from below into the muffle with its breech upwards, and retained in that position by means of a hook fixed to the furnace, till the collar was made to press up against the grate, by an iron lever, loaded with a weight, and resting on a support placed in front. In some experiments made in this way, the result was obtained very clean, as had been expected; but the force had been too feeble, and when it was increased, the furnace yielded upwards by the mechanical strain.

Description of the apparatus

I found it therefore necessary to use a frame of iron, (as in fig. 38.; the frame being represented separately in fig. 39.), by which the brick-work was relieved from the mechanical strain. This frame consisted of two bars (*a b* and *f c*, fig. 38. and 39. quarto Plate III.), fixed into the wall, (at *a* and *f*.) passing horizontally under the furnace, one on each side of the muffle, turning downwards at the front, (in *b* and *c*), and meeting at the ground, with a flat bar (*c d*), uniting the whole. In this manner, a kind of stirrup (*b c d e*) was formed in front of the furnace, upon the cross bar (*c d*, of which a block of wood (*h h*, fig. 38.), was placed, supporting an edge of iron, upon which the lever rested; the working end of the lever (*g*) acting upwards. A strain was exerted, by means of the barrel and its collar, against the horizontal bars, (*a b* and *f c*), which was effectually resisted by the wall (at *a* and *f*) at one end of these bars, and by the upright bars (*c b* and *d e*) at the other end. In this manner the whole strain was sustained by the frame, and the furnace stood without injury.

The iron bar, at its working end, was formed into the shape of a cup, (at *g*), and half filled with lead, the smooth surface of which, was applied to the muzzle of the barrel.

The

The lever, too, was lengthened, by joining to the bar of iron, a beam of wood, making the whole ten feet in length. In this manner, a pressure upwards was applied to the barrel, equal to the weight of 10 cwt.

Observations on this mode of experiment.

. In the former method, in which the barrel stood with its muzzle upwards, the weight was applied while the metal was liquid. In this case, it was necessary to let it previously congeal, otherwise the contents would have run out in placing the barrel in the muffle, and to allow the liquefaction essential to these trials, to be produced by the propagation of heat from the muffle downwards. This method required, therefore, in every case, the use of an air-tube; for without it, the heat acting upon the breach, while the metal at the muzzle was still cold, would infallibly have destroyed the barrel. A great number of these experiments failed, with very considerable waste of the fusible metal, which, on these occasions was nearly all lost. But a few of them succeeded, and afforded very satisfactory results, which I shall now mention.

Successful operations.

In November 1803, some good experiments were made in this way, all with a bore of 0.75, and a pressure of 10 cwt.

No. 3.—On the 19th, a good limestone was obtained in an experiment made in a temperature of 21°, with a loss of only 1.4 *per cent*.

No. 4.—On the 22, in a similar experiment, there was little exudation by the muzzle. The pyrometer gave 31°. The carbonate was in a porous, and almost frothy state.

No. 5.—In a second experiment, made the same day, the heat rose to 37° or 41°. The substance bore strong marks of fusion, the upper part having spread on the little tube: the whole was very much shrunk, and run against one side. The mass sparkling and white, and in a very good state.

Failure of one of the barrels.

No. 6.—On the 25th, an experiment was made with chalk, and some fragments of snail shell, with about half a grain of water. The heat had risen to near 51° or 49°. The barrel had been held tight by the beam, but was rent and a little swelled at the breech. The rent was wide, and such as has always appeared in the strongest barrels when they failed. The carbonate was quite calcined, it had boiled over the little tube, and was entirely in a frothy state, with large and distinctly

tinctly rounded air-holes. The fragments of shell which had occupied the upper part of the little tube, had lost every trace of their original shape in the act of ebullition and fusion.

The compressing weight lifted with explosion.

No. 7.—On the 26th a similar experiment was made, in which the barrel was thrown open, in spite of this powerful compressing force, with a report like that of a gun; (as I was told, not having been present), and the bar was found in a state of strong vibration. The carbonate was calcined, and somewhat frothy, the heart of one piece of chalk used was in a state of saline marble.

Operations without an included air tube.

It now occurred to me to work with a compressing force, and no air-tube, trusting, as happened accidentally in one case, that the expansion of the liquid would clear itself by gentle exudation, without injury to the carbonate. In this mode, it was necessary, for reasons lately stated, to place the muzzle upwards. Various trials made thus, at this time, afforded no remarkable results. But I resumed the method, with the following alteration in the application of the weight, on the 27th of April 1804.

A direct compressing weight used and the lever rejected.

I conceived that some inconvenience might arise from the mode of employing the weight in the former experiments. In them it had been applied at the end of the bar, and its effect propagated along it, so as to press against the barrel at its other extremity. It occurred to me, that the propagation of motion in this way, requiring some sensible time, a considerable quantity of carbonic acid might escape by a sudden eruption, before that propagation had taken effect. I therefore thought, that more effectual work might be done, by placing a heavy mass, (fig. 40.), so as to act directly and simply upon the muzzle of the barrel; this mass being guided and commanded by means of a powerful lever, *(a b)*. For this purpose, I procure an iron roller, weighing 3 cwt. 7lb., and suspended it over the furnace, to the end of a beam of wood, resting on a support near the furnace, with a long arm guided by a rope *(c c)* and pulley *(d)*, by which the weight could be raised or let down at pleasure.

With this apparatus I made some tolerable experiments; but I found the weight too light to afford certain and steady results of the best quality. I therefore procured at the foundry a large mass of iron *(f)*, intended, I believe, for driving piles, and

and which, after allowing for the counterpoise of the beam, gave a direct pressure of 8.1 cwt; and I could, at pleasure, diminish the compressing force, by placing a bucket (*e*) at the extremity of the lever, into which I introduced weights, whose effect on the ultimate great mass, was known by trial. Many barrels failed in these trials: at last, I obtained one of small bore, inch 0.54, which gave two good results on the 22d of June 1804.

No. 8.—Wishing to ascertain the least compressing force by which the carbonate could be effectually constrained in melting heats, I first observed every thing standing firm in a heat of above 20°; I then gradually threw weights into the bucket, till the compressing force was reduced to 2 cwt. Till then, things continued steady; but, on the pressure being still further diminished, metal began to ooze out at the muzzle, with increasing rapidity. When the pressure was reduced to 1½ cwt. air rushed out with a hissing noise. I then stopped the experiment, by pouring water on the barrel. The piece of chalk had lost 12 *per cent*. It was white and soft on the outside, but firm and good in the heart.

Trial of the least compressing force for effectual confinement of the carbonate in a fused heat. It was about 260 lbs. on the round inch, or near 22 atmospheres.

No. 9.—An experiment was made with chalk, in a little tube; to this, one grain of water was added. I had intended to work with 4 cwt. only; but the barrel was no sooner placed, than an exudation of metal began at the muzzle, owing, doubtless, to the elasticity of the water. I immediately increased the pressure to 8.1 cwt. by removing the weight from the bucket, when the exudation instantly ceased. I continued the fire for three quarters of an hour, during which time no exudation happened; then all came out remarkably clean, with scarcely any contamination of metal. The loss amounted to 2.58 *per cent*. The substance was tolerably indurated, but had not acquired the character of a complete stone.

Experiment with water included.

In these two last experiments, the bore being small, a pyrometer could not be admitted.

On the 5th of July 1804, I made three very satisfactory experiments of this kind, in a barrel with the large bore of 0.75 of an inch.

No. 10.—was made with a compressing force of only 3 cwt. A small eruption at the muzzle being observed, water was thrown

Experiments with the least pressures.

thrown on the barrel: the pyrometer gave 21°: the chalk was in a firm state of limestone.

No. 11.—with 4 cwt. The barrel stood without any eruption or exudation, till the heat rose to 25°. There was a loss of 3.6 *per cent.*: the result was superior, in hardness and transparency, to the last, having somewhat of a saline fracture.

No. 12.—with 5 cwt. The result, with a loss of 2.4 *per cent.*, was of a quality superior to any of those lately obtained.

Deductions of the pressures for forming limestone, marble, and fused carbonate expressed in atmospheres and in depths of the sea.

These experiments appear to answer the end proposed, of ascertaining the least pressure, and lowest heat, in which limestone can be formed. The results, with various barrels of different sizes, agree tolerably, and tend to confirm each other. The table shews, when we compare numbers 1, 2, 8, 10, 11, 12, That a pressure of 52 atmospheres, or 1700 feet of sea, is capable of forming a limestone in a proper heat. That under 86 atmospheres, answering nearly to 3000 feet, or about half a mile, a complete marble may be formed: and lastly, that with a pressure of 173 atmospheres, or 5700 feet, that is, little more than one mile of sea, the carbonate of lime is made to undergo complete fusion, and to act powerfully on other earths.

(To be continued.)

V.

Investigation of the Temperature at which Water is of greatest Density; from the Experiments of Dr. Hope, *on the Contraction of Water by Heat at low Temperatures. By* J. Dalton.

(*Concluded from* vol. iii. p. 380.)

To Mr. NICHOLSON.

SIR,

Farther consideration of Dr. Hope's experiments.

In my last letter I apprehend it has been shewn that the results of Dr. Hope's first experiment demonstrate that water is densest at or near the 360°. I now proceed to shew that the remaining five experiments, as far as they apply, tend to establish the same conclusion.

The

In the first interval, which commences with the change of the current from down to up, we observe the top loses 4°, and the bottom none. The bottom losing nothing is unaccountable, when the ascending force was a minimum, except on the supposition that the cooling liquid at bottom during this interval was 38 or 40° instead of 32°. The third interval gives 2° descent at bottom, and less than 1° at top; this cannot be the effect of an *ascending* current; and still less can the changes in the fourth and fifth intervals be ascribed to the same cause.

By frequent trials with a jar of 8 inches deep, and 2½ diameter, containing water of 40°, and plunged into an ice-cold mixture, to the same level, in a jar of 7 inches diameter, I have found the thermometer at bottom always descends to 38° before the top one; this last generally passes the other about 37°, and even after remains lowest: after being 10 minutes at rest the top thermometer is about 35°, the bottom about 37°, and the water at the bottom of the jar at 35°, the air being 40°.

Examination of Dr. Hope's third experiments.

Dr. Hope's third experiment is an instructive one, and the conclusion he derives from it is admitted; namely, that water of 32° is not specifically heavier than water of 40°. But whether are we to infer from it that water of 40° is *equal* to water of 32° in density, or that it is *superior*? I think the former. Whenever a column of water of 32° is situated on another warmer column of the same density, (whether it be 40 or 48°) there can be no current generated from the mutual interchange of heat of the two columns, neither immediately nor remotely. The connecting film, or stratum, will indeed instantly assume the intermediate temperature of greatest density (of 36 or 40°), and the contiguous strata above and below will gradually shade off into warmer and colder water; hence the horizontal strata will be of different specific gravities, but the *perpendicular* columns of particles will all be of the same gravity, and therefore no motion ensue. Now does not the experiment testify that this state takes place when the temperature is at 40°, and not at 48°? Hence we must infer from it that water of 40° is of the same specific

gravity

The second experiment objected to on various accounts.

The second experiment is by far the least satisfactory; indeed it may be shewn that the results are inconsistent with each other; this arises from the difficulty, not to say impossibility, of keeping up a steady ice-cold temperature in a jar containing another with comparatively warm water in it. When we consider that the water in the jar, the air, and the table are incessantly pouring heat into the ice-cold water, and that the warmer water *descends* by the exterior side of the inner jar, and by the interior side of the outer; also that the ice, which is to regulate the temperature, is every moment swimming on the top of the water when not agitated, we must be in doubt what was the *true mean* temperature of the water at the bottom of the jar during the 80 minutes which this experiment continued. From the circumstances, I judge that if the agitation were discontinued 10 minutes, the temperature at bottom would rise from 32 to 36°; however this may have been, we cannot suppose that it could be kept uniformly at 32° by "repeated cautious agitation;" whilst the temperature at top would be constantly at or near 32°. Hence the uncertainty of any conclusion derived from this experiment. If we take the experiment at the 46th minute, when the two thermometers were at 40o, and exhibit the succeeding temperatures with their differences, we shall immediately perceive an irregularity unaccountable on the supposition of uniform exterior temperature of 32°.

Minutes,	Top	Dif.	Bot.	Dif.
46 ——	40		40	
		4 ——		0
52 ——	36	—	40	
		1+—	--	1
58 ——	35	—	39	
		1—	—	2
65 ——	34	—	37	
		0 ——		1
75 ——	34	—	36	
		0 ——		2
103 ——	34	—	34	

second is that in which the liquid still descends, but so as to produce regular strata from the bottom, increasing in temperature upwards to the middle, where it at last becomes of that temperature which is of the same specific gravity as the upper liquid at 32°; the *third* is that in which all the heat acquired ascends into the upper half of the vessel. Let us now see whether the 5th experiment of Dr. Hope will more favour the notion of greatest density at 36 or at 40°. In transcribing his results, I will add a column denoting the temperature at the middle of the vessel, such as I apprehend it would have been found by a thermometer. It must therefore be noticed, that the middle column is an imaginary one, and not obtained from Dr. Hope's observations.

The observations were:

	Bottom.	Middle.	Top.
At commencement,	32	32	32
In 10 minutes,	35	34	32
15	36—	35	32
20	36+	36	32
25	37	37	33
30	38	38	33
38	38+	40	33
45	39—	42	33
50	39+	44	44
55	39+	46	45
60	39+	48	48

Induction that the greatest density is at 36° and not 40°.

Now if we take 30° as the point of greatest density, the first period of time will be 20 minutes, after which, the bottom thermometer was found at 56°+, the middle probably 35 or 36°, and the top one unaffected at 32°; the second period will terminate about 38 or 40 minutes, when the bottom thermometer was 38+, the middle 40° (indicating the same specific gravity as 32°), and the top 33°, having only gained 1°; the third terminates with the experiment; during this period the bottom thermometer has gained 1°, which may easily be accounted for by the natural tendency to equalization of temperature in the lower half of the jar; the middle of the jar, being the focus of heat, may be supposed about equal to the top

top in temperature, and to have gained 8°, whilst the top has rapidly passed from 33° to 48°, an increase of 15°.—If we take 40° for the point of greatest density, the bottom never attains it during the whole experiment, whilst the middle and top gradually arrive at, and surpass it long before the conclusion.

Dr. Hope's sixth experiment with a cooling zone.

The sixth experiment will now be easily explained. A frigorific mixture was put into the vessel surrounding the middle of the tall jar, which contained water of 39½°. The thermometer was observed as under:

		Bottom.		Top.
At commencement,	—	39.5	—	39.5
In 10 minutes,	—	39 +	—	38 +
25 ———	—	39 +	—	36.5
35 ———	—	39	—	36—
55 ———	—	39	—	35
1 h.-10 ———	—	39 —	—	34 +
—— 35 ———	—	39 —	—	34 —
2 — — ———	—	39 —	—	33 +

Reasons for concluding hence that the greatest density is at 36°.

Here the first observation is sufficient of itself to decide by which theory the whole are to be explained. In 10 minutes we observed a fall of nearly half a degree at the bottom, and one of 1½° at top. How will Dr. Hope account for the descent of the bottom thermometer? Water of 36° or 37° cannot descend into water of 39.5, that of less density into greater; it must then be the effect of the propagation of cold downwards by the proper conducting power of the liquid. Granted: but if this was the case, a thermometer in the centre of the jar, should have indicated 32°; and one in the middle, between the centre and the bottom 36°, or thereabouts; for every one allows, that in the proportion of heat (or cold) along any solid body, the effect is produced *gradatim*. This conclusion, however, would ill accord with what was observed in the third experiment. Let us now try this experiment by the other point, or 36°.—The sudden cold applied to the middle of the jar would quickly reduce the contiguous liquid to 36°, and below; this gives it a force of descent by which the tempera-

ture

ture below is reduced; but as soon as it gets to 39°, the force of descent is weaker, and the quick application of cold shortly reduces the marginal temperature to 32°, or below, by which the whole current is turned upwards; a slight diminution of temperature at bottom is, notwithstanding, still perceived as long as any water is found in the jar of 36°, by its constant superintendency downwards.

Conclusion. Thus I apprehend it is made appear that those who have hitherto investigated the point of temperature at which water is most dense, from experiments of a similar nature to those above, have mistaken it; that the true point of greatest density is at 36°, and that the density at 32° and at 40°, or at any other two equal intervals above and below 36°, is nearly the same, progressively diminishing alike by the addition or abstraction of heat from the said point.

I am your's, &c.

J. DALTON.

Manchester, May 12th, 1806.

VI.

Analysis of a new Mineral found in Cornwall. By J. Kidd, *M. D. &c. &c.*

To Mr. NICHOLSON.

SIR,

Oxford, May 9th, 1806.

Analysis of a new mineral. I HAVE taken the liberty of sending you an account of the analysis of a new mineral lately met with in Cornwall; and, if you think it of sufficient consequence, shall be obliged by your giving it a place in your Journal whenever it is convenient.

Its history. The mineral in question was met with in one of the Gwennap mines, and formed an incrustation round projecting particles of a spongy pyrites intermixed with quartz; this pyrites appeared to contain a considerable proportion of cobalt, since it produced a deep blue colour when fused in a very small quantity with glass of borax: the incrustation itself was supposed by the miners to be a variety of wood-tin. Its appearance was altogether

altogether new to those most acquainted with the mineral productions of Cornwall.

External characters.

Its colour both externally and internally varied from a light ash to a dark brown; fracture like that of flint, presenting sections of concentric layers; texture close and polished like that of a nut, and of a silky lustre; hardness about 8 of Kirwan; not easily broken in the mass, but its small fragments very brittle: when triturated giving out a strong hepatic odour; sp. gr. varying from 3.7 to 3.9.

Solulable in acids like a metal.

Soluble in nitric and muriatic acids with effervescence, violently decomposing the former, and giving out sulphureted hydrogen gas in abundance with the latter; in both instances depositing a considerable proportion of sulphur.

Precipitable by alkali,

Precipitable from the above acid solutions by aqua kali in a soft gelatinous form of a light cream colour, but becoming of a pale olive green at 300° Fahr.; sp. gr. of this precipitate about 4.5: the same change of colour took place at the same heat in some earthy calamine from Derbyshire, of the sp. gr. 3.6764.

—and precipitate of potash.

Precipitable entirely from the acid solutions by prussiate of potash; colour of the precipitate a light French grey.

Its habitudes by heat.

By the heat of an argand lamp losing about $\frac{1}{10}$ of its weight, owing to loss of water; in a low red-heat losing about $\frac{14}{100}$ of its weight; by a strong red-heat, in close vessels sublimed, in part, in the form of minute acicular crystals of the silvery appearance of similar crystals of flowers of zinc; in the strongest heat of a moderate forge, sublimed in small prismatic crytals of a brown colour, and adhering firmly to the sides of the crucible; these crystals when viewed through a microscope were in colour and lustre very like brown semitransparent blende, and were soluble in the nitric and muriatic acids with phenomena similar to those attending the solution of blende in the same acids.

It gives fusibility, &c. to platina.

It seems worthy of remark, that a quantity not exceeding 50 grains being reduced to powder and exposed to a moderate forge-heat in a small crucible of platina, prepared by Dr. Wollaston, enclosed in another made of earthen ware, the platina where in contact with the mineral was completely fused, and the remaining part was covered with an iridescent pellicle, and made soft and brittle throughout its substance.

A simi-

Comparative experiments with zinc and blende.

A similar effect was lately produced on a platina spoon, in which some galena was exposed to the blow-pipe.

As the quantity of the mineral under examination detached at different times did not in the whole amount to above 30 grains, of which the greater part had been wasted in the foregoing experiments, an analysis of so small a quantity would not have been published, but that unfortunately another specimen could not be procured of the same mineral,: in order however, to render as satisfactory as possible the results of an analysis conducted on so small a scale, a few comparative experiments were made on zinc, and on blende, of which last substance this mineral evidently appeared to be a variety; an account of these experiments has been added, as not altogether uninteresting in themselves, but principally in confirmation of the analysis: the reagents employed in these, were prepared by Messrs. Allen and Howard; and the precipitates were washed, separated, and dried, without the use of filters, except in the case of the precipitates by prussiate of potash,

Experiments on zinc.

	grs.
Zinc, grs. 5, dissolved in nitric acid (1.47), precipitated by potash, and dried at 300 Fahr. weighed	6.40.
Zinc, grs. 20, dissolved, &c.	24.50.
Zinc, grs. 100, dissolved, &c.	125.

Sp. gr. of these precipitates was about 4.3.

According to Mr. Proust's analysis, (vide Thompson, 2d. edition, vol. 1. p. 199,) the two first of the three preceding precipitates ought to have weighed respectively 6.25, and 25: the weight of the last is correct.

Experiments on blende.

	grs.
Laminated brown blende (sp. gr. 4.0678), grs. 100, dissolved in nitric acid, precipitated by potash, and dried at 300 Fahr. weighed	75.

Sp. gr. of the precipitate 4.54.

Zinc, grs. 5, dissolved in nitric acid, precipitated by prussiate of potash, and dried at 300 Fahr. weighed	16.50.
Zinc, grs. 20, dissolved, &c.	65.
Zinc, grs. 100, dissolved, &c.	330.

The weights of the three last precipitates would bear equal proportions to the respective weights of the zinc taken, if the two first, instead of 16.50 and 65, were 16 and 66.

As

As it was evident that the mineral under examination contained oxide of zinc, and as the usual mode of reducing this (particularly in minute quantities) is extremely difficult, the process of reduction was varied in the following manner. It is generally known, I believe, that by means of the galvanic apparatus, the metals may be precipitated on each other from their acid solutions without regard to the usual order of chemical affinity: the objection to the application of this process, as a means of analysis, arises from the quick re-absorption of the precipitated metal by the disengaged acid from which it had just been separated: but, as Mr. Klaproth has lately observed that the metals may be precipitated by each other from the alkaline solutions of their oxides in the order of their chemical affinity, a portion of oxide of zinc was dissolved in aqua kali, and by means of the galvanic apparatus a precipitation of metallic zinc was very readily obtained on plates of iron, copper, and platina; in this experiment it was accidentally observed, that upon withdrawing the communication with the galvanic apparatus, the recently precipitated zinc was soon re-dissolved by the aqua kali, and not only by the portion of aqua kali, employed in that experiment, but by any other portion also, or even by a solution of sub-carbonate of ammonia; however, by not interrupting the communication, the precipitate remained; and in this way, four parts out of five of metallic zinc were recovered from a solution of its oxide in aqua kali. The re-absorption of the zinc by the aqua kali at first appeared to take place without any effervescence; but upon a closer examination, minute air-bubbles were seen detaching themselves from the surface of the zinc; if these were bubbles of hydrogen gas, as probably they were, originating from the decomposition of the water, an easy explanation is afforded of the solution of the zinc.

Useful application of galvanism in analysis.

By the foregoing experiments were obtained the means of ascertaining, both by calculation and actual reduction, the proportion of zinc in a given weight of its oxide. In order to ascertain the quantity of sulphur acidified during solution in nitric acid, the following experiment was made:

Sulphur, grs. $7\frac{2}{10}$, were boiled in nitric acid till the whole of the sulphur had disappeared: a solution of nitrate of baryt was then added till there was no longer any precipitation; the precipi-

To ascertain the quantity of sulphur acidified by solution in nitric acid.

precipitate being then washed, and dried at a low red-heat, weighed grs. 50, (sp. gr. of it was 3.7826). This experiment must be considered as very satisfactory, from its near agreement with M. Cheneoix's statement; from which it only differs in the proportion of 7.20 to 7.22, (vide Thompson, 2d edit. vol. ii. p. 20.)

Analyses of different portions of the mineral.

It remains to give an account of two or three analyses of different portions of the mineral itself; in which, though the quantity of silex is noticed, yet as its presence is merely accidental, it is excluded from the whole sum in calculating the proportions of the other substances.

I.

	grs.
Quantity taken	30.
Detached particles of minutely xtallized quartz, separated during solution in nitric acid.	0.5.
Sulphur separated during solution	1.6.
Do. by calculation in sulph. of baryt, grs. 54	7.8.
Precipitate by potash, dried at 300 Fahr.	20.
	29.9.

Proportion of sulphur, about $\frac{32}{100}$.
——— of precipitate $\frac{68}{100}$.

II.

	grs.
Quantity taken	13 $\frac{8}{10}$.
Detached particles of quartz	0.4.
Sulphur separated	1.2.
Do. in sulph. of baryt, grs. 20	2.9.
Precipitate by potash	9.
	13.5.

Proportion of sulphur about $\frac{31}{100}$.
——— of precipitate $\frac{66}{100}$.

III.

	grs.
Quantity taken	22.
Driven off by a strong red-heat	8.
Precipitate by potash from nitric acid	13.5.
	21.5.

Proportion of sulphur in the third analysis almost $\frac{38}{100}$.
Proportion of the precipitate by potash not quite $\frac{63}{100}$.

It

It is probable, that in this instance part of the substance precipitable by the potash was driven off by the high degree of heat to which it was exposed: and this would account for the difference in the proportions of this analysis from those of the preceding.

IV.	grs.
Quantity taken	13.
Sulphur separated during solution in nitric acid	1.5.
Ditto in sulph. of baryt, grs. 19	2.7.
Potash precipitate at 300 Fahr	9.
	13.2.

Proportion of sulphur about $\frac{33}{100}$.
——— of precipitate $\frac{69}{100}$.

In this last analysis, the precipitate by potash was afterwards thrown into distilled water, and dissolved, as far as could be, by potash; a dirty-coloured flocculent substance, which remained undissolved, was caught on a filter: this was readily dissolved by muriatic acid, and gave a deep blue colour on the addition of prussiate of potash; but the prussiate of iron thus obtained was too small in quantity to afford a satisfactory examination.

The filtered potash solution was exposed to the action of the galvanic apparatus, and at different times $5\frac{8}{10}$ grains of a metallic substance were recovered from it, which resembled zinc closely in colour, and more closely in its property of burning with a blue flame when thrown on ignited iron, and leaving a white oxide on the surface of the iron.

But $5\frac{8}{10}$ grains of metallic zinc equal $7\frac{25}{100}$ of oxide: therefore 9 grains of the precipitate having been dissolved in the potash, there remained $1\frac{75}{100}$ grains unrecovered (including, that is, the small proportion of iron which had been separated by filtration). The potash solution was now neutralized by nitric acid, and in this manner $\frac{30}{100}$ grains of the original precipitate were recovered; which being added to the $7\frac{25}{100}$ separated by the galvanic apparatus, leaves only $\frac{20}{100}$ of the original 9 grains unaccounted for.

The grains $5\frac{8}{10}$ of metallic zinc obtained in the above manner, when dissolved, in part, in diluted sulphuric acid, gave a perfectly white precipitate with prussiate of potash.

By comparing the foregoing experiments and analyses with each other, this mineral appears to consist of about

33 sulphur,
66 oxide of zinc,
—
99

with a very minute proportion of iron.

I am afraid the preceding account will be thought tedious by those who are in the habit of chemical analysis; but not having been much accustomed to this myself, it seemed the safer way to be as particular as possible in the relation of the foregoing experiments.

I am, Sir,
Your most obedient servant,
J. KIDD.

VI.

Second Essay on the Analysis of Animal Fluids. *By* John Bostock, *M. D. Liverpool.**

Method of analysing compound animal fluids.

In my former essay I endeavoured to ascertain a definite character for the three primary animal fiuds, albumen, jelly, and mucus, and to point out tests by means of which their presence might be detected with facility and precision. I now propose to offer some observations upon the method to be employed in the analysis of those compound fluids, of which the three substances above mentioned form a principal part. I shall arrange my remarks according to the order adopted in my former paper, beginning with the consideration of the albumen.

Separation and proparation of albumen.

My first object was to discover some method by which the exact proportion of this substance might be ascertained in any fluid of which it formed a component part. The application of caloric, as appears from my former experiments, affords a very accurate test of the presence even of the smallest quantity of albumen; but I found that it was not possible, by this agent, to

* Received from the author; who has also inserted it in the Medical Journal. The former Essay is in our Journal, xi. 244.

sepa-

separate it from the water, or other substance, with which it is combined. When a solution containing $\frac{1}{10}$ of its weight of pure albumen was kept for some time at the boiling temperature, the whole fluid assumed an opake and semi-gelatinous appearance; but the water still remained so far attached to the solid matter, that it scarcely passed at all through a filter of bibulous paper: a part of it was not transmitted even after it had lain upon it for several days, and was beginning to exhibit marks of putrefaction. When albumen exists in that state of concentration in which it is found in the white of the egg, *i. e.* composing about 15 parts in the 100, it is capable, as we know, of becoming so completely concreted as to resemble a solid substance, and, if it be divided into small pieces, it may be digested in hot water, without its figure or consistency being affected.

Mere heat has little effect as to complete precipitation.

It appeared a subject of some importance, to ascertain the degree of dilution of which albumen admits without losing this property, as, by this means, some general idea might be formed of the proportion of it in any compound fluid, merely by the application of caloric, in those cases where we may not have it in our power to enter upon a more minute examination. I found that the white of the egg, after being mixed with half its weight of water, still retained the power of becoming so far coagulated, that the figure of its parts, when divided by a knife, was not altered; but that when an equal weight of water was added to the white of the egg, though it was rendered completely opake by heat, yet it still retained some part of its fluidity, so that it might be slowly poured from one vessel to another. In the former case the albumen composed somewhat less than $\frac{1}{10}$ part of the weight of the fluid, and in the second about $\frac{1}{13}$.

One-tenth of albumen in water, renders it solid if heated;—one-thirteenth does not.

I had next recourse to the oximuriate of mercury, which I had before found to be, as it were, the appropriate coagulator of albumen. I experienced, however, the same kind of difficulty in this case, as in the employment of caloric. Notwithstanding the delicacy with which the oximuriate of mercury detects the most minute portion of albumen, I found the coagulation to be so complete, that the fluid continued to retain a considerable degree of opacity, after being passed through a filter, and to be still coagulable by the application of heat, even when it indicated an excess of the oximuriate. The entire separation of the albumen seemed, however, to be attained by the union of both

Oximuriate of mercury presents the same difficulty of heat:

—but this salt aided by heat is effectual.

both these methods, *i. e.* by subjecting the fluid to the boiling temperature, after the addition of a requisite quantity of the oximuriate of mercury. That we may be assured that a sufficient quantity of the metallic salt has been employed, it is necessary that it be added a little in excess; a circumstance which may be easily ascertained, by observing whether the filtered fluid possess the power of precipitating a fresh solution of albumen.

The precipitate contains oximuriate:

The precipitate produced by the joint operation of caloric and the oximuriate of mercury is a compound of albumen and the metallic salt; so that, before we can ascertain the quantity of the former, it will be necessary to learn in what proportion they are disposed to combine with each other. But this point, simple as it may appear, is not unattended with difficulty; it is not easy to collect and detach from the filter a substance of this peculiar texture; and much nicety is requisite in the subsequent drying, so that all the moisture may be completely expelled, and yet that the substance should not experience any commencement of decomposition. Making the experiment with the requisite precautions, it appeared to me that albumen, when coagulated by the addition of the oximuriate of mercury, unites itself to between $\frac{1}{3}$ and $\frac{1}{4}$ of its weight of the salt. If this estimate be confirmed by more extensive experiments, it will be easy to calculate, with tolerable accuracy, the quantity of albumen in any compound animal fluid, by employing a solution of the oximuriate of mercury of a known strength, and observing what quantity it is necessary to saturate a given quantity of the body under examination. If, for example, we find that 100 grains of the fluid require 60 grains of a solution containing $\frac{1}{10}$ of its weight of the oximuriate of mercury, it will follow, that it contains 10.5 grains of albumen.

—quantity.

The uncoagulable part of white of egg is mucus.

Before I leave the subject of albumen, I shall make some remarks upon the uncoagulable part of the white of the egg. I found it very generally to constitute about $\frac{1}{4}$ of the weight of the whole solid contents, as stated in my former essay. A solution of this substance, in about 100 times its weight of water, was not effected by the addition of the oximuriate of mercury, or the decoction of galls, but a single drop of the aqua lithargyri acetati threw down a copious precipitate. I gradually evaporated the fluid, and occasionally stopped the process when it

it was nearly completed; but I did not observe any tendency towards gelatinization, or the exhibition of any crystalline appearance. I concluded, therefore, that it consisted altogether of mucus.

In the course of my experiments on albumen, particularly those made during the summer months, I have observed, that this substance is less disposed to become putrid in its natural state than when diluted with a greater proportion of water, and that a solution of the mucilaginous part, formed by washing the coagulated albumen, was still more subject to decomposition. In some instances, where I permitted a diluted solution of the albumen ovi to become putrid, I was forcibly impressed with the resemblance of its odour to that of pus; whereas the putrid mucilage discharged the usual nauseous smell.

Putrid albumen resembles pus.

With respect to the saline ingredients of the albumen ovi, they seem to exist in very minute proportion. I was never able to detect any visible indication of saline matter by the evaporation of the water in which coagulated albumen had been washed; a considerable precipitate was indeed produced by the addition of the nirate of silver; but I concluded, from its appearance, that at least the greatest part of the effect depended upon the coagulation of the animal matter, though some part of it might be due to the presence of the muriate of soda. The albumen ovi exhibits slight alkaline effects upon the appropriate test papers; and, by means of the oxalic acid, a very minute trace of lime may be detected, which probably exists in combination with the phosphoric acid. In order to ascertain the quantity of alkali, I formed a very diluted alkaline solution of a known strength, and observed how much acetous acid was necessary to neutralize a given weight of it. With the same acetous acid I neutralized a portion of the white of the egg, and, making the necessary calculations, I estimated that 100 grains of the albumen ovi contain no more than $\frac{1}{12}$ of a grain of alkali. This alkali has generally been supposed to be soda, and as this salt is more frequently present in the different parts of the animal body than potash, we may conclude, with some plausibility, that it is soda which exist in the albumen ovi. It has been supposed to exist in the pure or caustic state; but I am not aware of any method by which this circumstance can be ascertained. I added the carbonate of soda to a solution of albumen ovi, in considerably

Albumen ovi contains scarcely any saline matter:

—perhaps muriate of soda;

—a trace of lime;

—and a thousandth part of soda.

siderably greater quantity than that indicated above, yet the addition of the sulphuric acid produced no visible effervescence. I think it must therefore remain undetermined, whether the alkali exist in the pure or carbonated state.

Concerning jelly: It is easily separated, and its quantity determined by tannin.

The method of ascertaining the exact quantity of jelly in any compound fluid is, upon the whole, more easy. Isinglass affords us the means of obtaining jelly in a state of almost perfect purity; by forming a solution of this substance, and an infusion of galls of a known strength, by adding them to each other until they are neutralized, and collecting the precipitate, we can ascertain the respective proportions necessary to produce the neutral compound. As the precipitate formed in this case subsides slowly, it is more convenient, after the mixture of the jelly and the galls, to filter the compound, and to add a little of the filtered fluid to fresh solutions of jelly and galls respectively; from observing in which of the solutions a precipitate is produced, we are enabled to determine which ingredient exists in excess, and to correct the deficiency in a subsequent experiment; this process must be repeated until the filtered fluid produces no precipitate with either of the reagents. By proceeding in this manner I am led to conclude, that the compound formed by the union of jelly and tannin consists of somewhat less than two parts of tannin to three of jelly; as we always have it in our power to ascertain the quantity of tan that we employ, we may, by an easy calculation, deduce the amount of the jelly in any fluid under examination.

The proportion of mucus not easily determined.

I have not yet been able to fall upon a method for directly determining the proportion of mucus in a compound fluid, in consequence of the facility with which goulard decomposes the different ingredients, both animal and saline, which are always to be suspected in those substances that contain mucus, even in a state the nearest approaching to purity. Muriate of soda is, I believe, always present wherever we have mucus; and the goulard, which so readily and completely precipitates the mucus, likewise decomposes the common salt. The nitro-muriate and the muriate of tin, and the nitro-muriate of gold, all cause a considerable precipitation in a solution of salvia; but the supernatant fluid remains opake, as if it still contained some animal matter; and, in consequence of the muriatic acid which enters into the composition of these salts, we are not able after-

afterwards to search for the muriate of soda, by applying the test of the nitrate of silver. The nitrate of silver itself, although it scarcely porduces any effect upon a solution of vegetable gum, when it is added to saliva, throws down a very copious precipitate, partly of a dense powder, and partly of a flocculent matter: This, I apprehend, proceeds from its acting both upon the mucus and the muriate of soda. The nitro-muriates of tin and of gold do not decompose common salt, but they precipitate albümen as well as mucus, and on this account cannot be employed. The only way of proceeding that I have been hitherto able to employ is to discover the quantity of albumen and of jelly by the methods mentioned above, and after deducting their weight from the whole of the solid contents, to consider the remainder as mucus; but here we are necessarily confounding the mucus and the salts. After this statement, I need not add that the subject still requires farther elucidation.

Indirect method.

I have attempted, in a few cases, to apply my ideas respecting the analysis of animal fluids to the actual examination of some substances, and shall now proceed to detail my experiments. I must premise that, in the two first analyses, the small quantity upon which I was obliged to operate prevented me from determining the proportion of the ingredients as accurately as I could have wished. I have nevertheless inserted them, as these fluids are not at all times to be procured.

Examination of some other animal fluids by these methods.

The first set of experiments which I performed were upon the fluid discharged, by puncturing a tumour formed on the spine in the disease which is usually called *spina bifida*.

Discharge from a tumour.

1. The fluid was colourless, slightly opake, and gelatinous, of a specific gravity, scarcely differing from that of water, and insipid.

2. It did not affect either litmus or an infusion of mallows.

3. A hundred grains of the fluid were slowly evaporated; a residuum was left of 2.2 grains only.

4. When kept for some time at the temperature of boiling water, its opacity was slightly increased, but it did not exhibit any tendency to coagulation.

5. A saturated solution of the oximuriate of mercury, when first added, produced but little effect; after some time, however, an inconsiderable precipitate was thrown down.

 6. In-

Examination of the liquid from a tumour.

6. Infusion of galls produced a precipitate in small quantity.

7. Aqua lithargyri acetati produced a copious dense precipitate.

8 By the addition of the nitro-muriate of tin, the fluid was rendered considerably more opake, and, as if approaching to coagulation after some time a precipitate was formed.

9. The residuum from No. 3. was partly dissolved by being digested in hot water.

10. The water from No. 9. produced a copious precipitate with nitrate of silver.

11. It also produced a perceptible precipitate with oxalic acid.

12. It also produced a slight precipitate with the infusion of galls.

13. A quantity of this fluid, being evaporated very slowly, left cubical crystals of common salt in considerable quantity.

From No. 3. we learn that 97.8 parts in 100 consist of water. From 4. and 5. we learn that it contained a little albumen. The quantity was too small to be collected and measured by weighing; but, from the visible effect produced by heat and the oximuriate of mercury, I should conceive it could not be more than $\frac{1}{100}$ of its weight. From No. 6. and 12. and by comparing 6. with 5. we learn that it contains a minute quantity of jelly. From 7. and 8. especially by comparing them together, we learn that it contains mucus. By comparing 7. and 8. and from 10. and 13. we learn that it contains the muriate of soda in considerable quantity; and from 11. that it contains a very small trace of lime. The composition of the fluid will therefore be nearly as follows:

Water	97 8	
Muriate of soda	1 0	
Albumen	0 5	
Mucus	0 5	These proportions are in some measure conjectural.
Jelly	0 2	
Lime		a very minute quantity.
	100 0	

Examination of liquor pericardii.

The next fluid that I had an opportunity of examining was the *liquor pericardii*, which was obtained by opening the body of a boy

a boy who had died suddenly, in order to ascertain the cause of his death. The whole quantity collected was about half an ounce; it was nearly of the colour and appearance of the serum of the blood. Examination of the liquor pericardii.

1. A quantity of it was slowly evaporated, and a residuum was left, amounting to $\frac{1}{12}$ of the whole.

2. A quantity of the fluid was kept for some time at the heat of boiling water; it became considerably opake and gelatinous.

3. A copious precipitate was produced by the oximuriate of mercury.

4. After the fluid was saturated with the oximuriate of mercury, it produced no precipitate with the infusion of galls.

5. The nitrate of silver produced a precipitate which indicated both animal matter and the muriate of soda.

6. A quantity of the coagulated fluid, No. 2, being dried in the temperature of boiling water, was afterwards washed with boiling distilled water.

7. The water from No. 6. gave no precipitate with the oximuriate of mercury, nor with galls, but a pretty copious one with the aqua lithargyri acetati.

The small quantity of the fluid which I was able to obtain prevented me from prosecuting the analysis with more minuteness; from these experiments, however, we may form some idea of its composition. From the 1. we learn that it contains 92 of water; from No. 2. and 3. that it contains a considerable quantity of albumen, which I should estimate at somewhat more than $\frac{1}{20}$ of its weight. No. 4. and 7. show that it contained no jelly. No. 7. that it contained mucus; and No. 5. that it contained common salt, but the proportion of this latter appeared not very considerable. The constituents of the liquor pericardii will therefore be:

Water	92 0	
Albumen	5 5	
Mucus	2 0	The proportion of these substances is somewhat conjectural.
Muriate of soda	0 5	
	100 0	

The next analysis that I attempted was that of the saliva; this fluid, in its natural state, is mixed with such variable Examination of the saliva.

 pro-

Examination of the saliva; proportions of water, that it is almost impossible to fix any standard which can be considered even as the average quantity. It is, however, convenient, in observing the effects of reagents upon it, to have it in a more diluted state than it usually occurs; and I accordingly united it, by rubbing in a mortar, with a quantity of distilled water, until, by evaporation, 100 grains of the mixture were found to contain two grains of solid residuum. Upon this mixture the following experiments were performed.

1. The fluid was still opake, and there was an appearance as if some flocculent matter were suspended in it.

2. No effect seemed to be produced by exposing it to the boiling temperature.

3. When the oximuriate of mercury was added, no immediate visible effect was produced, but after some hours, a light flocculent coagulum separated and fell to the bottom, having the fluid nearly transparent.

4. A portion of the fluid, left for a few days without addition, gradually suffered a quantity of matter to separate from it, as in No. 3.; but the separation was less complete, and it was much longer in taking place.

5. A quantity of the fluid being passed through a filter of bibulous paper, was rendered perfectly transparent.

6. The oximurate of mercury being added to a quantity of No. 5. a very slight precipitate only was produced after some time.

7. The addition of the infusion of galls to No. 1. caused a precipitation of white flakes; but, after filtration, the galls produced no effect.

8. The filtered fluid, No. 5. produced a copious precipitate with the aqua lithargyri acetati.

9. It also produced a considerable precipitate with the nitro-muriate of tin.

10. And with the nitrate of silver.

11. Equal weights of the fluid, before and after filtration, were separately evaporated, and the amount of the residuum being ascertained, the quantities left were to each other nearly as 12 to 8.

12. The diluted saliva, both before and after filtration, slightly reddened a paper stained with litmus.

From

From these experiments we may draw the following conclusions: From No. 3. it would seem that the fluid contains albumen; but it appears from Nos. 1. 2. 4. 5. and 6. that the albumen is not soluble in water, but in that state in which it is found after coagulation. From this we learn that it constitutes only 0.8 of a grain in 100 grains of the fluid. From No. 7. we learn that there is no jelly; from 8. 9. and 10. that there is a quantity of mucus and muriate of soda; and, from comparing these with each other, we are led to conclude, that the last substance exists only in small quantity. The composition of the diluted saliva will therefore be nearly as follows: Examination of the saliva.

Water	98 0	
Coagulated albumen	0 8	
Mucus	1 1	The proportion of these is partly conjectural.
Salts	0 1	
	100 0	

It will, I conceive, be admitted, that the albumen in this saliva existed in coagulated state. This I consider to be decidedly proved from the effects of heat, by its gradual, spontaneous deposition, and by the ease with which it was separated by filtration. Still, however, the oximuriate of mercury and the galls showed that it was albumen. The difficulty of uniting saliva with water, and the effects of filtration, were noticed by Dr. Fordyce;* but he imagined that the whole of the animal matter was removed by the process. The saliva that I employed manifested slightly acid properties: How far this may be the case in general, I am unable to decide. Haller thinks that, in a state of perfect health, the saliva is not acid; but, at the same time, he quotes a number of authors who are of a contrary opinion. † M. Hapell de la Chenaie informs us, that the saliva of the horse is alkaline. ‡

The quantity of water contained in the saliva, as discharged from the mouth, is very various. Haller estimates it at about $\frac{4}{5}$ of the whole; but Dr. Fordyce supposes that $\frac{1}{12}$ only consists of solid matter. If we take the estimate of Haller,

* De Catarrho, p. 17.
† El. Phys. lib. xviii. sect. 2. § 10.
‡ Mem. of Med. Soc. for 1780-1, p. 325

which

Examination of the saliva.

which is sanctioned by Fourcroy * and Thomson †, the constituents of saliva will exist in the following proportions;

Water	80	0
Coagulated albumen	8	0
Mucus	11	0
Saline substances	1	0
	100	0

The quantity of the saline ingredients in my analysis is confessedly conjectural; they have been stated by Haller to be $\frac{1}{100}$ of the whole. I have not been able to satisfy myself respecting the nature and proportion of the salts which compose this residuum: it has been said to consist of the muriate of soda, and the phosphates of lime and of soda.*

VII.

The Report of a Committee of the Horticultural Society of London, drawn up at their request by T. A. KNIGHT, *Esq.; and ordered to be immediately published by the Council.*‡

The primeval state of those vegetables which are cultivated in gardens is little known.

WERE it possible to ascertain the primeval state of those vegetables which now occupy the attention of the gardener and agriculturist, and immediately, or remotely conduce to the support and happiness of mankind; and could we trace out the various changes which art or accident has, in successive generations, produced in each, few inquiries would be more extensively interesting. But we possess no sources from which sufficient information to direct us in our inquiries can be derived; and are still ignorant of the native country, and existence in a wild state, of some of the most important of our plants. We, however, know that improved flowers and fruits

* Systeme, ix. 366. † Chemistry, iv. 613.

‡ It is printed in eight quarto pages. I shall shortly have the pleasure of giving some account of this very respectable and useful society. N.

are

are the necessary produce of improved culture; and that the offspring, in a greater or less degree, inherits the character of its parent. The austere crab of our woods has thus been converted into the golden pippin; and the numerous varieties of the plumb, can boast no other parent than our native sloe. Yet few experiments have been made, the object of which has been new productions of this sort; and almost every ameliorated variety of fruit appears to have been the offspring of accident, or of culture applied to other purposes. We may therefore infer, with little danger of error, that an ample and unexplored field for future discovery and improvement lies before us, in which nature does not appear to have formed any limits to the success of our labours, if properly applied.

Still greater improvements may be expected from direct culture.

The physiology of vegetation has deservedly engaged the attention of the *Royal* and *Linnean* Societies; and much information has been derived from the exertions of those learned bodies. Societies for the improvement of domestic animals, and of agriculture in all its branches, have also been established, with success, in almost every district of the British empire. Horticulture alone appears to have been neglected, and left to the common gardener, who generally pursues the dull routine of his predecessor; and, if he deviates from it, rarely possesses a sufficient share of science and information to enable him to deviate with success.

Objects of various eminent societies. Horticulture has not hitherto met with public patronage and research.

The establishment of a national Society for the improvement of horticulture has therefore long been wanted; and if such an institution meet with a degree of support proportionate to the importance of its object; if it proceed with cautious circumspection to publish well ascertained facts only, to detect the errors of ignorance, and to expose the misrepresentations of fraud; the advantages which the public may ultimately derive from the establishment, will probably exceed the most sanguine hopes of its founders.

Horticultural Society.

Horticulture, in its present state, may with propriety be divided into two distinct branches, the useful, and the ornamental: the first must occupy the principal attention of the members of the society, but the second will not be neglected; and it will be their object, wherever it is practicable, to combine both.

Horticulture; useful and ornamental.

Experi-

The wild plants bear changes of climate better than the cultivated.

Experience and observation appear to have sufficiently proved, that all plants have a natural tendency to adapt their habits to every climate in which art or accident places them: and thus the pear tree, which appears to be a native of the southern parts of Europe, or the adjoining parts of Asia, has completely naturalized itself in Britain, and has acquired, in a great number of instances, the power to ripen its fruit in the early part even of an unfavourable summer: the crab tree has in the same manner, adapted its habits to the frozen regions of Siberia. But when we import either of these fruits, in their cultivated state, from happier climates, they are often found incapable of acquiring a perfect state of maturity even when trained to a south wall.

Whence the vine and peach may probably flourish here without artificial heat.

As the pear and crab tree, in the preceding cases, have acquired powers of ripening their fruits in climates much colder than those in which they were placed by nature, we have some grounds of hope the vine and peach tree may be made to adapt their habits to our climate, and to ripen their fruits without the aid of artificial heat, or the reflection of a wall: and though we are at present little acquainted with the mode of culture best calculated to produce the necessary changes in the constitution and habit of plants, attentive observation and experience will soon discover it; and experiments have already been made, which prove the facility of raising as fine varieties of fruit in this country, as any which have been imported from others.

Propagation by seed and by section.

Almost every plant, the existence of which is not confined to a single summer, admits of two modes of propagation; by Division of its Parts, and by Seed. By the first of these methods we are enabled to multiply an individual into many; each of which, in its leaves, its flowers, and fruit, permanently retains, in every respect, the character of the parent stock. No new life is here generated; and the graft, the layer, and cutting, appear to possess the youth and vigour, or the age and debility of the plant, of which they once formed a part*.

* The diseased state of young grafted trees of the *golden pippin*, and the debasement of the flavour of that fruit, afford one, amongst a thousand instances which may be adduced, of the decay of those varieties of fruit which have been long propagated by grafting, &c.

No

No permanent improvement has therefore ever been derived, or can be expected, from the art of the grafter, or the choice of stocks of different species, or varieties; for to use the phrase of Lord Bacon, the graft in all cases *overruleth the stock*, from which it receives *aliment, but no motion.* Seedling plants, on the contrary, of every cultivated species, sport in endless variety. By selection from these therefore we can only hope for success in our pursuit of new and improved varieties of each species of plant or fruit; and to promote experiments of this kind the Horticultural Society propose to give some honorary premiums to those who shall produce before them, or such persons as they shall appoint, valuable new varieties of fruit, which having been raised from seeds, have come into existence since the establishment of the institution.

Grafting does not improve the variety. Seedling plants are indefinitely various.

In the culture of many fruits, without reference to the introduction of new varieties, the Society hope to be able to point out some important improvements. Several sorts, the walnut and mulberry for instance, are not produced till the trees have acquired a very considerable age, and therefore, though the latter fruit is highly valued, it is at present very little cultivated. But experiments have lately been made, which prove that both walnut and mulberry trees may be readily made to produce fruit at three years old; and there appears every reason to believe that the same mode of culture would be equally successful in all similar cases.

Improvements as to early bearing, &c.

In training wall trees there is much in the modern practice which appears defective and irrational: no attention whatever is paid to the form which the species or variety naturally assumes; and be its growth upright, or pendent, it is constrained to take precisely the same form on the wall.

Training wall trees.

The construction of forcing houses appears also to be generally very defective, and two are rarely constructed alike though intended for the same purposes; probably not a single building of this kind has yet been erected, in which the greatest possible quantity of space has been obtained, and of light and heat admitted, proportionate to the capital expended. It may even be questioned, whether a single hot bed has ever been made in the most advantageous form; and

Construction of forcing houses.

and the proper application of glass, where artificial heat is not employed, is certainly very ill understood.

Application of manure to trees.

Every gardener is well acquainted with methods of applying manure, with success, to annual plants; for these, as Evelyn has justly observed, *having but little time to fulfil the intentions of nature*, readily accept nutriment in almost any form in which it can be offered them: but trees, being formed for periods of longer duration, are frequently much injured by the injudicious and excessive use of manure. The gardener is often ignorant of this circumstance; and not unfrequently forms a compost for his wall trees, which for a few years stimulating them to preternatural exertion, becomes the source of disease, and early decay.

Best varieties of soils.

It is also generally supposed that the same ingredients, and in the same proportion to each other, which are best calculated to bring one variety of any species of fruit to perfection, are equally well adapted to every other variety of that species: But experience does not justify this conclusion; and the peach in many soils acquires a high degree of perfection, where its variety, the nectarine, is comparatively of little value; and the nectarine frequently possesses its full flavour in a soil which does not well suit the peach. The same remark is also applicable to the pear and apple; and as defects of opposite kinds occur in the varieties of every species of fruit, those qualities in the soil which are beneficial in some cases, will be found injurious in others. In those districts where the apple and pear are cultivated for cider and perry, much of the success of the planter is found to depend on his skill, or good fortune, in adapting his fruits to the soil.

And numerous other fields of improvement offer themselves.

The preceding remarks are applicable to a part only of the objects, which the Horticultural Society have in view; but they apply to that part in which the practice of the modern gardener is conceived to be most defective, and embrace no inconsiderable field of improvement.

In the execution of their plan, the committee feel that the society have many difficulties to encounter, and, they fear, some prejudices to contend with; but they have long been convinced, as individuals, and their aggregate observations have tended only to increase their conviction, that there scarce exists a single species of esculent plant or fruit, which

(relative

(relative to the use of man) has yet attained its utmost state of perfection; nor any branch of practical horticulture which is not still susceptible of essential improvement: and under these impressions, they hope to receive the support and assistance of those who are interested in, and capable of promoting, the success of their endeavours.

VIII.

Account of a Machine for performing the Thread-Work in Shoe-making in a standing Posture; contrived, and for many Years constantly used by THOMAS HOLDEN, *Shoemaker, of Fettleworth, near Petworth, Sussex.**

A VERY moderate observation of the different processes of handicraftsmen will shew how extremely various are their habits of manipulating. Every different position of standing, sitting, and, perhaps, lying, may be found among them; and in works apparently of the same nature, the positions are found to be considerably unlike each other. It is probable that the first workman assumed positions which, whether aukward, confined, or inconsistent with health, or the contrary, came into universal use when the habit of the individual and the confirmed custom of master and apprentice had given them the sanction of many years. Thus we see that men's cloths are sewed by men who sit cross-legged; women's cloths by women who sit in no unusual position: turners in India hold the tool with their feet, and turn the lathe with the left hand, while they sit on the ground with the body leaning very much forward; in Europe they stand and turn with one foot, while the hands direct the tool: comb-cutters, for coarse or open-toothed combs, sit astride a large triangular stool with their work as low as the seat, so that they must keep their bodies almost

The attitudes and method of working in handicrafts are very various:

Instances:—taylors and mantua-makers; —turners in India and in in Europe; —comb-cutters for fine and for coarse work.

* For which the Society of Arts gave a premium of fifteen guineas. See vol. xxii. of their Transactions. One of the machines is in their repository.

horizon-

horizontal while at work; but those who cut ivory combs sit very nearly upright.

Other arts are also practised with injury to health.

Many other examples might be given, not only of works which are practically shewn to be capable of being executed with less injury to the comfort and health of the professors than at present; but of others where the mischiefs are no less evident, and their remedies by no means difficult, if man could be, by the gentle influence of reason, induced to depart from what habit has confirmed and made easy.

Shoemaking is an unhealthy craft.

The numerous bodily complaints which are consequent to the practice of the art of shoe-making, as now performed, are well known to all; and the remedies in this, as well as other branches of human industry, are intitled to general regard. He who improves the cultivation of the ground, or renders human labour more productive by machinery, is intitled to universal gratitude as a benefactor of the human race: He increases their comforts, and renders the means of subsistence more easy. The same argument will apply to every man who by his ingenuity or his influence shall diminish the evils which may be ultimately attendant on a life of labour. Thomas Holden has been impelled, for his own personal relief, to construct and use a simple machine for one branch of his craft. The master shoemakers and their masters, or employers, will act meritoriously in extending its use. As the editor of a Journal of *the Arts*, I have thought it my duty in this, as in most other instances, to second the views of that excellent Society, which has been so long established in London, and has so actively and constantly exerted itself for their encouragement.

It is deserving of general attention to improve the practice of the arts. Thomas Holden's machine.

Statement of its advantages.

Mr. Nicholas Turner, who addressed the Society on behalf of this machine, speaking highly in its commendation, says that its cost will not be more than from between twenty and thirty shillings. The inventor himself, after stating his sufferings from the pursuit of his business as a maker of shoes, says that he has found it to answer, and its use to have been followed by a restoration of his health. When he wrote his letter, he had made about two thousand pair of shoes with it, and he considers it as the quickest way of closing all the thread-work.

Certificates.

Certificates were also sent to the Society, from six cordwainers of the vicinity, who were witness to the use and advantages

tages of the machine; and Mr. Peter Martin, Surgeon, of Tulborough, wrote a letter to Charles Taylor, Esq. secretary to the Society, which I have extracted verbatim.

"I am sincerely of opinion, that Thomas Holden's invention is a desirable acquisition to men of that profession, especially to those who may be diseased internally, or who may suffer from stomach weakness and indigestion. These diseases may be aggravated, if not occasioned, by their working in a bent posture. Mr. Martin's opinion of the machine.

"The inventor, about twenty years ago, often applied to me for relief from a train of bowel complaints, and frequently had occasion to take the medicines usually employed for the relief of dyspepsia. Bad state of the health of the inventor from practising his employment.

"I repeatedly informed him, that his employment was the cause of his disorder, and desired him to relinquish it, or invent some method to do his work standing. This hint, and his corporeal sufferings, prompted to the invention. That it answers the purpose, I have reason to believe, as he and others use it. He is now free of complaints, and so improved in his corpulence and countenance, that he is not like the same man, and for years has had no occasion for medicine." Perfectly remedied by using the machine.

Reference to the Engraving of Mr. Holden's Invention for Shoemakers, Pl. IV. Fig. 2. Description of the machine.

A. The bed for the closing block, and to lay the shoe in whilst sewing.

B. The closing block.

C. A loose bed to lay the shoe in whilst stitching; the lower part of which is here exhibited reversed, to show how it is placed in the other bed, A.

D. The hollow or upper part of the loose bed C, in which the shoe is laid whilst stitching.

E. A table on which the tools wanted are to be laid.

F. An iron semicircle, fixed to each end of the bed A, to allow the bed to be raised or depressed. This half circle moves in the block G.

H. Another iron semicircle, with notches, which catch upon a tooth in the centre of the block, to hold the bed in any angle

angle required. This semicircle moves sideway on two hooks in staples, at each end of the bed.

I. The tail or stem of the bed A, moving in a cylindrical hole in the pillar, enabling the bed to be turned in any required direction, and which, with the movement F, enables the operator to place the shoe in any position necessary.

K. The pillar, formed like the pillar of a claw table, excepting the two side legs being in a direct line, and the other leg at a right-angle with them.

L. The semicircle H, shown separately, to explain how it is connected with the staples, and how the notches are formed.

M. The tail or stem of the bed A, and the lower part of the bed N, shown separately, to explain how the upper part of the bed is raised or depressed occasionally.

IX.

An Essay on the Cohesion of Fluids. By THOMAS YOUNG, *M. D. For. Sec. R. S.**

(Concluded from page 88.)

VII. *Cohesive Attraction of Solids and Fluids.*

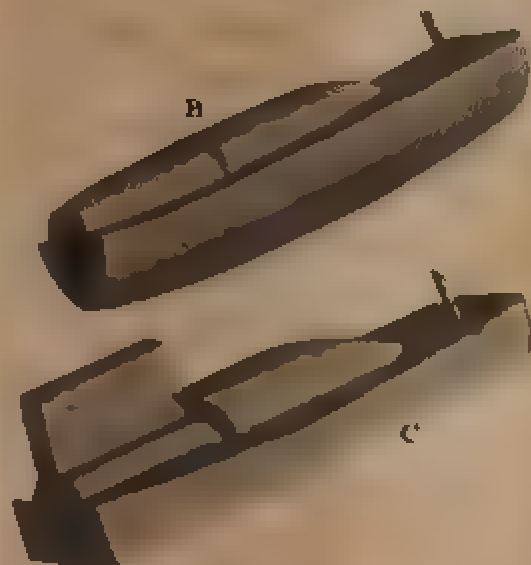
Cohesive attraction of solids and fluids.

WE may therefore inquire into the conditions of equilibrium of the three forces acting on the angular particles, one in the direction of the surface of the fluid only, a second in that of the common surface of the solid and fluid, and the third in that of the exposed surface of the solid. Now, supposing the angle of the fluid to be obtuse, the whole superficial cohesion of the fluid being represented by the radius, the part which acts in the direction of the surface of the solid will be proportional to the cosine of the inclination; and this force, added to the force of the solid, will be equal to the force of the common surface of the solid and fluid, or to the differences of their forces: consequently, the cosine added to twice the force of the solid, will be equal to the whole force of the fluid, or to the radius: hence the force of the solid is represented by half

* Philos. Trans. 1805.

the

Mr Holden's Machine for Shoe-makers

Galvanic Apparatus by Mr Sylvester

the difference between the cosine and the radius, or by half the versed sine; or, if the force of the fluid be represented by the diameter, the whole versed sine will indicate the force of the solid. And the same result follows when the angle of the fluid is acute. Hence we may infer, that if the solid have half the attractive force of the fluid, the surfaces will be perpendicular; and this seems in itself reasonable, since two rectangular edges of the solid are equally near to the angular particles with one of the fluid, and we may expect a fluid to rise and adhere to the surface of every solid more than half as attractive as itself; a conclusion which Clairaut has already inferred, in a different manner, from principles which he has but cursorily investigated, in his treatise on the figure of the earth.

Cohesive attraction of solids and fluids.

The versed sine varies as the square of the sine of half the angle: the force must therefore be as the square of the height to which the fluid may be elevated in contact with a horizontal surface, or nearly as the square of the number of grains expressing the apparent cohesion. Thus, according to the experiments of Morveau, on the suppositions already premised, we may infer that the mutual attraction of the particles of mercury being unity, that of mercury for gold will be .1 or more, that of silver about .94, of tin .90, of lead .81, of bismuth .72, of zinc .21, of copper 10, of antimony .08, of iron .07, and of cobalt .0004. The attraction of glass for mercury will be about one-sixth of the mutual attraction of the particles of mercury; but when the contact is perfect, it appears to be considerably greater.

Although the whole of this reasoning on the attraction of solids is to be considered rather as an approximation than as a strict demonstration, yet we are amply justified in concluding, that all the phenomena of capillary action may be accurately explained and mathematically demonstrated from the general law of the equable tension of the surface of a fluid, together with the consideration of the angle of contact appropriate to every combination of a fluid with a solid. Some anomalies, noticed by Muschenbroek and others, respecting in particular the effects of tubes of considerable lengths, have not been considered: but there is great reason to suppose that either the want of uniformity in the bore, or some similar inaccuracy, has been the cause of these irregularities, which have by no means

Cohesive attraction of solids and fluids.

means been sufficiently confirmed to afford an objection to any theory. The principle, which has been laid down respecting the contractile powers of the common surface of a solid and a fluid, is confirmed by an observation which I have made on the small drops of oil which form themselves on water. There is no doubt but that this cohesion is in some measure independent of the chemical affinities of the substances concerned; tallow when solid has a very evident attraction for the water out of which it is raised; and the same attraction must operate upon an unctuous fluid to cause it to spread on water, the fluidity of the water allowing this powerful agent to exert itself with an unresisted velocity. An oil which has thus been spread is afterwards collected, by some irregularity of attraction, into thin drops, which the slightest agitation again dissipates: their surface forms a very regular curve, which terminates abruptly in a surface perfectly horizontal: now it follows from the laws hydrostatics, that the lower surface of these drops must constitute a curve, of which the extreme inclination to the horizon is to the inclination of the upper surface as the specific gravity of the oil to the difference between its specific gravity and that of water: consequently since the contractile forces are held in equilibrium by a force which is perfectly horizontal, their magnitude must be in the ratio that has been already assigned: and it may be assumed as consonant both to theory and to observation, that the contractile force of the common surface of two substances, is proportional, other things being equal, to the difference of their densities. Hence, in order to explain the experiments of Boyle on the effects of a combination of fluids in capillary tubes, or any other experiments of a similar nature, we have only to apply the law of an equable tension, of which the magnitude is determined by the difference of the attractive powers of the fluids.

I shall reserve some further illustrations of this subject for a work which I have long been preparing for the press, and which I flatter myself will contain a clear and simple explanation of the most important parts of natural philosophy. I have only thought it right, in the present paper, to lay before the Royal Society, in the shortest possible compass, the particulars of an original investigation, tending to explain some facts and establish some analogies, which have hitherto been obscure and unintelligible.

Y.

X.

Facts and Observations relating to the Theory of Heat, Light, and Combustion. By Mr. J. ARNOLD.

To Mr. NICHOLSON.

SIR,

IT has been said, when a person begins to theorize, there is no absurdity which he may not give credit to, and that a professed theorist is much allied to a madman. This appears partly to be true, if we consider the numerous futile hypotheses, on various subjects, which have each had its reign; each has been supposed true, and each has deservedly fallen. But when we observe farther into the matter, there is ample room to suppose, that however wild the imaginations of certain persons may have been, yet from the exuberances of some, the most important facts have been discovered. The accurate attention of a person to the most trivial subject, has led to the most important discovery: need I mention the circumstance which induced the great Newton to invent his most just theory of gravitation: If common report be true, it was the train of thought induced by seeing an apple * fall from a tree. Lavoisier, by a proper train of thinking, discovered the theory which would account for the phenomena of combustion: † and Copernicus, by attentive observation, was led to believe, that the earth was a globe; ‡ and, like the planets, revolving round the sun. Even the publisher of a false theory has often gained himself a great degree of praise, as we see in the cases of Stahl, Scheele, Des Cartes, &c. From this preface you may be led to think that I also am about to theorize and you may be induced to cry out, "*delirat, delirat!*" be that as it may, I certainly *am* theorizing.

Dissertation on the nature and effects of heat, light and combustion.

Before I begin the more immediate subject of my hypothesis, I shall take a slight notice of the new-invented field in which you and so many distinguished persons have, and still continue

* Newtonii opera omnia. † Thompson.
‡ Biographia Generalis.

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to labour; what is the origin of the science? It has no pretensions to antiquity; it was not a part of the learning of the antients; the Greeks *did not* cultivate it; the Romans were unacquainted with it; but the Egyptians! the people of that country were great magicians, and could perform works which persons of other countries were unable to effect; they are supposed to have had converse with devils. What is the meaning of the new art? to what part of nature does its actions belong? It is called chemistry, an art which relates to the formation of medicines to prolong the life of man; but it has improved but little in this respect: formerly it was dignified with the name of alchemy,* a most august and reverend art, the votaries of which attended to the transmutation of metals, and attempted to discover the art of making gold, as well as of forming an universal medicine,† by which riches would abound, and mankind would dispense with that last distressing rite which hitherto has been performed by all, and to which all alive must bend.

Such a description, a few years since, might have been given of our art; but since that time, to what an extensive expanse has it reached! to it all nature is subject, whether animate, or unorganized; it is a science which treats of the minute ‡ particles of matter, and of the changes which take place upon applying different particles to each other.

The different substances in nature may be divided into several orders, solids, fluids, gases, and § unconfinable substances. The three first are always cognizable to two senses; the latter are apparent to one sense alone, viz. light to the sense of vision, and heat to the sense of touch

With regard to our speculation, the last set of substances are most to be attended to, though to explain their effects, the other substances must be had resort to. One of the most wonderful phenomena in nature, but at the same time the most familiar, *videlicet*, combustion, has been attempted to be explained at various times, by well-adapted theory. In this inquiry, and as inventors of different hypothesis have appeared at various times, Hooke, Mayow, Beecher, Stahl, Macquer,

* Albertus. † Paracelsus. ‡ Fourcroy. Heron.
§ Thompson.

and

and, at length, Lavoisier. In the remotest ages fire was supposed to be a substance which, by being applied to certain other substances, devoured them, and what was left was supposed to be unfit for the * food of fire: to this succeeded the opinion that a † solvent acting rapidly on the combustible, was the cause of the evolution of heat and light; to this the supposition that violent friction and ‡ agitation between the combustible and a matter existing in the air was the cause of the phenomenon. This was followed by the hypothesis that inflammables had the peculiarity of § running into violent whirlings, by which combustion was produced; the next opinion was that light existed in a || dense state in all inflammables, and by certain actions was set free; afterwards a peculiar very subtile,** most elastic fluid, was supposed to be condensible, in certain bodies, from which it escaped, and produced certain appearances, among which was combustion. This hypothesis was superseded by the opinion that heat and light were evolved by the †† air, which combined with the inflammable body, at the same time giving out a certain principle which rendered the air afterwards incapable of supporting combustion; after this combustion was supposed to depend on a substance which was the same in all combustibles ‡‡. This theory was the last to overturn, and was succeeded by that which stated that during combustion oxigen gas was §§ always absorbed. This theory is certainly the best, and appears to have a firm foundation, but it evidently fails in explaining the most striking phenomena of combustion, viz. the |||| origin of the heat and light, which is the very essence of combustion. It is the province, therefore, of this paper to explain the probable sources of these substances; though, from the extreme intricacy of the subject, it is with great diffidence that I do it.

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It may be proper, before immediate procedure to the business, to notice those substances which are necessary to constitute combustion. Authors inform us, that oxigen gas and an

* Albertus Magnus et alii. † Hooke. ‡ Mayow.
§ Stahl Scheele et alii. || Macquer. ** Newton.
†† Crawford. ‡‡ Kirwan. §§ Lavoisier.
|||| Murray et alii.

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* inflammable substance are necessarily present in every case of combustion, but in the present speculation I am under the necessity of supposing both oxigen gas and combustibles compound substances, so that we have four substances present in every case of combustion,—oxigen, light, the base of an inflammable substance, and heat.

In speaking of caloric and light, we do not think it necessary to enter into the subject minutely, but only to mention those circumstances respecting them which are allied to our theory. With regard to the mode of existence of heat, there is still some dispute among philosophers, some supposing that it is a property produced by the motion of a very subtile æther † which pervades all space; others, that it is a real substance, sometimes giving a sensation to the sense and touch, but often so hidden ‡ in bodies, that its presence cannot be perceived. Perhaps the latter opinion is the best, for by it we are the better enabled to treat of the subject. As I mentioned before, this substance is capable of entering into chemical union with certain other substances; in which case it can only be discovered by certain properties which those substances possess, and this property is that of inflammability; here therefore I differ in opinion from most philosophers, who suppose that heat exists latent in greatest quantity in certain aerial susbstances, as oxigen, carbonic acid, &c. § which I profess to have none, unless that which regards the temperature of such bodies, for I am not inclined to deny the circumstance that different substances require different portions of heat to make them of an equal temperature; on the contrary, I suppose that the experiments of Crawford, Irvine, &c. with respect to the capacities of bodies for heat, are as accurate as the subject will allow; but the heat, which I call latent, has no effect in increasing temperature; a quantity of sulphur at *plus* one hundred degrees, contains as much, and no more, of that latent heat, than the same portion would at the most intense cold ever observed.

Perhaps it will be better to state in what the peculiarity of my opinion consists. According to the writings of the

* Thompson et multi alii. † Newton. Leslie. ‡ Black.
§ Crawford.

most

most celebrated chemists, caloric is of two kinds; one of these is called caloric of temperature, which gives the sensation of warmth; the other is hidden in substances,* so that its presence cannot be perceived but by certain changes which such substances may be made to undergo; thus we find that ice at 32° contains much less heat than water at 32°,† though to the sense of touch they are the same. The third state in which I suppose heat is capable of existing, is as a component part of all inflammable substances, and is that substance on which their combustibility depends; and this I think it necessary to take for granted, to elucidate the phenomena of combustion, which I hope to do in this imperfect memoir; but as it is a thing necessary, for the truth of every theory, that the data on which it is founded should have truth for their basis, it may be prudent to state the grounds on which the present assumption is settled, especially as it is a common fault with speculators to build castles without examining the foundation which is to support them, and hence the superstructure being well increased, or perhaps nearly finished, by a fault in the foundation, has soon fallen prone; as has been observed in those buildings which were raised by Stahl, Scheele, Des Cartes, &c. though, at first sight, they appeared very fair, and exceedingly strong.

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Since the reception of the Lavoiserian theory of combustion, some philosophers have supposed that the evolution of caloric and light was from the oxigen gas alone,‡ others have supposed that light was afforded by the § combustible substance, and heat by the oxigen gas,|| which last opinion has had the greater number of followers, Some unable to account for the circumstance have supposed that light and heat are only different forms of the ** same substance. Lavoisier appears to have inclined to the opinion that the light was afforded by the †† oxigen gas, though he also supposed that the heat also might have the same origin. Before I

* Dr. Black et alii. † Dr. Black. ‡ Lavoisier, Brugnatelli, &c.
§ Maquer, Richter, De la Metherie, Chenevix, Thompson, Gren, et alii. || Thompson, et cæteri. ** Murray's Chemistry, p. 178.
†† Fourcroy.

state

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state my objections to these opinions, I shall proceed to notice light, oxigen, and inflammable bodies.

Light, like heat, according to some, is a property caused by the vibration of a very subtile fluid which fills all space, and which undulation * is produced by the sun, and other luminous bodies. According to others, it is a real substance, emanating from luminous bodies in strait lines, † and which by approaching our eyes gives the sensation of light. In obedience to this, therefore, I shall suppose it a substance, and like heat, capable of existing in a latent sensible form. The first is that which affects our organs of vision, the second as a component part of a substance, with which it is in chemical union, but from which, by the action of certain substances on it, may be expelled and rendered sensible. Whether light be able to combine with more substances than one, I cannot at present determine, but from many appearances, which are familiar to chemists, I am apt to believe that it is.

Inflammable substances have been usually considered as elements, but I am under the necessity of calling them all compounds, ‡ each consisting of a base and the condensed matter of heat. This opinion indeed, does not appear to be peculiar to myself; for an excellent chemist, who has published an extensive chemical work, appears to think them binary compounds, as consisting of a base and light. Of these substances there are various kinds, each elementary combustible, consisting of its peculiar base; but they all resemble each other in this, that each one contains in composition the condensed matter of heat, but that in different degrees of cohesion, some very easily part with it, as sulphur, phosphorus, &c. but from others it can scarcely be expelled, as the metals. Some philosophers have supposed that these substances consist of a base, and the matter of *light*.

Oxigen gas has been by many supposed to be a compound of heat; and a base, § some have reckoned it a ternary compound, consisting of a base, heat, and light; || but I, as was before observed, take it to be a combination of the condensed

* Huygens. Euler. † Newton. ‡ Thompson.
§ Murray et alii. || Brugnatelli, Lavoisier, &c.

matter

matter of light, and a base: when I speak therefore of oxigen, I mean the base of oxigen gas, which perhaps can only exist in combination with the base of an inflammable substance, thereby forming a product of combustion.

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Now therefore let us examine some cases of combustion, to observe whether our theory will apply, and it will be preferable to begin with the most simple. Water is known, (a product of combustion) to consist of oxigen and hydrogen, or rather of the bases of oxigen and hydrogen gases, and consequently the heat and light were evolved in the conversion of these gases into water. It therefore must be apparent to all, that as oxigen and hydrogen gases consist of four substances, and water of but two, that to reproduce the gases from water, the light and caloric must be added; but it may be said that heat and light will not unite with water, and convert it into its component parts; if we apply water to the strongest light and heat, it will only be converted into vapour. To this I answer, that the heat and light in the gases exist in the latent form, and we can only cause them to combine with water by presenting them to it in such states. But how can we procure such light and heat, they are incognizable to the senses, and therefore must be imperceptible? *I hold electricity to be light and heat combined, and capable of effecting this change.* When I speak of electric matter, I mean, that though light and heat are generally apparent on electric matter passing from one body to another, yet that this light and heat are not necessarily in a sensible state, for electricity has the power of entering some substances without producing a shock, or evolving light and heat: to prove this, we will make some observations upon the effects of electricity on combustible substances, and products of combustion. Combustible substances, according to our theory, are binary compounds, consisting of a base and heat; to expel this heat it is necessary to present to them a substance known by the title, oxigen gas, which is a combination of a base and light. A mixture of these two substances may therefore be supposed to be saturated with the matters of light and heat, but by an increase of temperature a divellent attraction takes place, the base of the oxigen having a greater affinity for the base of the inflammable

than

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than each has for its heat and light, which therefore, from their unconfinable nature, escape. The action of electric fluid, on a combustible substance, is only to increase its temperature, and to produce combustion; for the substance is already saturated with that matter which electricity offers to it. But let us present to the electric matter a substance of a different nature, a binary compound, consisting of the bases of oxigen gas and an inflammable substance, or what is called a product of combustion; here the action is widely different, no heat is evolved, no light appears; but the substances received the electric matter in a latent state, and is resolved into two binary compounds, an inflammable substance, and oxigen gas. This process may be exemplified, in a common case of combustion, in which both the necessary compounds are in the gaseous form. If a certain quantity of oxigen gas be mixed with hydrogen gas, we shall have the four substances necessary for combustion, oxigen, light, the base of an inflammable, and heat. The electric spark being applied to this mixture, causes the bases of the oxigen and hydrogen gases to unite, and the light and heat to escape; here therefore we have the product of combustion, which is a substance void of light and heat, and it will be necessary to offer light and heat to it, to reproduce the oxigen and hydrogen gases; but we may offer sensible light and heat to them in profusion and no union will be produced; if we heat it ever so greatly it will only be evaporated. Heat and light exist in the gases we have mentioned in an imperceptible form, and to induce an union, we must offer them in the same state. This form of light and heat is nothing else than the electric fluid, and hence, by passing electricity through the water, it is resolved into its primitive gases.

Electric matter, as it is produced from the common machine, is of a compound nature, a combination of the matters of heat and light; being therefore introduced into water, myriads of minute bubbles escape, which are oxigen and hydrogen gases *mixed together*. But of late years a different modification of the electric matter has been observed, in which the principle may be divided, with matter of heat escaping at one point, and the matter of light at another. So that by introducing the points of the galvanic machine under

under water, we may obtain the oxigen gas from one point, and the hydrogen gas from the other; and this appears to me to be caused by the base of the oxigen gas existing in the water combining with the matter of light from one end of the trough, and the matter of heat uniting with the base of the hydrogen gas at the other.

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With galvanism we appear to be but very little acquainted; we are shown its powerful effects in burning substances, which were before supposed uninflammable; we have exhibited to us the powerful effects it has upon the vital animal fibre, but no one has attempted to explain the causes of these effects; the cultivators of it appear to have explored in the dark, making numerous experiments, and wondering at their results at the beginning of an experiment, unknowing what to expect; and, having finished it, unable to account for the change: like the practice of the empirics of old, who were employed in obtaining experience by actual observation only, unassisted by reason or theory. If a disease disappeared under the use of a particular remedy, that substance was a cure for the complaint; if a person was affected with purging after swallowing a certain article, that article was set down as of a purgative nature, when perhaps neither of these effects were really caused by the substance employed, the person recovered by the *vires medicatrices naturæ*, and the purging was caused by a substance prepared by the body itself; and it remained for future experi nce to prove the fallacy of the unjust account. Just so it is with galvanism, by passing the influence through water, oxigen and hydrogen gases appear; it is therefore set down as a fact, proved by experience, that galvanism decomposes water; and, if galvanism has the effect of decomposing one substance, and has no effect on another, these circumstances are related in the empirical account, and no one endeavours to seek further into the subject, no one endeavours to explain the causes of these occurrences.

If I may hazard an opinion, galvanism differs from common electricity in this: in the latter, the influence escapes from one point only, and is thence a compound matter; but in the former it escapes by different points. I have said before, that electric matter is a combination of light and heat

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in a pure and detatched state, and capable of entering certain subs ances, without affecting either the organs of vision or of touch, as is observed when it enters a product of combustion.

The effect of galvanism varies with different inflammable substances according to the intensity of heat which is necessary for their combustion; phosphorus, hydrogen, carbon, and sulphur, inflame at low temperatures, and therefore are immediately set on fire; but the metals which I consider as inflammable substances, are more difficultly ignited, and are capable of retaining a considerable quantity of the electric fluid without undergoing change; but even these are inflamed by a powerful instrument.

In treating of combinations of oxigen with other substances much ambiguity has arisen among chemists; for no one can suppose that the oxigen which is contained in the products of combustion is the same as oxigen gas, which is a supporter of combustion. An ingenious philosopher of Italy * observing this circumstance, has endeavoured to remedy it by supposing oxigen able to combine with substances retaining its heat, as in nitric and oxi-muriatic acids; and in its simple state as in the sulphuric and carbonic acids, but as he supposed that heat and a base were the component parts of oxigen gas, he called it thermoxigen, as retaining its heat; but, as I have differed from him in supposing it a compound of light and a base, I may call it photoxigen.

It is a question which, I must confess, I am unable to resolve, whether the matter of light can exist in substances without the presence of oxigen? But thus far may be said, that oxigen and light are capable of combining with other substances, and thereby may be in a dense form, as is seen in the acid supporters, and certain metallic supporters. Nitric acid therefore we may state as a combination of oxigen, light, and another substance called azote, which has hitherto been called an element; but whether it is the base of any substance, or can be united with the matters of heat and light, I am unable to determine. It has long been known that the component parts of nitric acid were the same as that of atmospheric air, but that the proportions of different sub-

* Brugnatelli.

stances

stances varied. I am induced to think that the change consists principally in this, that nitric acid contains a larger portion of condensed light than atmospheric air, which may be supported from an old and * celebrated experiment, which has been often repeated, and with the same result. A quantity of atmospheric air was inclosed in a proper vessel, and frequent electric sparks were passed through it, till at length its bulk diminished, and it had lost its properties, having manifestly become nitric acid. But the most remarkable part of the experiment is the appearance of the electric matter which acts upon it: as I stated before, electricity is a compound of heat and light; but there are certain substances which can only unite with one of these, which is the case with the experiment now spoken of; the light is absorbed by the mixture, and the heat evolved, for no sparks are perceived, but a considerable quantity of heat is evolved in a sensible form. Many may suppose that this heat escapes from the condensation which occurs, and that the capacity of the substances are changed. One of these opinions appears as likely as the other; and I think mine is the more preferable, for the condensation of gases does not necessarily evolve heat, as we observe on presenting gaseous muriatic acid and ammonia to each other.

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As azote is called a simple elementary substance which is uninflammable, so we have another which has the same title and property of incombustibility, *videlicet*, the muriatic acid; and this, like atmospheric air, appears to be capable of uniting with the matter of light, and thereby becoming a supporter of combustion. The real combination of this acid has not been stated, though I have no doubt that the experiments which have been made, in Italy† have great weight in leading us to believe that it is a combination of oxigen and hydrogen, and may be supposed a product of combustion. Besides the supporters of combustion we have already mentioned, there appear to be others which deserve that name, which are certain metallic oxides, especially the black oxide of manganese, and the red oxide of lead.

* Cavendish. † Pacchiani.

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Before going farther, it may be necessary to give some examples of the changes which galvanic influence produces on substances, the nature of which, with respect to combustion, are very different. These substances may be arranged under four heads, supporters of combustion, products, inflammable substances, and detonating or deflagrating substances. One example, with regard to a supporter of combustion, I have given in treating of the conversion of atmospheric air into nitric acid. It now remains therefore to make some remarks on muriatic acid. I do not know whether the same experiment has been made on this substance as on atmospheric air, but I am induced to think that a similar change would be observed, *scilicet*, that on passing the electric fluid through it, light would be absorbed, and heat evolved, the acid thereby becoming a supporter, or what is commonly called oxigenated muriatic acid. I leave this to future experimenters to determine. It appears to me, that oximuriatic acid is always obtained by a process analogous to that of passing electricity through muriatic acid; a quantity of a substance containing light in a dense state is presented to it, and unites with it in precisely the same manner as electrical light would. Black oxide of manganese, a supporter of combustion, a combination of light, oxigen, and the base of an inflammable substance or a metal, is presented to the muriatic acid, consisting of the bases of oxigen and hydrogen gases, and which, at the same time, is capable of absorbing and retaining the matter of light, which it, in effect, receives from the black oxide, and thereby becomes a supporter. In this instance we manifestly have a translation of that substance, on which combustibility depends, passing from the oxide to the muriatic acid, so that what was before an incombustible becomes a supporter, and that which was formerly a supporter becomes an incombustible. What name can we give to this chemical action? is it semi-combustion? the heat only escapes! In all real cases of combustion light accompanies it, in this it is retained.

The most common supporter of combustion is oxigen gas, a combination of oxigen and light: other supporters are ternary compounds; oxigen, azote, and light, in the nitric acid, and oxigen, the base of an inflammable, and light, in the oximuriatic acid; the metallic supporters are combinations of light, oxigen,

oxigen, and a metal. Products of combustion are all combinations of the bases of oxigen gas, and an inflammable substance. Hence, as they are bereft of that light and heat which they possessed before combustion, it is necessary to afford light and heat to them to obtain a decomposition, and this decomposition is effectually obtained by offering electric matter to them, as *it* is that form of light and heat with which they can combine. One example of this decomposition has been mentioned in a former part of this paper, in which it was shown that water is convertible into its primitive gases; we shall take another instance of a different substance, namely, sulphuric acid, which is a product of combustion, and possesses most active properties. On introducing the wires of the galvanic pile into a vessel containing this acid, it is soon decomposed, as is the case with water, oxigen gas appearing at one extremity and inflammable sulphur at the other, and at the same time we observe little or no changes of temperature, as is the case with water, for it absorbs both principles of the electricity.

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The third set of substances on which the influence of galvanism has been tried, are inflammables; these are all compounds of heat and a base, but they differ in this, that their principles vary very much in the degree of cohesion which exists between them, some are separated by a *little* increase of temperature, others scarcely at all by the highest temperature which we can apply; but to produce this decomposition it is always necessary that a substance should be in contact with them, which contains the matter of light as a component part; and this, in most cases, is oxigen gas. As an instance of the influence of galvanism on this set of substances, we may mention charcoal, which is soon ignited, by which it parts with the heat which it contained, and absorbs a principle from the air called oxigen, which at the same time gives out light, which usually exists in composition with it, in its aerial form. To mention another instance, we may take a subject more difficultly inflamed, viz. iron; this, on being presented to the influence, if it be in any considerable quantity, is only rendered red hot, but by being in the form of small wire it burns with very brilliant sparks and great heat; the product which is obtained from it is an oxide of iron.

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Dissertation on the nature and effects of heat, light, and combustion.

The next set of substances which we may notice, is what have been called deflagrating or detonating substances, these are necessarily quaternary, many of them quinary compounds, and contain the four substances necessary for combustion, and are hence inflammable in close vessels, without being in contact with oxigen gas, they contain a supporter and an inflammable, or, as we may otherwise speak, oxigen, light, the base of an inflammable, and heat. A mixture of oxigen and hydrogen is an example of this kind, and so is gunpowder, the ammoniureta argenti, and auri, and other fulminating compositions. These immediately explode on the approach of the electric fluid, and two of their component parts escape, viz. the light and heat. One farther instance I shall mention of the action of a supporter of combustion on an inflammable substance. Oximuriatic acid being mixed with ammonia, both, in their gaseous forms, have a remarkable action on each other. The first is a compound of hydrogen, oxigen, and light; the latter of hydrogen, azote, and heat. In this case an attraction exists between the bases of the combustibles which form the muriatic acid and the ammonia, the heat and light escaping, as well as the azote, which is set free. From this experiment it may not appear strange that muriatic acid has been detected in passing galvanism through water.

We have now given our opinion with respect to the nature of the electric and galvanic fluids, and endeavoured to prove the truth of it from certain effects, which it produces on different substances; from hence it may appear that it is entirely dependant on chemical changes, and upon the action which different bodies have on each other. It appears to me, to be nothing more than an action of a supporter of combustion on a combustible body; for we find that in every galvanic apparatus there is an oxidable metal, a substance capable of oxidating it, and what may be called a conductor, which has the power of conveying off the electric matter produced by this chemical action, and it remains for future experiments to prove that every such chemical action really does produce electric matter, which if performed in proper apparatus might be as evident as that from the trough or pile.

From

From these transient remarks it may appear evident to any one employed in galvanic research, what is the true action of this wonderful chemical agent: its nature also I hope has been somewhat elucidated. And as a path has been shown to lead on persons labouring in the field, they will be more sure of employing themselves to advantage, whence they may have a probable supposition of what will be the result of their experiments. If the influence be applied to an inflammable substance, inflammation will ensue, if to a product of combustion, it will be resolved into oxigen gas and an inflammable matter. If to a substance capable of becoming a supporter, *it* will absorb light and give out heat.

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It is a question, and I cannot say whether it has been solved, whether the condensed matter of light can exist except with a necessary quantity of oxigen to retain it; or whether it can be present in any substance which does not contain oxigen? I believe that nothing has been found as supporter of combustion unless it contains oxigen gas as the nitric and oxi-muriatic acids. Would it be possible to form an oxi-sulphuric acid, or an oxi-nitric acid, by help of the galvanic apparatus? Those salts, which contain the nitric or oxi-muriatic acids, have been called detonating salts, perhaps improperly, for but few of them detonate without the addition of an inflammable. The nitrous ammonia is a real detonating salt.

Among the discoveries by the galvanic instruments, I believe no one has decomposed the carbonic acid or the phosphoric acid: by proper management it may be done, and with regard to the first, some very curious appearances may be observed. Carbonic acid and water are products of combustion, but if they are combined with that substance on which the inflammability of bodies depend, they become ardent spirit or oil. There appears a gradation of changes between gluten farina, saccharum, alkohol, and oleum. Any of these give a product of water and carbonic acid.

It may now be proper to notice the objections which have been made to the opinion, that light is afforded by oxigen, and heat by the inflammable body during the process of combustion. The much lamented French chemist who was the inven-

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Dissertation on the nature and effects of heat, light, and combustion.

tor of the present theory, was much inclined to the opinion that light was afforded by * oxigen gas; but he appears to have supposed that the heat had also the same origin. A very celebrated chemist of Italy † also has published the same opinion. Succeeding chemists, however, have been led to suppose, that the light was afforded by one of the substances necessary for combustion, and heat by the other, and for certain reasons they have attributed the heat to the oxigen, and the light to the inflammable. These reasons therefore, it behoves us to scrutinize and try the weight they have in this affair.

Primo.—Bodies in the aeriform state contain in equal weights at the same temperure much more caloric than fluids or solids do. This assertion is true in one sense; a pound of steam at 212° contains much more latent caloric than a pound of water at 212°, in the same manner as a pound of water at 32° does than as much ice at the same temperature. But this rule does not hold with substances of different *natures*, otherwise a gaseous substance could not be absorbed by a fluid or solid without a great increase of temperature. But if we add a very small quantity of water to a large portion of ammonical gas, the ammonia is immediately absorbed, but the sensible heat is but little increased; or if we mix muriatic acid gas with ammoniacal gas, a solid substance is immediately formed, with but little increased temperature. The same also is observable on the mixing of carbonic acid gas and ammoniacal gas, the heat in these cases can entirely be acconnted for from the different capacities which these bodies have for caloric.

Hence I think we may lay aside the opinion, that the condensation of an aereal substance is the cause of the evolution of heat. For if this were the case, a substance receiving gaseous form would always produce a decrease of temperature; but we do not find that the case, when carbonic acid gas is evolved from lime, or when a large quantity of gas escapes in the deflagration of gun-powder or other substances of the same nature.

* Lavoisier, Fourcroy. † Professor Brugnatelli.

Secundo.

Secundo.—If the light were afforded by the oxigen gas we should have most when the greatest quantity of oxigen disappears; but the light is greater in the combustion of phosphorus, than in that of charcoal, and still greater than in the combustion of hydrogen gas; but more oxigen gas disappears in the combustion of the hydrogen gas than in the other two. We have no accurate photometer by which we can measure the intensity of light, and even granting that 1000 times as much light appeared in the combustion of phosphorus as in that of hydrogen gas, no one can affirm that the light on inflammation does not bear a ratio with the intensity of heat, and we find that the heat evolved in the inflammation of hydrogen gas is but small when compared to that of phosphorus or charcoal. Light does not appear from the extremities of the galvanic pile, unless they be connected, and the heat and light conjoined. Hence we find the greater is the intensity of the heat in combustion, so also the light is increased in the same proportion.

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Tertio.—By the combustion of hydrogen gas and oxigen gas from the new invented blow-pipe, we obtain a more intense heat than could have been done before, except by mirrors or lenses, but we observe but little light in this case. The heat produced from this blow-pipe is by no means so intense as many have supposed. Its effect are tried upon inflammable substances, iron, copper, and which are themselves combinations of considered heat; in these cases therefore we have the heat of the hydrogen gas, and that of the inflammable substance, acting together, and must consequently have very great heat. I believe, that by passing a stream of oxigen gas through the flame of spirit of wine or oil, the heat might be as great or greater. I have observed the action of the common blow-pipe on a stick of glass, and it appeared nearly as great as that of the hydrogen and oxigen gas blow-pipe: quite so great for the reasons before mentioned, it could not be.

These, I believe, are the three principal objections which have been stated against the doctrine which it is here attempted to support. It would take up too long a time to mention all which are to be met with in authors.

 With

Dissertation on the nature and effects of heat, light and combustion.

With regard to the objections which may start against my opinion of the electric fluid, two at least have occurred: the first is, that if the galvanic influence were nothing more than the combined matters of heat and light, every case of combustion would be an electrical experiment. To this I may answer, that from substances in combustion the light and heat go off in the sensible form; but it is very different in the galvanic light and heat, the essence of *it* consists in this, that it is not in its sensible form; as soon as it affects the eye, or the thermometer, it is no more electric matter, but common light and heat; by how much light and heat is given out from an electrified substance, by so much is that electric matter diminished in quantity. Galvanism is produced by an action very similar to combustion; a substance is oxidated as is the case with combustion, but the heat being insufficient, and the substance present varying somewhat from those producing real combustion, heat and light are given out in that peculiarly subtile form which characterizes the electric fluid. A supporter of combustion in both cases acts on a combustible substance, the former of which in both cases gives out light, and the latter heat, and these moving in different directions through the apparatus, at the place of contact, appear in the form of sparks.—Hence it may appear that galvanic troughs may be much improved, for the action appears to be proportionately great, according to the surface of the oxidable metal which is presented to the oxidizer.

J. ARNOLD.

May 4th, 1806.

XI.

On the Sugar of Grapes. *By* Professor Proust.*

Sugar of grapes.

My assistant, after several days occupation in drying grape sugar which had been drained of its melasses, has succeeded

* Journal de Physique, lxi. p. 399.

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in rendering it as perfectly white as that of my former experiments. I have remarked, that the sugar which forms granular crystals is easy to be divided, and yields readily to the operation of claying: but it is not so with what remains thin, fat, like honey, and consequently imperfectly freed from its syrup; the humidity of the argil penetrates it too slowly, and it dissolves and carries away too much sugar. All the efforts of the sugar-baker should therefore, in my opinion, be directed towards obtaining the crystallization in the most granulated state possible; and I am encouraged to hope that the difficulties which may appear in the way of our object will be surmounted, as grape sugar crystallizes considerably quicker than that made from cane: The candy which we see so frequently on sweetmeats of every kind affords a daily proof of this. All that I have examined for several years past in the confectionary of my own house, was a sugar perfectly analogous to that from grapes, without admixture of that from cane. The latter is therefore much less crystallizable. Sugar of grapes.

Grape sugar is not so white, I must repeat it, as that from cane; but its flavour is full, pure, and without the least remains of a vegetable taste or smell.

XII.

SCIENTIFIC NEWS.

Magnetical Telescope.

MR. Edward Troughton has constructed a new telescope for determining the magnetical meridian. It consits of a tube of steel, containing a set of lenses with cross wires or spiders weebs, in the usual manner. It will easily be understood that an instrument of this kind, after receiving the magnetic power, may traverse upon pivots or by any other similar mode of suspension, and will dispose itself in the magnetic meridian. One of the difficulties attending the magnetic bar of the usual form is, that its line of direction may not be parallel to its side; and it is not easy to determine the quantity of error, by reversing Mr. Troughton's invention of a magnet in the form of a tube fitted up as a telescope.

Magnetical telescope.

versing, because this last operation is, in most cases, impracticable. Mr. Troughton's magnetic telescope may be turned round in its support like that of the levelling instrument; and it will determine the magnetic meridian, whenever any one and the same distant object is seen, upon the centre of the cross wires after the telescope has been turned half round on its axis, as in its former state. By this contrivance the diurnal and the other variations, to which the magnetic bar is subject, may be easily observed, and it may even be ascertained whether the direction of the magnetic force varies with regard to the axis of the tube. As the present notice is intended to be short, and as I hope for farther communication from the inventor, I forbear to enter upon observations respecting the kind of instrument to which this telescope may be attached. It is evident that observations for the dip and variation may easily be made by reference to the plumb line, and to the heavenly bodies.

Mr. Troughton is already engaged in the executing orders received for this instrument, as well from scientific men of this kingdom, as from those on the Continent. The learned reader will recollect several instances, in which the eye-peice of a telescope, applied to its focal image, has, with great convenience and precision, afforded angular determinations which could not with the same convenience have been observed in the usual methods. The application of a magnifier to the extremity of a magnetic needle is also attended with difficulties, which Mr. Troughton's invention will obviate; at the same time that it facilitates and extends our views of a very curious and useful natural power. Among those active philosophers who do not wait for the construction of instruments, or cannot afford to make purchases for all their several occasions, this contrivance will suggest experiments, which may be made by tyeing together a telescope and a magnetic bar, and suspending them by a thread or fine wire, for some of the purposes of observation which they may be disposed to make. This, though a clumsy instrument, will also bear reversing, and may be usefully applied.

New

New Metal Columbium

The learned Millin, member of the French Institute, in his Magasin Encyclopediæ, for December last, page 388, relates the following particulars concerning the mineral to which Mr. Hatchett has given so much celebrity; (see our Journal XX. 236). They were communicated to him by Mr. Valentin, physician at Marseilles, who is well known as a cultivator of natural philosophy and history. Ore of columbium.

The mineral examined by Mr. Hatchett was found in a spring in the American province of Massachusetts. The spring is in the town of New London, in the state of Connecticut. It is near the house where Governor Winthorp lived, at the distance of about three miles from the sea up the harbour. The place was formerly called Nantneague. Mr. Francis B. Winthorp, of New York, has obligingly forwarded to the Historical Society of Massachusetts, the manuscript paper of his ancestor relative to the place, and the minerals which he presented to Dr. Hans Sloane, at London. It is to be hoped, says our author, that other specimens of this mineral will be obtained.

Sudden Eruption of Water near Como.

A remarkable phenomenon has excited the curiosity of the inhabitants of the vicinity of Como. In the commune of Laorca, in the territory of Alleco, a subterraneous spring all at once burst forth, which immediately overthrew two houses, and in the course of fifteen or twenty hours a forge which stood in its way. This spring is loaded with a thick chalky matter, which mixing with the water has rendered the lake into which it falls quite turbid. M. La Carte, officer of engineers, who visited the spot, attributes the accidents which have happened to a subterraneous excavation made by the water, and he judges that the extent of further damage will depend on the actual magnitude of the cavity. A new spring in Italy.

Prussian

Prussian Academy.

Sittings of the Prussian Academy. Prize questions.

The Royal Academy of Sciences at Berlin, in their public sitting of last August, gave an account of the competition for the prizes for 1804. The question relative to the law of Mariotte not having been satisfactorily resolved, was deferred to this present May. The same measure was adopted with regard to their question concerning the structure of the lungs. The third question on the inflammation of the spleen was resolved in three memoirs: The prize of fifty ducats was adjudged to Mr. Klausch, physician at Militsch in Silesia.

Seven memoirs were received on the philosophical question concerning analysis and the analytical method. That of Mr. Francke, rector of Husum, obtained the prize. The question by an anonymous proposer of last year, "*Why civilization proceeded from the East,*" produced several memoirs; but the decision of the academy is deferred till they shall have been examined.

The class of philosophy has proposed for the year 1807, the following subject for a prize:

"Does there exist an immediate internal perception, and in what respect does it differ from intuition and the simple abstraction by the rules of thinking and perceiving.

"In what respect do intuitions differ from sensations and the intimate sense.

"In what relation do these actions and situations of the intellect stand with regard to conceptions and ideas."

Poland

Prize questions.

In the month of June last, the class of Physical Sciences of the University and Imperial Academy of Vilna, proposed the following prizes:

"Besides the diabetes mellitus of medical writers, are there other maladies peculiar to man, which from decided experiments are known to produce in different organs a secretion similar to that of saccharine matter, and in abundance sufficient to occasion consumption by its loss? and what are these disorders?"

The prize is 100 gold ducats of Holland; and the concurrence is open till the first of September 1806.

Second

Second prize. "What are the true characters and principal causes of that disorder which, though not confined to Poland, is nevertheless called *Plica Polonica?* Are there any methods of curing it with more success than by those hitherto known and employed? and what are those methods?"

Prize questions.

The prize is 100 gold ducats of Holland; and the concurrence is open till the first of September 1807.

The third prize. "What are the principal maladies of vegetables, and what is the true analogy between those and the disorders of animals?"

The prize is 100 gold ducats of Holland; and the concurrence is open till the first of September 1808.

The class of Mathematical Societies of the same academy proposed the following prize:

"Suppose a canal from which flows per minute or second a quantity of water *m*. through a transverse section of given width and depth, terminated by two sides. This being admitted, if from one bank or side to the other there be constructed in the section a dyke or obstacle in which an aperture of given dimensions be made for the efflux of the water; it is demanded according to what law, the water elevated by means of the obstacle, will be forced to enlarge itself, not only near the dyke, but also in proceeding up the canal. It is desired that formulæ may be afforded sufficiently general to be applied to the efflux, not only of the same quantity m, but also any other quantity $m+x$. The theory and experiment, not being exactly corresponding with each other, it will be required that the necessary corrections should be made to the formulæ, and that it be proved by facts and observations how nearly they approach the truth."

The prize is 100 gold ducats of Holland; and the concurrence is open till the first of September 1806.

The class of Moral and Political Sciences have proposed as their first prize:

"As we see the mathematical and physical sciences make daily advances, and become enriched with new discoveries, it is demanded: 1st.—Why the same does not happen with regard to the moral sciences? 2d.—Among the different branches of these sciences, are there not some which are capable of greater perfection? and what are they? 3d.—To what point are

Prize questions.

are they by their own nature capable of being advanced, and what are the limits which appear to bound their possible improvement? 4th.—What are the properest means for giving this possible degree of perfection to those parts of moral science? 5th.—It is more particularly desired that the discussion of this matter should be so conducted as to present results tending to advance the theory of the legislation most conformable to the nature of man."

The prize is 100 gold ducats of Holland; and the concurrence is open till the first of September 1806.

The second prize. "To determine by analytical investigation of political economy what are the points in which the fundamental positions of Adam Smith and Dr. Quesnay agree, and those in which they differ or are even opposite to each other. This examination must necessarily present results useful to the progress of the science of political economy."

The prize is 100 gold ducats of Holland; and the concurrence is open till the first of September 1806.

The dissertations are required to be written in Latin, French, or Polish, and the packet addressed to the rector of the Universaty of Vilna, to the care of Messrs. Layser or Karner, who are bankers in the same town, and to whom the rector will give a receipt. The University does not engage to return either the memoirs or drawings which shall be towarded in this competition; but the authors may take copies of the same at any time, These works will not be printed by the University without the the formal consent of the authors; but the authors themselves are at liberty to publish them in whatever manner they please.

The prizes will be awarded before the new year, that is to say, before the first of January 1807, for those solutions made in the first year, and before the 1st of January 1808, for those made in the second year; and lastly, before the first of January 1809, for those made in the third year. The adjudications will be respectively announced in the public gazettes.

Each author may in person receive his prize from the administrative committee of the Imperial University of Vilna, or he may employ a person to whom he shall have given his full procuration. The prize will, according to the election of the candidate, consist either in the sum of money named, or a gold medal of the same value.

The actual professors and honorary members resident in Vilna are not admitted to this competition.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1806.

ARTICLE I.

Letter from a Correspondent, enquiring the Cause why a Swell of the Sea is sometimes observed to precede a Storm from the same quarter. With some observations by the Editor.

To Mr. NICHOLSON.

SIR, *Helston, Cornwall, June* 4, 1806.

IT frequently happens on this coast that a heavy swell of the sea arrives from the westward, without any perceptible cause, which is followed by a gale of wind or storm from the same quarter many hours afterwards. I have observed the same fact on other coasts; and I believe the phenomenon is very generally known and admitted. It is not difficult to form a notion, that an expanded surface of water, undulating in a certain direction, may communicate a progressive motion to the air above it; but in all the theories relating to winds and waves, it has constantly, as far as my knowledge extends, been asserted that the waves are caused by the winds, and not the winds by the waves. We are also told by writers on meteorology, that

An heavy swell often arrives on the Cornish coast, which is followed by a storm.

that a gentle breeze passes at the rate of about fifteen miles an hour; and that the velocity, if progressively augmented to sixty miles an hour, will produce a violent storm, of force sufficient to overthrow trees and houses. Now the velocity of the swell of the sea is so far from being in the least likely to produce any extreme progressive motion in the air, that I really think it never exceeds eight or ten miles an hour. Whatever you may think, Mr. Nicholson, of the importance of this philosophical difficulty, I hope you will have the goodness to propose it to your numerous correspondents. I have occasionally had the pleasure, in my constant perusal of your Journal, to remark that you have yourself sometimes given answers to questions which have been proposed. May I hope that you will not think mine undeserving your consideration.

Question: As the wind is not caused by the swell, but the contrary,—how happens it that the swell comes before the storm?

I am, Sir,

Your obliged reader,

M. M.

Reply. W. N.

Description of a squall at sea: It is a sudden and violent wind with rain.

I do not know that any of our philosophers have expressly considered the appearances which constitute what is called a squall at sea. A strong wind accompanied with rain comes on, almost instantaneously, and the impulse of this wind is sufficient to carry away a ship's topmasts, and even to do more material damage, if navigators were not to hold themselves in readiness to lower their sails as soon as the first impression takes place. The squall is more common in low latitudes than in high latitudes, where its duration is likewise shorter. It usually lasts eight or ten minutes or half an hour, and when it has ceased or passed to leeward, the ordinary wind, with which for the most part it coincides in direction, resumes its course.

Theory of winds. Much attention has been paid to horizontal currents of air, and but little to those streams which are directed upwards or downwards.

Every theory of the winds supposes part of the lower air to ascend, and that its place is supplied by an horizontal current. Very few writers have supposed a descending current to operate in the same way; and seldom indeed has any attention been paid to those direct or oblique ascending or descending winds which must be produced at the places where the causes of motion most powerfully operate.

I appre-

I apprehend the squall to be occasioned by a wind which blows immediately downwards. If we suppose a cloud to be suddenly condensed into drops (no matter by what process of chemistry, electricity, or other general cause, to us but little known), the falling drops or masses of water will occasion a descending current of air by their impulse against those portions of the atmosphere through which they pass. This kind of blast was formerly used in the water-blowing engine, an apparatus which acted with a force capable of supporting a pressure of about three feet of water.* But the tropical rains frequently descend with velocities much exceeding any that could be produced by engines of this kind, and their effects are much more striking.

The squall is a descending wind, —produced by the impulse of falling rain. It resembles the water-blowing engine.

The cloud which affords the descending stream is seldom stationary, but in general carried along by the common wind or lower current of the atmosphere; and the stream of descending water is certainly and in all cases affected with the same horizontal motion. The descending wind, which might otherwise have struck the water perpendicularly, is by this cause made to take an oblique direction, and runs rapidly along that surface, diverging from its principal place of descent in all directions, but most particularly in the direction which the wind already possesses. We must therefore be careful to distinguish two kinds of velocity belonging to the squall or descending current; namely, first, the velocity of the current itself, which is greatest at the place of descent, and diminishes in receding from that place; and, secondly, the velocity of the cloud, or blowing apparatus, which is carried along by the common horizontal current or prevailing wind. The first of these two velocities constitutes, within its limitted sphere of activity, a storm; and the other velocity measures the progress with which the storm is horizontally conveyed along. Let us suppose a stone to be thrown into smooth water; its action will cause a wave to be propagated horizontally to great distances around: In the same manner we can conceive a wave or a swell to be raised by the impulse of descending air, and propagated around in all directions, though modified by the winds it may meet. This swell, moving uniformly, will not be propagated as speedily as the descending current, where this last is most rapid; but

The cloud, which affords the blast, moves horizontally, and therefore gives an oblique impulse to the sea.

The velocity of the blast and that of the clouds are distinct objects; —the first produces a local storm, and the second carries the storm forwards along the sea.

* Lewis, in his Philos. Commerce of Arts.

at a certain distance from the place of greatest storm the swell will proceed, modified only by the common wind which it will either outrun, or follow, or cross, according to circumstances,—nothing being more common at sea than to have the wind in one direction and the swell in another.

All storms are probably descending currents of wind, or extensive squalls.

I strongly suspect that a squall is a storm in miniature, or that all storms are produced by descending streams from the upper part of the atmosphere, caused, in some instances, by the fall of great masses of water, and in others by chemical processes, concerning which we scarcely dare to form a conjecture.

Uncommon squalls or short storms. White squall; typhon; showers of stones; narrow streams of air, &c.

The white squall, or squall without rain, in the Chinese seas; the typhon, or storm of twelve or eighteen hours, which, in the same regions, comes on suddenly, and blows with extreme violence, in succession from almost every point of the compass; the sudden condensation or production of ignited stones, which have so often fallen with great agitation of the atmosphere; the limited streams of air which have been known to rush across the face of a country, making a narrow line of devastation;—these, and many other facts of whirlwinds, waterspouts, explosive noises, and the like, shew that the air may be put into violent motion by other causes as well as its change of elasticity from heat and cold, and the mechanical action of descending water.

Concluding remarks. The swell, caused by a storm, may be propagated with a greater mean velocity than the storm that causes it, and may therefore arrive on a coast before it,

Whatever great and powerful agent may therefore cause the descending air to throw the sea into a swell, it does not necessarily follow that the swell shall exist only in the vicinity of the blast which occasioned it. The centre of action, if it may be so called, may either be stationary, or it may move along with any determinate degree of velocity. From the nature of the case, its motion will be such as to follow or coincide with that of the swell it causes. Whenever a heavy swell arrives upon any coast, it will, according to the doctrine here laid down, indicate that a storm or long continued squall has existed, and probably continues to exist, towards that point of the compass from which the swell arrives. If its progressive velocity and duration be sufficient, the storm will arrive at the coast subsequent to the swell, unless its first generation or commencement was near the shore; but we may suppose, and undoubtedly it often so happens, that the atmospheric cause of the swell may have ceased long before the swell itself has subsided.

—or come after the storm has ceased.

II.

Enquires respecting various subjects relating to the Arts. By JUVENIS. *With some remarks by* W. N.

To Mr. NICHOLSON.

SIR,

YOUR kind attention to your correspondent R. B. vol. vii. 71, has induced me to trouble you with the following queries, not doubting but that from you or your learned correspondents, I shall receive a satisfactory answer to them.

1st. Is there any method of freeing iron boilers from the incrustation left on them by hard water, without subjecting them to a red heat. Copper vessels, I know, are readily so cleaned, but cast iron will not stand that heat without cracking.

Query 1.—Respecting the incrustation left by water upon iron boilers.

2d. Is it possible to reduce printing ink to a fluid state, so as to take a copy of a copper-plate, or printed paper, similar to the method used in copying writing, and this without the original receiving any other damage than that of becoming fainter, which the loss of the ink must inevitably occasion.

Query 2.—On the taking off counterproofs from engravings.

3d. Is there any method of ascertaining the degree of porosity of the metals of reflecting telescopes, except by their performance when polished? Edwards says the porosity arises from the calcination of the tin, and I have tried a number of ways to prevent this, but am at a loss which to prefer, for want of some criterion whereby to judge of the degree of porosity in each separate metal. When the tin is left long in the fire, the metal I know will be bad, but I cannot (when well polished), even with the highest magnifiers, discover any pores in it, though I have no doubt but the badness of its performance arises from that cause. But the performance alone cannot certainly indicate the degree of porosity, since I find metals of the same composition, when ground and polished in every respect the same, seldom or never exactly agree in performance.

Query 3.—Imperfections of speculums.

4th. In a late edition of Ferguson's lectures by Brewster, the following rule is given for making the eye-pieces of telescopes achromatic. With two lenses, the focal length of the first three times

Query 4.—Supposed achromatic eye-glass.

times that of the second, and their distance ¼ the focal length of the first. I must own I do not see the reason why this combination should remove the chromatic aberration, nor does the author give any. He hints also how much superior such combinations of lenses are for magnifiers of solar microscopes; if this is the case, why will not a similar combination answer for the object-glass of a telescope?

That a combination of lenses may diminish or nearly remove the error arising from the spherical figure, I can easily conceive, but not the chromatic aberration.

JUVENIS.

June 2d, 1806.

The above questions may, and to you I dare say will, appear trifling and frivolous; but you will pardon them when you reflect how frequently some little obstacle starts up to stop the course of the young and ardent enquirer, and that unless a kind friend remove it, seems an insurmountable barrier.

Reply.

Singular fact respecting the incrustation in a kettle.

I wish it were in my power to give full information concerning the objects of the present letter. What it may be that incrusts the surface of tea-kettles, and is very troublesome in steam engines, I do not know, but suppose it to be sulphate of lime. Many years ago I was informed, that if an oyster-shell be constantly kept in a tea-kettle there will be no incrustation formed, except upon the shell. If this be true, the same effect would be produced by a loose piece of incrustation, provided it were put in before any deposition had began to fix upon the metal.

Porosity of speculum metal.

2. It does not seem probable that a bad speculum which has no pores discernible by the microscope owes its badness to porosity. When I was formerly busied upon speculums, it was my custom to grind off a small portion of the metal and polish it, in order to determine whether I should bestow an farther labour upon the piece. This polish was often, for the sake of expedition, given with a leather or buff stick; and it always happened that the surface took a wavy appearance; an effect which did not follow when pitch was used as the polisher. I ascribed this irregularity to some parts being softer than

Speculum metal appears wavy if polished on leather.

than others, and on that account yielding more to the pressure of the springy leather; and the differences of hardness seemed to me to have arisen from the nature of the crystallization in cooling. The slower the cooling the larger will be the crystals and the coarser the grain upon fracture. I did not pursue this object far; but I found, and I believe it is general to all castings, that the colder the metal, when poured out, the denser specifically and more uniform in its fracture was the cast. These facts and observations seem to give probability to the following inference: that if two metals were cast by successive pouring out of the same pot, the latter would have more density and uniformity of aggregation and hardness, and would take a better figure upon the polisher, which though of pitch, and supposed to have no elasticity, has, it is most likely, enough of spring to affect a figure which would be essentially injured by a deviation of one millionth part of an inch.*

Cause of this effect supposed to lie in the crystallization in cooling.

It is thought that a cast of metal nearly cold would take the best figure.

3. I do not see how chromatic abberration is to be corrected by refraction towards the same parts, unless by mediums, such as described by Doctor Blair in his paper on aplanatic refraction, in the second volume of the Edinburgh Transactions, of which an abridgment is given in our Quarto Series, vol. i. p. 1. But these do not apply to the case mentioned by Mr. Brewster.

On achromatic eye-pieces.

III.

On the means of obtaining Stamps (Clichés) with Moulds of Plaster of Paris, Sulphur, or Sealingwax. By M. DARCET. *Read at the Society of Encouragement, the* 12*th of February,* 1806.†

FOUNDERS use the term *clicher* for making impressions on metals without casting them in a mould; and that of

Art of stamping, or making impressions in metals.

* Without recurring to optical principles, as to the figure of a speculum, it may be observed that a single stroke on the polisher will sensibly alter that figure; but that a great number of strokes would be required to work out a scratch of one hundred thousand to the inch in breadth.

† Translated from *La Revue Philosophique, Litteraire, et Politique,* No. 10, for April, 1806, p. 1.

cliché

cliché for an impression thus made. The fabrication of *assignats*, or rather the lamentable necessity of reproducing them without end, was in some degree the creating cause of the art of taking metallic impressions (*de clicher*), which does not seem at the first view to be of any great importance; but it is in reality capable of being applied with advantage to several arts, and particularly to those connected with the arts of design.

Improved by M. Darcet.

M. Darcet, the son of the celebrated chemist, in forming a collection of impressions from engraved gems, has just carried this art to a great extent. From an object of amusement, for which he at first began to employ it, he has applied it to the manufactory of paper-hangings, and even of printed cottons, of ornamental furniture, of artificial flowers, and in short of every thing that taste or industry would decorate. It is easy to conceive how much expence may be saved by taking impressions of engravings already executed instead of making new ones.

Its uses.

The following is an account of the advantages, which Mr. Darcet expects from his process, as exhibited in the accurate report delivered by him to the society.

For blocks for paper-hangings and printed goods.

" In the manufacture of paper-hangings and printed goods, the pattern is printed by means of wooden blocks; and these, though cut in a coarse manner and at a moderate price, appeared to me capable of having their place in part supplied by the processes of metallic impression (*clichage*). My trials were successful; and the various specimens laid before the assembly, show that this art may be carried to a great extent, and cannot fail to produce beneficial results if pursued by expert hands: in fact, all that is requisite is to model in wax or plaster of Paris the ornament we would multiply, and to obtain from it as many matrices, and consequently as many metallic stamps as the block is required to contain.

Capable of forming a number of patterns by varying the combination of the same figures.

" This process is particularly applicable to blocks in which the same ornament is repeated several times. It has likewise the advantage of furnishing very speedily a great number of separate pieces, which may afterward be combined in a great variety of ways, to form at a very trifling expence, a number of patterns differing from each other.

In printing calicos the mordants may

" I am aware, that in many cases it will be more advantageous to cut wooden blocks for paper-hangings; and I apprehend

hend that the mordants employed for printed goods may frequently render the use of metallic blocks unadvisable: but a few inconveniences should not lead us to reject a process which may speedily be brought to perfection, as we are led to expect from the purposes to which it has already been applied, and the success obtained. *sometimes be an obstacle to the use of this art.*

"The upholsterer will ornament his goods at a small expence, by multiplying the engraved gems and basso relievoes of antiquity, and the fine productions of modern art, by means of plaster moulds. *In furniture.*

"These ornaments will be in a style as chaste as that of the models, of which they will only be polytypes; and the labours of the carver and sculptor becoming needless, works may be afforded at a low price, of such a degree of perfection, as could not by any other means be attained without considerable charge. *A cheap substitute for carving.*

"The maker of artificial flowers, by employing plaster of Paris, may find a cheap method of procuring solid moulds taken from the leaves themselves, and more perfect than those imitations made by art. *In artificial flower making.*

"The figurer of stuffs too may substitute moulds of metal for those of plaster of Paris, sulphur, and wood, which he commonly employs. *In figuring stuffs.*

"The antiquary may use metallic impressions instead of the plaster casts and sulphurs of his cabinet; and his impressions, thus rendered more solid, will have the advantage of being capable of being multiplied more easily without fear of injuring them, and his collection will be increased by the exchange of copies he may make with others. *In imitating ancient coins and gems.*

"Lastly, this art affords the means of improving engraving in relief; of which I have already exhibited proofs, and more positive demonstration will soon be afforded the Society. I hope it will see brought to perfection, an art almost lost among us, the success and advantages of which, however, concern many branches of national industry. *For engraving in relief, or block cutting.*

"I might mention several other uses of stamps or blocks made with plaster of Paris *(du clichage en plâtre)*, and I conceive there are several cases in which it may be of utility, particularly when the process shall have attained all the perfection of which it is susceptible." *Blocks of plaster.*

 The

The whole report of M. Darcet may be read in the *Bulletin de la Société d' Encouragement*, No. 20, Feb. 1806. It is not a simple process, but an art described in a clear and simple manner, with all its circumstances, such as we could wish the description of every art to be given.

Principal points to be attended to.

The two principal points are the preparation of the moulds, and the metal intended for stamps. Though good stamps may be obtained by letting a perfectly dry plaster mould fall on the fusible compound of bismuth, tin, and lead, M. Darcet has thought that, if he could impart more solidity to the plaster, and fill up the pores that form bubbles or diminish the polish of the stamp, he should improve the art very much. After having tried several means he has preferred Flanders glue.

Method of making the plaster moulds. They are steeped in weak hot glue.

"Three ounces and half avoirdupois weight of fine Flanders glue are to be steeped, and then dissolved by heat in four pints and half of water. The solution being strained through a piece of fine linen or a sieve, is to be heated till it is near boiling, and the plaster moulds, previously dried and slightly heated, are then to be immersed in it. The air they contain expands and escapes, while the water, taking its place, carries with it into the interior parts of the mould the glue which it held in a state of considerable attenuation. As soon as the air ceases to be disengaged, the plaster mould is to be taken out and shaken, and the operator must blow strongly on its engraved surface, that pellicles of glue may not be formed in cooling, which would impair the delicacy of the work."

The moulds thus saturated should be dried slowly; toward the end, however, the temperature may be raised as high as 50° or 60° of the centigrade thermometer (112° or 140° Farht.) The moulds thus saturated with glue must not be used except when very dry; and as they attract the moisture of the air, in consequence of the glue contained in them, they must be kept in a dry place, or heated before they are used, taking care to let them grow cold before the metal is stamped with them.

The moulds must be kept dry, and used dry and cold.

Darcet's fusible metal,

As to the metallic compound, it was invented by M. Darcet, senior. That worthy man, who found no pleasure in his discoveries without rendering them public and useful, made this known in the years 1773 and 1777, by means of the *Journal de Médecine* and the *Journal de Physique*. It has since been employed

employed for the fabrication of assignats, and may be esteemed the foundation of the stereotype art. M. Erhmann will testify this.

—used in fabricating assignats, and the foundation of stereotypes.

In making stamps with moulds of plaster, the success depends greatly on the fusibility of the metal; that employed by M. Darcet is the most fusible known.

The most fusible known.

"Numerous experiments," says M. Darcet, "convinced my father, that the compound should be formed of eight parts of bismuth, five of lead, and three of tin. When it is well made, it begins to soften at 91° of the centigrade thermometer (196° Farht.), and melts between 92° and 93° (about 199° Farht.).

8 parts bismuth, 5 lead, 3 tin, melts 13 degrees below boiling water.

"This compound when cold is sufficiently malleable to stand a blow, and hard enough to preserve the impressions from friction, and to allow them to be retouched with the graver.

Sufficiently hard when cold.

"The following is the best mode of preparing it:

"Fuse the bismuth, cover it with resin or suet, and heat the whole rather strongly; add the lead, stir them well together, increase the temperature a little, and add to the metals in fusion the requisite quantity of tin. Stir the mixture again, and cast it into a plate or ingot.

Mode of preparing it.

"This compound is to be used only in the soft state, that is, at a degree of heat much below the point of boiling water; it is then fitted for receiving the impression of the matrix, without emitting the air and water this contains, which would form blebs or flaws in the stamp.

To be used only when soft.

"The soft state is that which the fusible metal assumes at 91° (196° Farht.). When it begins to grow solid, a crystallization forms in the still fluid mass of metal; and this crystallization must be broken and rendered confused, by agitating the metal as quickly as possible, kneading it as it were, and particularly by bringing the part near the edges to the centre, and alternately carrying the central part to the edges.

Directions for using it.

"When the whole of the metal is reduced to this state, nothing more is necessary, but to stamp it quickly with the plaster matrix.

"Every time this compound is fused, oxided pellicles are formed on its surface; and the greater heat, or the longer it is continued, the more considerable they are. These scoriæ should be collected together, and afterwards fused with resin,

Scoriæ,

 oil,

to be removed. oil, or tallow, at a temperature sufficiently high to reduce them to the metallic state, so that they may be used in subsequent operations."

The manipulation easy.

Nothing more remains but the mechanical process, which is easy, and for which we must refer to the Bulletin of the *Société d' Encouragement* already quoted. This Bulletin should be consulted by all who would know the minutiæ of the art.

Fidelity of the process.

We shall conclude by observing, that the fidelity of the impressions or stamps *(clichés)* is so great, as to give with the greatest accuracy every ramification in a rose leaf.

During the fabrication of assignats, they were stamped both by the hand and by engines. Like all other useful processes, this was soon improved in consequence of the necessity of repeating it. The operation is rendered more expeditious and more exact by means of a simple machine. A plate added to the paper of M. Darcet, and engraved in the Bulletin of the *Société d' Encouragement*, represents this machine which is described in M. Darcet's paper.

The stamping performed by a simple machine.

To this abstract were subjoined some impressions taken with plaster of Paris from wooden cuts of M. Duplat, and then worked off from stamps made with M. Darcet's compound metal.*

IV.

Account of a series of Experiments, shewing the Effects of Compression in modifying the Effects of Heat. By Sir JAMES HALL, *Bart. F. R. S. Edinburgh.*

(Continued from page 128.)

SECT. VIII.—*Formation of Coal.—Accidental occurrence which led me to undertake these Experiments.—Results extracted from a former publication.—Explanation of some*

* The prints in *La Revue* exactly resemble wood-cuts. The press-work not being performed in a better manner than common, it is not possible to form any judgment of the minute differences.

diffi-

difficulties that have been suggested.—The Fibres of Wood in some cases obliterated, and in some preserved under compression.—Resemblance which these Results bear to a series of Natural Substances described by Mr. Hatchett.—These results seem to throw light on the history of Surturbrand.

As I intend, on some future occasion, to resume my experiments with inflammable substances, which I look upon as far from complete, I shall add but a few observations to what I have already laid before this Society, in the sketch I had the honour to read in this place on the 30th of August last.

Inflammable substances.

The following incidental occurrence led me to enter upon this subject rather prematurely, since I had determined first to satisfy myself with regard to the carbonate of lime.

Incidental occurrence wherein leather was exposed to heat under compression,

Observing, in many of the last-mentioned class of experiments, that the elastic matters made their escape between the muzzle of the barrel and the cylinder of lead, I was in the habit, as mentioned above, of placing a piece of leather between the lead and the barrel; in which position, the heat to which the leather was exposed, was necessarily below that of melting lead. In an experiment, made on the 28th of November 1803, in order to ascertain the power of the machinery, and the quantity of metal driven out by the expansion of the liquid, there being nothing in the barrel but metal, I observed, as soon as the compressing apparatus was removed, (which on this occasion was done while the lower part of the barrel was at its full heat, and the barrel standing brim full of liquid metal), that all the leather which lay on the outside of the circular muzzle of the barrel, remained, being only a little browned and crumpled by the heat to which it had been exposed. What leather lay within the circle, had disappeared; and on the surface of the liquid metal, which stood up to the lip of the barrel, I saw large drops of a shining black liquid, which, on cooling, fixed into a crisp black substance, with a shining fracture, exactly like pitch or pure coal. It burned, though not with flame. While hot, it smelt decidedly of volatile alkali. The important circumstance here, is the different manner in which the heat had acted on the leather, without and within the rim of the barrel. The only difference consisted in compression, to which, therefore, the difference of effect must be ascribed: by its force, the volatile matter

—and converted into coaly matter.

matter of the leather which escaped from the outward parts, had within the rim, been constrained to remain united to the rest of the composition, upon which it had acted as a flux, and the whole together had entered into a liquid state, in a very low heat. Had the pressure been continued till all was cool, these substances must have been retained, producing a real coal.

Direct experiments.

On the 24th April 1803, a piece of leather used in a similar manner, (the compressing force being continued, however, till all was cold,) was changed to a substance like glue, owing doubtless to compression in a heat under that of melting lead.

These observations led me to make a series of experiments with animal and vegetable substances, and with coal; the result of which I have already laid before the Society. I shall now repeat that communication, as printed in Nicholson's Journal for October last (1804.)*

Pit coal exposed to heat under compression.

" I have likewise made some experiments with coal, treated in the same manner as the carbonate of lime: but I have found it much less tractable; for the bitumen, when heat is applied to it, tends to escape by its simple elasticity, whereas the carbonic acid in marble, is in part retained by the chemical force of quicklime. I succeeded, however, in constraining the bituminous matter of the coal, to a certain degree, in red-heats, so as to bring the substance into a complete fusion, and to retain its faculty of burning with flame. But, I could not accomplish this in heats capable of agglutinating the carbonate; for I have found, where I rammed them successively into the same tube, and where the vessel has withstood the expansive force, that the carbonate has been agglutinated into a good limestone, but that the coal has lost about half its weight, together with its power of giving flame when burnt, remaining in a very compact state, with a shining fracture. Although this experiment has not afforded the desired result, it answers another purpose admirably well. It is known, that where a bed of coal is crossed by a dike of whinstone, the coal is found in a peculiar state in the immediate neighbourhood of the whin: the sub-

When some of the volatile matter escaped a production

* As the present extract is short, it was thought better to repeat it here, than to interrupt the subject by referring to our ixth volume. Ed.

stance

stance in such places being incapable of giving flame, it is distinguished by the name of *blind coal*. Dr. Hutton has explained this fact, by supposing that the bituminous matter of the coal, has been driven by the local heat of whin, into places of less intensity, where it would probably be retained by distillation. Yet the whole must have been carried on under the action of a pressure capable of constraining the carbonic acid of the calcareous spar, which occurs frequently in such rocks. In the last-mentioned experiment, we have a perfect representation of the natural fact; since the coal has lost its petroleum, while the chalk in contact with it has retained its carbonic acid.

resembling blind coal was afforded.

"I have made some experiments of the same kind, with vegetable and animal substances. I found their volatility much greater than that of coal, and I was compelled, with them, to work in heats below redness; for, even in the lowest red-heat, they were apt to destroy the apparatus. The animal substance I commonly used was horn, and the vegetable, saw-dust of fir. The horn was incomparably the most fusible and volatile of the two. In a very slight heat, it was converted into a yellow-red substance, like oil, which penetrated the clay tubes through and through. In these experiments, I therefore made use of tubes of glass. It was only after a considerable portion of the substance had been separated from the mass, that the remainder assumed the clear black peculiar to coal. In this way I obtained coal, both from saw-dust and from horn, which yielded a bright flame in burning.

Animal and vegetable matters treated in the same manner.

"The mixture of the two produced a substance having exactly the smell of soot or coal-tar. I am therefore strongly inclined to believe, that animal substance, as well as vegetable, has contributed towards the formation of our bituminous strata. This seems to confirm an opinion, advanced by Mr. Keir, which has been mentioned to me since I made this experiment. I conceive, that the coal which now remains in the world, is but a small portion of the organic matter originally deposited: the most volatile parts have been driven off by the action of heat, before the temperature had risen high enough to bring the surrounding substance into fusion, so as to confine the elastic fluids, and subject them to compression.

It is probable that animal as well as vegetable matter has contributed to our bituminous strata.

"In

Animal matter totally dissipated under compression, which would else have left a coak.

"In several of these experiments, I found, when the pressure was not great, when equal, for instance, only to 80 atmospheres, that the horn employed was dissipated entirely, the glass tube which had contained it being left almost clean : yet undoubtedly, if exposed to heat without compression, and protected from the contact of the atmosphere, the horn would leave a cinder or coak behind it, of matter wholly devoid of volatility. Here, then, it would seem as if the moderate pressure, by keeping the elements of the substance together, had promoted the general volatility, without being strong enough to resist that expansive force, and thus, that the whole had escaped. This result, which I should certainly not have foreseen in theory, may perhaps account for the absence of coal in situations where its presence might be expected on principles of general analogy."

Since this publication, a very natural question has been put to me. When the inflammable substance has lost weight, or when the whole has been dissipated, in these experiments, what has become of the matter thus driven off?

It is uncertain what became of the matters volatilized under pressure.

I must own, that to answer this question with perfect confidence, more experiments are required. But, in the course of practice, two circumstances have occurred as likely, in most cases, to have occasioned the loss alluded to. I found in these experiments, particularly with horn, that the chalk, both in powder and in lump, which was used to fill vacuities in the tubes, and to fix them in the cradle, was strongly impregnated with an oily or bituminous matter, giving to the substance the qualities of a stinkstone. I conceive, that the most volatile part of the horn has been conveyed to the chalk, partly in a state of vapour, and partly by boiling over the lips of the glass tube; the whole having been evidently in a state of very thin fluidity. Having, in some cases, found the tube, which had been introduced full of horn, entirely empty after the experiment, I was induced, as above stated, to conceive, that, under pressure, it had acquired a greater general volatility than it had in freedom; and I find that, in the open fire, horn yields a charcoal equal to 20 *per cent.* of the original weight. But more experiments must be made on this subject.

Another

Another cause of the loss of weight, lay undoubtedly in the excess of heat employed in most of them, to remove the cradle from the barrel. With inflammable substances, no air-tube was used; and the heats being low, the air lodged in interstices had been sufficient to secure the barrels from destruction, by the expansion of the liquid metal. In this view, likewise, I often used lead, whose expansion in such low heats, I expected to be less than that of the fusible metal. And the lead requiring to melt it, a heat very near to that of redness, the subject of experiment was thus, on removing the cradle, exposed in freedom to a temperature which was comparatively high. But, observing that a great loss was thus occasioned, I returned to the use of the fusible metal, together with my former method of melting it, by plunging the barrel, when removed from the furnace, into a solution of muriate of lime, by which it could only receive a heat of 250° of Fahrenheit. Some part was lost in taking the subject out of the apparatus.

The effect was remarkable, in the few experiments tried in this way. The horn did not, as in the other experiments, change to a hard black substance, but acquired a semifluid and viscid consistency, with a yellow-red colour, and a very offensive smell. This shews, that the substances which here occasioned both the colour and smell of the results, had been driven off in the other experiments, by the too great heat applied to the substance, when free from compression. Observation.

I found that the organization of animal substance was entirely obliterated by a slight action of heat, but that a stronger heat was required to perform the entire fusion of vegetable matter. This, however, was accomplished; and in several experiments, pieces of wood were changed to a jet-black and inflammable substance, generally very porous, in which no trace could be discovered of the original organization. In others, the vegetable fibres were still visible, and are forced asunder by large and shining air-bubbles. Slight heat destroys the organization of animal matter; vegetable requires strong heat.

Since the publication of the sketch of my experiments, I have had the pleasure to read Mr. Hatchett's very interesting account of various natural substances, nearly allied to coal; and I could not help being struck with the resemblance which my results bear to them, through all their varieties, as brought into view by that able chemist; that resemblance affording a pre- Substances mentioned by Mr. Hatchett and elucidated by these experiments.

presumption; that the changes which, with true scientific modesty, he ascribes to an unknown cause, may have resulted from various heats acting under pressure of various force. The substance to which he has given the name of *Retinasphaltum*, seems to agree very nearly with what I have obtained from animal substance, when the barrel was opened by means of low heat. And the specimen of wood entering into fusion, but still retaining the form of its fibres, seems very similar to the intermediate substance of Bovey-coal and *Surturbrand*, which Mr. Hatchett has assimilated to each other. It is well known, that the surturbrand of Iceland, consists of the stems of large trees, flattened to thin plates, by some operation of nature hitherto unexplained. But the last-mentioned experiment seems to afford a plausible solution of this puzzling phenomenon.

Application of the author's experiments to other natural events.

In all parts of the globe, we find proofs of slips, and various relative motions, having taken place amongst great masses of rock, whilst they were soft in a certain degree, and which have left unequivocal traces behind them, both in the derangements of the beds of strata, and in a smooth and shining surface, called *slickenside*, produced by the direct friction of one mass on another. During the action of subterranean heat, were a single stratum to occur, containing trees intermixed with animal substances, shell-fish, &c. these trees would be reduced, to a soft and unctuous state, similar to that of the piece of wood in the last-mentioned experiment, whilst the substance of the contiguous strata retained a considerable degree of firmness. In this state of things, the stratum just mentioned, would very naturally become the scene of a slip, occasioned by the unequal pressure of the surrounding masses. By such a sliding motion, accompanied by great compression, a tree would be flattened, as any substance is ground in a mortar, by the combination of a lateral and direct force. At the same time, the shells along with the trees, would be flattened, like those described by Bergman; while those of the same species in the neighbouring limestone-rock, being protected by its inferior fusibility, would retain their natural shape.

SECT.

SECT. IX.—*Application of the foregoing results to Geology.—The fire employed in the Huttonian Theory is a modification of that of the Volcanoes.—This modification must take place in a lava previous to its eruption.—An Internal Lava is capable of melting Limestone.—The effects of Volcanic Fire on substances in a subterranean and submarine situation, are the same as those ascribed to Fire in the Huttonian Theory.—Our Strata were once in a similar situation, and then underwent the action of fire.—All the conditions of the Huttonian Theory being thus combined, the formation of all Rocks may be accounted for in a satisfactory manner.—Conclusion.*

Having investigated, by means of the foregoing experiments, some of the chemical suppositions involved in the Huttonian Theory, and having endeavoured to assign a determinate limit to the power of the agents employed; I shall now apply these results to Geology, and inquire how far the events supposed anciently to have taken place, accord with the existing state of our globe. The results applied to geology.

The most powerful and essential agent of the Huttonian Theory, is Fire, which I have always looked upon as the same with that of volcanoes, modified by circumstances which must, to a certain degree, take place in every lava previous to its eruption. Fire is the essential agent of the Huttonian theory.

The original source of internal fire is involved in great obscurity; and no sufficient reason occurs to me for deciding whether it proceeds by emanation from some vast central reservoir, or is generated by the local operation of some chemical process. Nor is there any necessity for such a decision: all we need to know is, that internal fire exists, which no one can doubt, who believes in the eruptions of Mount Vesuvius. To require that a man should account for the generation of internal fire, before he is allowed to employ it in geology, is no less absurd than it would be to prevent him from reasoning about the construction of a telescope, till he could explain the nature of the sun, or account for the generation of light.* To account for the origin of that fire is no part of the doctrine. It is considered as a mere fact.

* This topic, however, has of late been much urged against us, and an unfair advantage has been taken of what Mr. Playfair has said upon it. What he gave as mere conjecture on a subject of collateral importance, has been argued upon as the basis and fundamental doctrine of the system.

But while we remain in suspense as to the prime cause of this tremendous agent, many circumstances of importance with regard to it, may fairly become the subjects of observation and discussion.

Volcanic fire is deep seated,

Some authors (I conceive through ignorance of the facts) have alleged, that the fire of Ætna and Vesuvius is merely superficial. But the depth of its action is sufficiently proved, by the great distance to which the eruptive percussions are felt, and still more, by the substances thrown out uninjured by some eruptions of Mount Vesuvius. Some of these, as marble and gypsum, are incapable in freedom of resiting the action of fire. We have likewise granite, schistus, gneiss, and stones of every known class, besides many which have never, on any other occasion, been found at the surface of our globe. The circumstance of these substances having been thrown out, unaffected by the fire, proves, that it has proceeded from a source, not only as deep, but deeper, than their native beds; and as they exhibit specimens of every class of minerals, the formation of which we pretend to explain, we need inquire no further into the depth of the Vesuvian fire, which has thus been proved to reach below the range of our speculations.

— and subject to perpetual and irregular variations.

Volcanic fire is subject to perpetual and irregular variations of intensity, and to sudden and violent renewal, after long periods of absolute cessation. These variations and intermissions, are likewise essential attributes of fire as employed by Dr. Hutton; for some geological scenes prove, that the indurating cause has acted repeatedly on the same substance, and that, during the intervals of that action, it had ceased entirely. This circumstance affords a complete answer to an argument lately urged against the Huttonian Theory, founded on the waste of heat which must have taken place, as it is alleged, through the surface. For if, after absolute cessation, a power of renewal exists in nature, the idea of waste by continuance is quite inapplicable.

The external phenomena of volcanoes are sufficiently well known; but our subject leads us to inquire into their internal actions. This we are enabled to do by means of the foregoing experiments, in so far as the carbonate of lime is concerned.

Some

Some experiments which I formerly * laid before this Society and the public, combined with those mentioned in this paper, prove, that the feeblest exertions of volcanic fire, are of sufficient intensity to perform the agglutination, and even the entire fusion, of the carbonate of lime, when its carbonic acid is effectually confined by pressure; for though lava, after its fusion, may be made, in our experiments, to congeal into a glass, in a temperature of 16° or 18° of Wedgwood, in which temperature the carbonate would scarcely be affected; it must be observed, that a similar congelation is not to be looked for in nature; for the mass, even of the smallest stream of lava, is too great to admit of such rapid cooling. And, in fact, the external part of a lava is not vitreous, but consists of a substance which, as my experiments have proved, must have been congealed in a heat of melting silver, that is, in 22° of Wedgwood; while its internal parts bear a character indicating that they congealed in 27° or 28° of the same scale. It follows, that no part of the lava, while it remained liquid, can have been less hot than 22° of Wedgwood. Now, this happens to be a heat, in which I have accomplished the entire fusion of the carbonate of lime, under pressure. We must therefore conclude, that the heat of a running lava is always of sufficient intensity to perform the fusion of limestone.

A low volcanic heat will fuse the carbonate of lime.

Lavas, congealed between 22° and 28°.

In every active volcano, a communication must exist between the summit of the mountain and the unexplored region, far below its base, where the lava has been melted, and whence it has been propelled upwards; the liquid lava rising through this internal channel, so as to fill the crater to the brim, and flow over it. On this occasion, the sides of the mountain must undergo a violent hydrostatical pressure outwards, to which they often yield by the formation of a vast rent, through which the lava is discharged in a lateral eruption, and flows in a continued stream sometimes during months. On Ætna most of the eruptions are so performed; few lavas flowing from the summit, but generally breaking out laterally, at very elevated stations. At the place of delivery, a quantity of gaseous matter is propelled violently upwards, and, along with it, some liquid lava; which last, falling back again in a spongy state, produces one of those conical hills which we see

The phenomena of active volcanoes.

* *Edinburgh Transactions*, vol. v. part I. p. 60—66.

in

in great number on the vast sides of Mount Ætna, each indicating the discharge of a particular eruption. At the same time, a jet of flame and smoke issues from the main crater, proving the internal communication between it and the lava; this discharge from the summit generally continuing, in a greater or a less degree, during the intervals between eruptions. (Fig. 41. represents an ideal section of Mount Ætna; *a b* is the direct channel, and *b c* is a lateral branch.)

The pressure of 600 feet of liquid lava is required to constrain the acid of carbonate.

Let us now attend to the state of the lava within the mountain, during the course of the eruption; and let us suppose, that a fragment of limestone, torn from some stratum below, has been included in the fluid lava, and carried up with it. By the laws of hydrostatics, as each portion of this fluid sustains pressure in proportion to its perpendicular distance below the point of discharge, that pressure must increase with the depth. The specific gravity of solid and compact lava is nearly 2.8; and its weight, when in a liquid state, is probably little different. The table shews, that the carbonic acid of limestone cannot be constrained in heat by a pressure less than that of 1708 feet of sea, which corresponds nearly to 600 feet of liquid lava. As soon, then, as our calcareous mass rose to within 600 feet of the surface, its carbonic acid would quit the lime, and, assuming a gaseous form, would add to the eruptive effervescence. And this change would commonly begin in much greater depths, in consequence of the bubbles of carbonic acid, and other substances in a gaseous form, which, rising with the lava, and through it, would greatly diminish the weight of the column, and would render its pressure on any particular spot extremely variable. With all these irregularities, however, and interruptions, the pressure would in all cases, especially where the depth was considerable, far surpass what it would have been under an equal depth of water. Where the depth of the stream, below its point of delivery, amounted, then, to 1708 feet, the pressure, if the heat was not of excessive intensity, would be more than sufficient to constrain the carbonic acid, and our limestone would suffer no calcination, but would enter into fusion; and if the eruption ceased at that moment, would crystallize in cooling along with the lava, and become a nodule of calcareous spar. The mass of lava, containing this nodule, would then constitute a real whinstone, and would belong

Calcareous spar formed by volcanic fusion in the winstone.

belong to the kind called *amygdaloid.* In greater depths still, the pressure would be proportionally increased, till sulphur, and even water, might be constrained; and the carbonate of lime would continue undecomposed in the highest heats.

Submarine eruptions,

If, while the lava was in a liquid state, during the eruption or previous to it, a new rent (*d e*, Fig. 41.), formed in the solid country below the volcano, was met by our stream (at *d*), it is obvious that the lava would flow into the aperture with great rapidity, and fill it to the minutest extremity, there being no air to impede the progress of the liquid. In this manner, a stream of lava might be led from below to approach the bottom of the sea (*f f*), and to come in contact with a bed of loose shells (*g g*), lying on that bottom, but covered with beds of clay, interstratified, as usually occurs, with beds of sand, and other beds of shells. The first effect of heat would be to drive off the moisture of the lowest shell-bed, in a state of vapour, which, rising till it got beyond the reach of the heat, would be condensed into water, producing a slight motion of ebullition, like that of a vessel of water, when it begins to boil, and when it is said to simmer. The beds of clay and sand might thus undergo some heaving and partial derangement, but would still possess the power of stopping, or of very much impeding, the descent of water from the sea above; so that the water which had been driven from the shells at the bottom, would not return to them, or would return but slowly; and they would be exposed dry to the action of heat.*

and formation of limestone.

In this case, one of two things would inevitably happen. Either the carbonic acid of the shells would be driven off by the heat, producing an incondensable elastic fluid, which, heaving up or penetrating the superincumbent beds, would force its way to the surface of the sea, and produce a submarine eruption, as has happened at Santorini and elsewhere; or the volatility of the carbonic acid would be repressed by the weight of the superincumbent water (*k k*), and the shell-bed, being softened or fused by the action of heat, would be converted into a stratum of limestone.

* This situation of things, is similar to what happens when small-coal is moistened, in order to make it cake. The dust, drenched with water, is laid upon the fire, and remains long wet, while the heat below suffers little or no abatement.

The

The foregoing experiments enable us to decide in any particular case, which of these two events must take place, when the heat of the lava and the depth of the sea are known.

Application of the preceding experiments to determine the formation of limestone.

The table shews, that under a sea no deeper than 1708 feet, near one-third of a mile, a limestone would be formed by proper heat; and that, in a depth of little more than one mile, it would enter into entire fusion. Now, the common soundings of mariners extend to 200 fathoms, or 1200 feet. Lord Mulgrave* found bottom at 4680 feet, or nearly nine-tenths of a mile; and Captain Ellis let down a sea-gage to the depth of 5346 feet.† It thus appears, that at the bottom of a sea, which would be sounded by a line much less than double of the usual length, and less than half the depth of that sounded by Lord Mulgrave, limestone might be formed by heat; and that, at the depth reached by Captain Ellis, the entire fusion would be accomplished, if the bed of shells were touched by a lava at the extremity of its course, when its heat was lowest. Were the heat of the lava greater, a greater depth of sea would, of course, be requisite to constrain the carbonic acid effectually; and future experiments may determine what depth is required to co-operate with any given temperature. It is enough for our present purpose to have shewn, that the result is possible in any case, and to have circumscribed the necessary force of these agents within moderate limits. At the same time it must be observed, that we have been far from stretching the known facts; for when we compare the small extent of sea in which any soundings can be found, with that of the vast unfathomed ocean, it is obvious, that in assuming a depth of one mile or two, we fall very short of the medium. M. de la Place, reasoning from the phenomena of the tides, states it as highly probable that this medium is not less than eleven English miles.‡

It might be formed at less than double the usual depth of soundings.

Less depths would afford the same pres-

If a great part or the whole of the superincumbent mass consisted, not of water, but of sand or clay, then the depth re-

* *Voyage towards the North Pole*, p. 142.

† *Philosophical Transactions*, 1751, p. 212.

‡ "On peut donc regarder au moins comme très probable, que "la profondeur moyenne de la mer n'est pas au-dessous de quatre "lieues" De la Place, *Hist. de l'Acad. Roy. des Sciences*, année, 1776.

quisite

quisite to produce these effects would be lessened, in the inverse ratio of the specific gravity. If the above-mentioned occurrence took place under a mass composed of stone firmly bound together by some previous operation of nature, the power of the superincumbent mass, in opposing the escape of carbonic acid, would be very much increased by that union and by the stiffness or tenacity of the substance. We have seen numberless examples of this power in the course of these experiments, in which barrels, both of iron and porcelain, whose thickness did not exceed one-fourth of an inch, have exerted a force superior to the mere weight of a mile of sea. Without supposing that the substance of a rock could in any case act with the same advantage as that of a uniform and connected barrel; it seems obvious that a similar power must, in many cases, have been exerted to a certain degree.

sures against carbonic acid if the superior mass were heavier or firmer.

We know of many calcareous masses which, at this moment, are exposed to a pressure more than sufficient to accomplish their entire fusion. The mountain of Saleve, near Geneva, is 500 French fathoms, or nearly 3250 English feet, in height, from its base to its summit. Its mass consists of beds, lying nearly horizontal, of limestone filled with shells. Independently, then, of the tenacity of the mass, and taking into account its mere weight, the lowest bed of this mountain, must, at this moment, sustain a pressure of 3250 feet of limestone, the specific gravity of which is about 2.65. This pressure, therefore, is equal to that of 8612 feet of water, being nearly a mile and a half of sea, which is much more than adequate, as we have shewn, to accomplish the entire fusion of the carbonate, on the application of proper heat. Now, were an emanation from a volcano, to rise up under Saleve, and to penetrate upwards to its base, and stop there; the limestone to which the lava approached, would inevitably be softened, without being calcined, and, as the heat retired, would crystallize into a saline marble.

Instance in Mount Saleve, where this pressure actually exists.

Some other circumstances, relating to this subject, are very deserving of notice, and enable us still further to compare the ancient and modern operations of fire.

It appears, at first sight, that a lava having once penetrated the side of a mountain, all subsequent lavas should continue,

Ancient lavas stop up side passages thro' which other

eruptions might else have passed.

as water would infallibly do, to flow through the same aperture. But there is a material difference in the two cases. As soon as the lava has ceased to flow, and the heat has begun to abate, the crevice through which the lava had been passing, remains filled with a substance, which soon agglutinates into a mass, far harder and firmer than the mountain itself. This mass, lying in a crooked bed, and being firmly welded to the sides of the crevice, must oppose a most powerful resistance to any stream tending to pursue the same course. The injury done to the mountain by the formation of the rent, will thus be much more than repaired; and in a subsequent eruption, the lava must force its way through another part of the mountain or through some part of the adjoining country. The action of heat from below, seems in most cases to have kept a channel open through the axis of the mountain, as appears by the smoke and flame which is habitually discharged at the summit during intervals of calm. On many occasions, however, this spiracle seems to have been entirely closed by the consolidation of the lava, so as to suppress all emission. This happened to Vesuvius during the middle ages. All appearance of fire had ceased for five hundred years, and the crater was covered with a forest of ancient oaks, when the volcano opened with fresh vigour in the sixteenth century.

Even the perpendicular vent may be closed, as happened to Vesuvius for half a century.

The opening of a Volcano in many cases is effected with prodigious explosion and disruption.

The eruptive force, capable of overcoming such an obstacle, must be tremendous indeed, and seems in some cases to have blown the volcano itself almost to pieces. It is impossible to see the Mountain of Somma, which, in the form of a crescent, embraces Mount Vesuvius, without being convinced that it is a fragment of a large volcano, nearly concentric with the present inner cone, which, in some great eruption, had been destroyed all but this fragment. In our own times, an event of no small magnitude has taken place on the same spot; the inner cone of Vesuvius having undergone so great a change during the eruption in 1794, that it now bears no resemblance to what it was when I saw it in 1785.

Hence the earthquakes preceding common eruptions.

The general or partial stagnation of the internal lavas at the close of each eruption seems, then, to render it necessary, that in every new discharge, the lava should begin by making a violent laceration. And this is probably the cause of those tremendous earthquakes which precede all great eruptions, and

and which cease as soon as the lava has found a vent. It seems but reasonable to ascribe like effects to like causes, and to believe that the earthquakes which frequently desolate countries not externally volcanic, likewise indicate the protrusion from below of matter in liquid fusion, penetrating the mass of rock.

The varying resistances opposed by old lavas to the escape of new, occasions great variety in the phenomena.

The injection of a whinstone-dike into a frail mass of shale and sandstone, must have produced the same effects upon it that the lava has just been stated to produce on the loose beds of volcanic scoria. One stream of liquid whin, having flowed into such an assemblage, must have given it great additional weight and strength: so that a second stream coming like the first, would be opposed by a mass, the laceration of which would produce an earthquake, if it were overcome; or by which, if it resisted, the liquid matter would be compelled to penetrate some weaker mass, perhaps at a great distance from the first. The internal fire being thus compelled perpetually to change the scene of its action, its influence might be carried to an indefinite extent: So that the intermittance in point of time, as well as the versatility in point of place, already remarked as common to the Huttonian and Volcanic fires, are accounted for on our principles. And it thus appears, that whinstone possesses all the properties which we are led by theory to ascribe to an internal lava.

This connection is curiously illustrated by an intermediate case between the results of external and internal fire, displayed in an actual section of the ancient part of Vesuvius, which occurs in the Mountain of Somma mentioned above. I formerly described this scene in my paper on Whinstone and Lava; and I must beg leave once more to press it upon the notice of the public, as affording to future travellers a most interesting field of geological inquiry.

Remarkable facts exhibited in the vertical Cliff of Somma.

The section is seen in the bare vertical cliff, several hundred feet in height, which Somma presents to the view from the little valley, in form of a crescent, which lies between Somma and the interior cone of Vesuvius, called the *Atrio del Cavallo*. (Fig. 42. represents this scene, done from the recollection of what I saw in 1785. *a b c* is the interior cone of Vesuvius; *d f g* the mountain of Somma; and *c d e* the *Atrio del Cavello*). By means of this cliff (*f d* in Fig. 42. and which is represented

 sepa-

separately in Fig. 44.), we see the internal structure of the mountain, composed of thick beds *(k k)* of loose scoria, which have fallen in showers; between which thin but firm streams *(m m)* of lava are interposed, which have flowed down the outward conical sides of the mountain. (Fig. 43. is an ideal section of Vesuvius and Somma, through the axis of the cones, shewing the manner in which the beds of scoria and of lava lie upon each other; the extremities of which beds are seen edgewise in the cliff at *m m* and *k k*, Fig. 42. 43. and 44.)

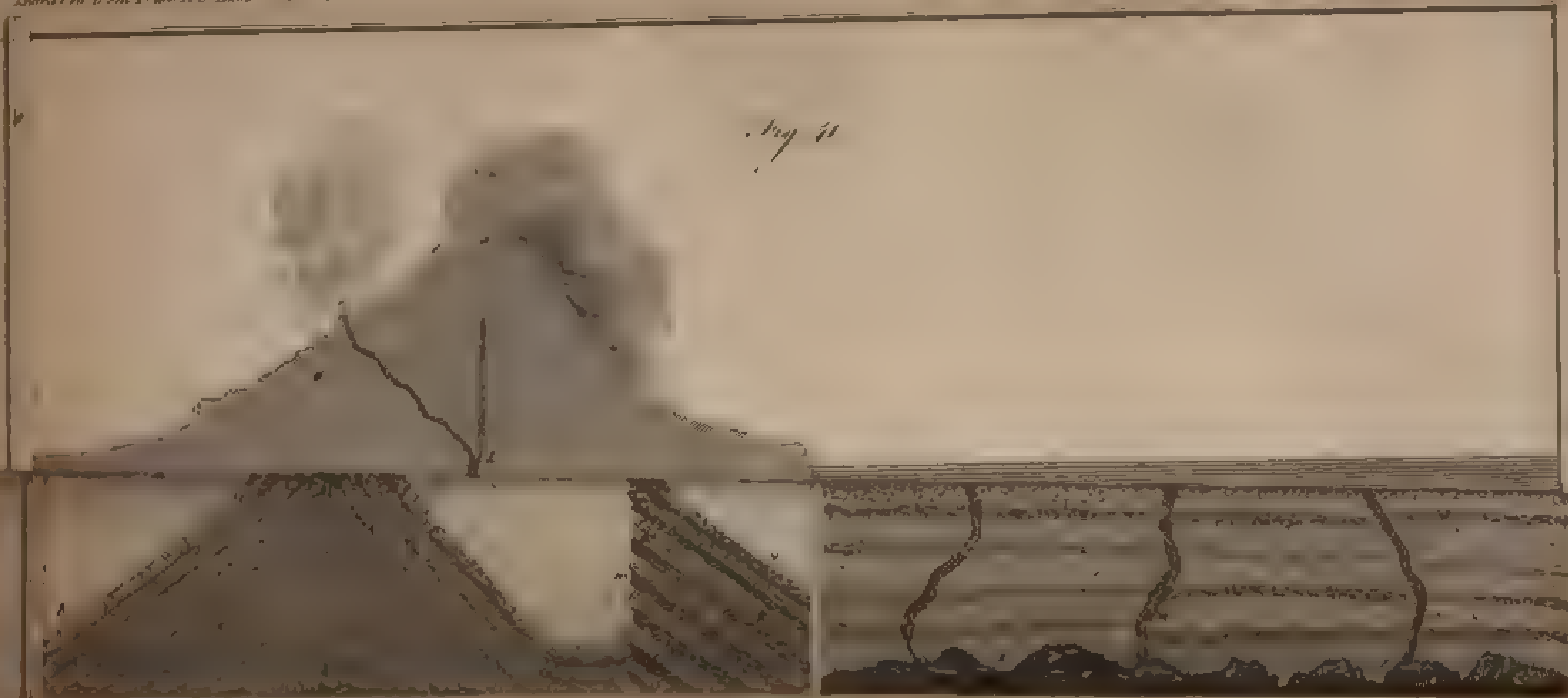
Explanation. This assemblage of scoria and lava is traversed abruptly and vertically, by streams of solid lava (*n n*, Fig. 44.) reaching from top to bottom of the cliff. These last I conceive to have flowed in rents of the ancient mountain, which rents had acted as pipes through which the lavas of the lateral eruptions were conveyed to the open air. This scene presents to the view of an attentive observer, a real specimen of those internal streams which we have just been considering in speculation, and they may exhibit circumstances decisive of the opinions here advanced. For, if one of these streams had formerly been connected with a lateral eruption, discharged at more than 600 feet above the *Atrio del Cavallo*, it might possibly contain the carbonate of lime. But could we suppose that depth to extend to 1708 feet, the interference of air-bubbles, and the action of a stronger heat than was merely required for the fusion of the carbonate, might have been overcome.

Larger scale of operation at Mount Ætna. Perhaps the height of Vesuvius has never been great enough for this purpose. But could we suppose Ætna to be cleft in two, and its structure displayed, as that of Vesuvius has just been described, there can be no doubt that internal streams of lava would be laid open, in which the pressure must have far exceeded the force required to constrain the carbonic acid of limestone; since that mountain occasionally delivers lavas from its summit, placed 10.954 feet above the level of the Mediterranean,* which washes its base. I recollect having seen, in some parts of Ætna, vast chasms and crags, formed by volcanic revolutions, in which vertical streams of lava, similar to those of Somma, were apparent. But my attention not having been turned to that object till many years afterwards, I have only now to recommend the investigation of this interesting point to future travellers.

(To be concluded in our next.)

* Phil. Trans. 1777, p. 595.

PLATE V.



V.

*Instructions for building very strong and durable Walls and Houses of any dimensions of common unprepared Earth, rammed into Moulds, by the method called Pisé, which has been practised from the earlies times in the vicinity of Lyons, and elsewhere.**

Strong and durable houses built of earth only.

IN the year 1791, a work was published at Paris, by M. Francois Cointeraux, containing an account of a method of building strong and durable houses with no other materials than earth, which has been practised for ages in the province of Lyons. It appeared to be attended with so many advantages, that several gentlemen of England, who employ their leisure in the study of rural economy, were induced to make trial of its efficacy; and the event of their experiments has rendered them anxious to extend, by all possible means, the knowledge and practice of so beneficial an art. With a view to promote this desirable end, the account contained in the following pages has been extracted from the French work; and it will be found to contain every necessary information by those into whose hands the original may not have fallen, or who, being unacquainted with the language, may have been prevented from consulting it. The appearance of those wretched hovels which are built with mud, in most parts of Ireland, will perhaps dispose many persons to doubt the strength and durability of houses whose walls are composed of no other materials than earth. The French author says, "The possibility of raising the walls of "houses two or even three stories high, with earth only, which "will sustain floors loaded with the heaviest weights, and of "building the largest factories in this manner, may astonish

The method: It is totally different from that of making mud walls, —and incomparably stronger.

* Extracted (by permission) from "Barber's Farm Buildings, containing designs for Cottages, Farm-houses, Lodges, Farm yards, &c. with appropriate scenery to each." London, printed for W. Harding, 36, St. James's Street.

It may be an acceptable piece of information for agriculturists to know, that Mr. Harding has taken considerable pains to render his shop a repository of all the works of the first character relating to the various branches of agriculture.

"every

"every one who has not been an eyewitness to such things." But it is hoped that a description of this manner of building will sufficiently explain the reason of its superiority.

The method, called Pisé, consists simply in compressing earth in moulds or cases.

Pisé is a very simple operation: it is merely by compressing earth in moulds or cases, that we may effect the building of houses of any size or height. This art, though at present confined almost to the Lyonese in France, was known and practised at a very early period of antiquity, as appears from a passage in Pliny's Natural History. M. Goiffin, who published a treatise on Pisé in 1772, is of opinion that the art was practised by the Romans, and then introduced into France: and Abbé Rozier, in his Journal de Physique, says, that he has discovered some traces of it in Catalonia; so that Spain, like France, has a single province in which this ancient manner of building has been preserved. The art, however, well deserves to be introduced into more general use: the cheapness of the materials, and the great saving of time and labour which it affords, must recommend it in all places and on all occasions. But the French author says that it will be found particularly useful in hilly countries where carriage is difficult and sometimes impracticable; and for farm buildings, which as they must be made of considerable extent, are usually very expensive.

It appears to be of considerable antiquity.

Is very economical.

Of the Implements necessary for building in Pisé.

Account of the tools requisite for the practice:

Besides the common tools, such as spades, trowels, baskets, watering-pots, plumb-rule, hatchet and hammer, the only implements required for building in pisé are a mould and a rammer, of which it will be necessary to give a particular description.

The following is a list of their several parts, as they are delineated in Plate V.

—they are a mould and a rammer.

Fig. 1. One side of the mould seen on the outside.
2. The other side seen within.
3. Head of the mould seen without.
4. The other face seen within.
5. Wedges.
6. A round stick, called the wall-gauge.
7. Posts set upright, seen flat-wise with its tenons.
8. The same on the edge.

Fig.

Fig. 9. Joists in which the mortices are cut, seen flat.
10. The same, with the side and bottom seen.
11. A mould put together, in which are seen all the parts above mentioned, and a small rope.
12. The rammer (or pisoir) for ramming the earth in the mould.
13. The same, seen on its side.

For the construction of the mould, take several planks, each ten feet long, of light wood, in order that the mould may be easy to handle. Deal is the best. Let them be ploughed and tongued, or jointed close, and planed on both sides of these planks, fastened together with four strong ledges or battens on each side: the mould must be made two feet nine inches in height; and two handles should be fixed on each side. See Fig. 1. and 2. The head of the mould, which serves to form the angle of the building, must be made of two pieces joined at the sides; its breadth eighteen inches, and height three feet. See Fig. 3. and 4., where it will be remarked that this part of the mould diminishes gradually to the top, in order that the wall may be made to diminish in the same degree. **Description of the mould,**

All the boards should be full an inch thick. The wedges must be an inch thick, and from eight to twelve inches long, and the gauge, Fig. 6. must be cut in length equal to the wall you intend to erect. **—and its several parts.**

The posts are to exceed the height of the mould by eighteen inches. They must therefore be about five feet high, including their tenons (which should be six inches long) and three by four inches thick.

The joists may be three feet six inches long, three inches and a half broad, and three inches thick. On the broad part must be made the two mortices (as marked Fig. 9.) ten inches and a half long, and full an inch wide, and at each end three inches and a half beyond the mortices; so that the interval between them will be fourteen inches.

For a further explanation, an elevation of the whole machine is annexed, Fig. 11.; and the following is a list of the several parts, in the same order that the workmen must follow when they erect the mould.

Eleva-

Elevation of the Mould on the Wall.

Method of fixing or using the mould, between which the earth is to be compressed;

A. A stone foundation, eighteen inches thick.

B. Joists laid across the foundation wall.

CC. The two sides of the mould, including between them three inches of the foundation wall.

DD. The two upright posts, the tenons of which fit into the mortices of the joists.

E. Wall-gauge which fixes the width of the mould at top, and which is shorter than the thickness of the wall at bottom, to regulate the diminution of the wall.

F. A small cord making several turns round the posts.

G. A stick, which being wound round, fastens the cord, and holds the posts tight together.

HH. Wedges which enter into the mortices in the joists, and keeps the posts and mould firmly fixed against the wall.

—and of taking it down.

Such is the process of erecting the mould. A contrary order must be observed in taking it to pieces. The rope must be loosened, the wedges taken out, and the posts, mould, and joists removed, to refix the whole again.

The rammer.

The instrument with which the earth is rammed into the mould, is a tool of the greatest consequence in the formation, on which the durability and perfection of the work depends. It is called a pisoir, or rammer. An idea of its construction may be formed by examining Fig. 12. and 13. better than by words. It should be made of hard wood, either ash, oak, beech, or walnut.

Method of Working.

The method of working, exemplified in an house constructed of earth,

Let us not confound pisé with the miserable way of building with clay or mud, mixed with straw, as practised through Ireland. Nothing can in reality be more different. Those wretched huts are built in the very worst manner that can be imagined; whereas pisé contains all the best principles of masonry, together with some rules peculiar to itself. Fig. 14. and 15. represent the elevation and plan of a house, the building of which will be regularly described, according to the method of pisé.

The

The foundation must be of masonry, eighteen inches thick, and may be raised to a foot or eighteen inches above the ground; which is necessary, to secure the walls from moisture or splash. Mark upon them the distance at which the joists are to be set for receiving the mould. These should be three feet each, from centre to centre. This will leave six inches at each end, which serve to lengthen the mould at the angles of the house. After having set the joists in their places, the masonry must be raised between them, six inches higher than the upper side of the joists. Raise the mould immediately on the masonry, as described, placing it over one of the angles of the wall. The head of it, which is to be placed against the angle, should have eighteen inches in breadth at the bottom, and only seventeen inches and a half at top. Thus the sides of the moulds will incline towards each other, and produce the necessary diminution. The wedges must then be driven in, the posts well fixed by cords, and the head of the mould secured by iron pins.

—on a low foundation of masonry.

The mould fixed.

A workman should be placed in each of the three divisions of the mould, the best workman at the angle. He is to direct the work of the other two; and by occasionally applying a plumb-rule, to take care that the mould does not swerve from its upright position. The labourers who prepare the earth must give it in small quantities to the workmen in the mould, who, after having spread it with their feet, begin to press it with the rammer. They must only receive so much at a time as will cover the bottom of the mould to the thickness of three or four inches. The first strokes of the rammer should be given close to the sides of the mould, but they must be afterwards applied to every other part of the surface. The men should then cross their strokes, so that the earth may be pressed in every direction. Those who stand next to one another in the mould, should regulate their strokes so as to beat at the same time under the cord; because that part cannot be got at without difficulty, and must be struck at obliquely. With this precaution, the whole will be equally compressed. The man at the angle of the wall should beat carefully against the head of the mould; and to encrease the strength of the building, it is usual to spread, every six inches high, a layer of mortar near the head. Care must be taken that no fresh earth is received

Three workmen ram at the same time, in the length of twelve feet.

Method of ramming.

into the mould till the first layer is well beaten, which may be ascertained by striking it with the rammer. The stroke should leave hardly any print on the place. They must proceed in this manner, to ram in layer after layer, till the whole mould is full. When this is done, the mould may be taken to pieces, and the earth which it contained will remain firm and upright, about nine feet in length, and two feet and a half in height. The mould may then be replaced for another length, including one inch of that which has first been completed. The regular manner of joining the different lengths may be seen in the geometrical elevation, Fig. 14. Pl. VI., where it will be observed that no joints are left in this work, as the different lengths are united, and made to press one upon the other. In the second length, and most of the following, the head of the mould is useless; it is only made use of at the angles.

When the ramming is complete, the earth is quite firm.

Partition walls.

When the workmen have gone round the building, they must begin upon the partition-walls, where the head of the mould must be used to form the door-jambs.

Subsequent courses: They are so firm, that no time is lost by waiting for them to become indurated. Three courses may be raised in a day.

The first course being thus completed, we proceed to the second; and in each successive course we must proceed in a direction contrary to that of the preceding. It may easily be conceived, that with this precaution the joints of the several courses will be inclined in opposite directions, which will contribute very much to the firmness of the work. There is no reason to fear overcharging the first course with the second, though but just laid; for three courses may be laid, without danger, in one day. Mark the grooves for receiving the joists in the first course, at the distance of three feet from each other, but not immediately over the former grooves, but over the middle points between them. See Fig. 14. These grooves must be cut for the joists, and the second course completed as the former, except that it must proceed in a contrary direction, and the head of the mould and wall-gauge must be diminished, in order that the same inclination of the sides to each other that was given to the first course may be preserved in this second.

Connection of the party-wall with the side-walls.

It must, however, be remarked, that this second course is not to be continued, without interruption, like the first, as it is necessary that the partition-wall, should join or bind into the exterior wall, or rather that all walls in the building, whether outside

outside or partition-walls, which meet at an angle, should cross each other at every course. In pursuance therefore of this rule, when the work has been advanced from A to C. Fig. 15. leave the exterior wall, and turn the mould to the partition. When the work has been carried on along the partition-wall to its termination, bring back the mould to the part which remained unfinished in the exterior wall marked C; and after having filled up that space, carry on the mould beyond the partition-wall and complete the course.

This description of the two first courses is equally applicable to all the other, and will probably enable any person to build a house with no other material than earth, of any height and extent he pleases.

The gable ends.

With respect to the gables, they cannot be crossed, as they are detached from each other; but as their height is so inconsiderable, and they are, beside, connected together by the roof, this is not of any consequence.

It has been observed, that each course will be two feet and a half high, if the mould be two feet nine inches; for the mould must include three inches of the course beneath. For this reason the grooves are made six inches deep, though the joists are only three inches in thickness.

Houses built in this manner are very durable.

Such is the method of building which has been practised by the Lyonese for many centuries. Houses thus built are strong, healthy, and very cheap. They will last a great length of time: for the French author says he had pulled down some of them, which, from the title deeds in the hands of the proprietors, appeared to be above a century old. The rich traders of Lyons have no other way of building their country-houses. An outside covering of painting in fresco, which is attended with very little expence, conceals the nature of the building, and is a handsome ornament to the house. That method of painting has more freshness and brilliancy than any other, because water does not impair the colours. No size, oil, or expence is required; manual labour is almost all it costs, with a little red or yellow ochre, or other mineral colours.*

The outside coating.

Plaster and pebbles recommended for the outside.

* I would recommend the outside to be plastered and pebbled handsomely, or rough-cast, as painting in fresco is not understood in this country; and the other method would have a greater neatness. The interior to be plastered as common.

Strangers who have sailed upon the Rhone probably never suspected that those beautiful houses which they saw rising on the hills around them, were built of nothing but earth.

Many advantages to be derived from this manner of building.

There is every reason for introducing this method of building into all parts of Ireland: whether we consider the honour of the nation as concerned in the neatness of its villages, and the consequent health of the inhabitants, to which it will greatly contribute (as such houses are never liable to the extremes of heat or cold), or whether we regard the project on an economical or an expeditious scale, by saving both time and labour in building, and rendering the houses thus built almost immediately inhabitable after they are finished; for which latter purpose, the holes made for the joists should not be closed up directly, but left for the air to dry the walls more speedily.

Method of forming the openings for Doors and Windows.

How the door and window-ways are to be made.

The openings for the doors and windows must be left at the time of building the walls. This may be done by placing within the mould either two or one of the heads, wherever the wall is to terminate, and the opening commence. They should be made sloping a little, in order to leave room for the frames and sashes.

On the description of compressed Earth.

Experiments which shew the time of drying.

After beating a small portion of earth, it was found to weigh thirty-nine pounds and a half. Fifteen days afterwards it had lost four pounds and a quarter. In the space of another fifteen days, it lost but one pound; and in fifteen days after that, it diminished only half a pound. In the space of forty-five days the moisture was completely evaporated, and it's weight was diminished about one-eighth. This small portion of moisture cannot at all affect the solidity and consistency of the earth so treated. This experiment is also sufficient to shew the difference between this kind of building and that vulgar kind called in Ireland "Mud-walling." The latter cannot be executed without adding a great deal of water, which occupies a considerable space in the mud, and leaves, on evaporating an infinite number of pores or little cavities; and thus the walls become weak and brittle, and incapable of supporting much weight, as beaten earth or pisé can sustain.

In forty-five days the rammed earth is dry, and loses only one-eighth.

In

In a single day three courses may be laid, one over the other, so that a wall of eight or nine feet, or one story high, may be raised in one day. Experience has proved, that as soon as raised to a proper height for flooring, the heaviest beams and rafters may, without danger, be placed on the walls, and that the thickest timber of a roof may be laid on the gables the instant they are completed.

One story may be built in a day.

On Earth proper for Pisé.

First,—All earths in general are fit for that use, when they have not the lightness of poor lands, nor the stiffness of clay.

Buildings may be made of any earth not too light or too stiff.

Secondly,—All earths fit for vegetation.

Thirdly,—Brick earths.

Fourthly,—Strong earths, with a mixture of small gravel, which for that reason cannot serve for making either bricks, tiles, or pottery, but make the best pisé.

The following appearances indicate that the earth in which they are found is fit for building. When a pick-axe, spade, or plough brings up large lumps of earth at a time; when arable lands lie in clods or lumps; when field-mice have made themselves subterraneous passages in the earth; all these are favorable signs. When the roads of a village, having been worn away by the water continually running over them, are lower than the other lands, and the sides of those roads support themselves almost upright, it is a sure mark that the pisé may be executed in that village.

How to chuse the kind of earth,

Proper earth is found at the bottom of the slopes of low lands that are cultivated, because every year the rain brings down the good earth. It is also found on the banks of rivers. If the earth to be had is not quite fit, it may be mixed to make it so. Strong earths must be tempered with light; those in which clay predominates, with others composed more of chalk and sand; and those of a rich substance with others of a poor nature. It will not be amiss to mix with the earth some small pebbles, gravel, rubbish of mortar, or any small mineral substances; but none of the animal or vegetable kind must be admitted. Such hard substances bind the earth firmly between them; so that a well worked earth, in which there is a mixture of gravel, becomes so hard at the end of two years that a chisel must be used to break it, as if it were freestone.

—and to improve it if required.

The old walls are as hard as a stone.

Experi-

Experiments to ascertain the qualities of any Earth.

Simple experiments by which the goodness of any earth may be tried.

Take a small wooden tub without a bottom, dig a hole in the ground, fix a piece of stone or flag at the bottom, place the tub upon the stone, fill round it the earth you dug out and ram it well to prevent the tub from bursting; then ram into the tub the earth you intend to try, a little at a time, as described. When the tub is full, loosen the earth around it, and take it out with the compressed earth in it, then turn the tub upside down, and the pisé will come out. If not immediately, let it stand to dry, and it will fall out of itself. Leave the lump to stand some time; and if it do not crack, but increase in hardness, it is fit for building. This experiment may be made in a small box, in the hand. Every person in walking over his grounds may make little balls of earth, and press them, as firmly as he can, between his hands. If he brings them home, and puts marks on them, he will by that means know the quality of every piece of land, and also be a judge of the mixture it will be necessary to make.

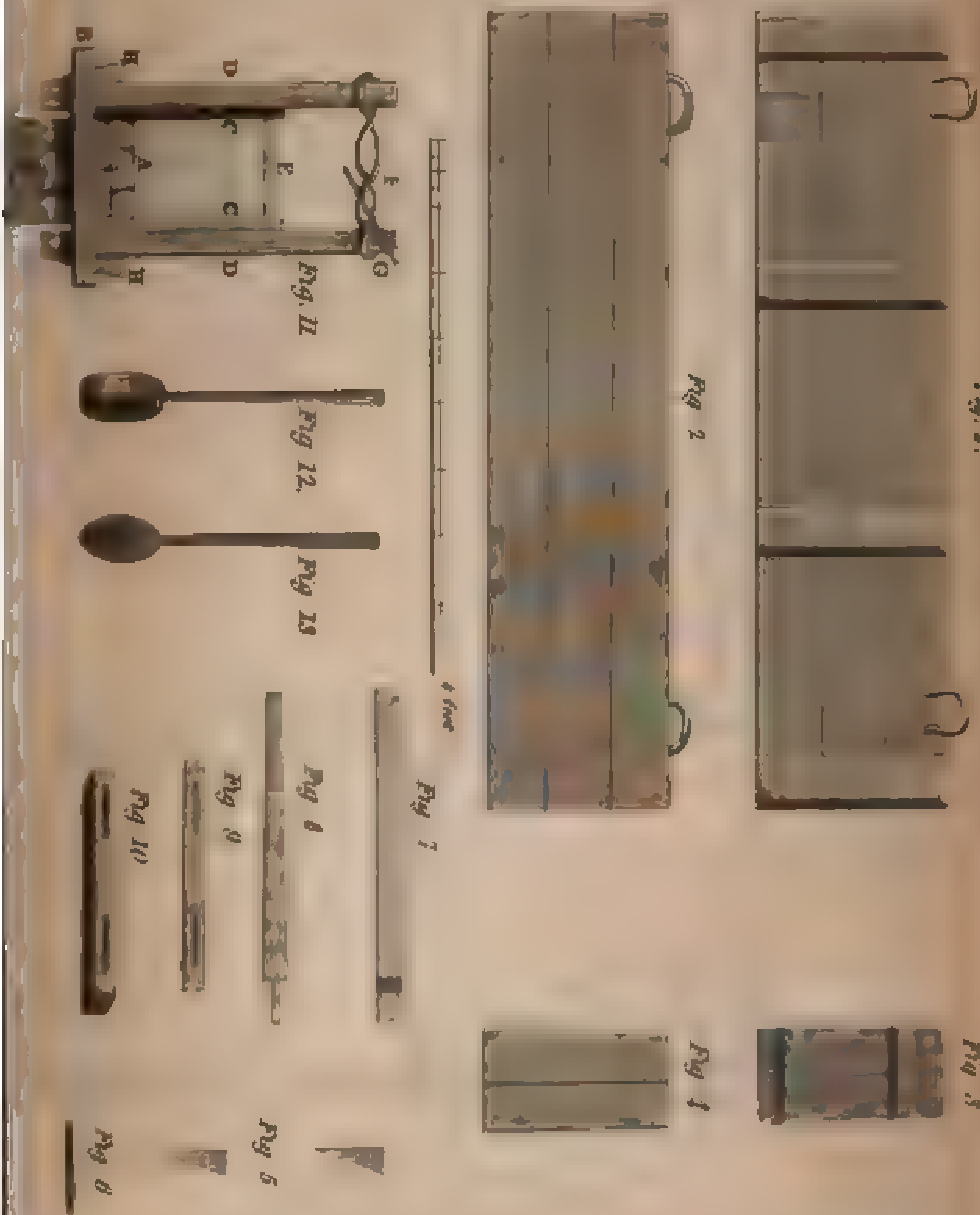
Simplicity of this art.

All the operations of this art are simple and easy. There is nothing to be done but to dig up the earth, break the clods with a shovel, and to lay it in a heap, where the large lumps are to be drawn away by a rake, in which there may be intervals of an inch and a quarter between the teeth, that the stones and pebbles, of the size of a walnut, may remain.

Binders or bond-pieces of wood.

It is necessary to lay in binders or bonds when the first course is laid and the mould fixed for the succeeding one. Lay in at the bottom of it a board, rough from the saw, about five or six feet long, eight or nine inches broad, and about an inch thick. There will be some inches of earth on each side, by the wall being so much thicker. This will entirely conceal the board in the wall and prevent its rotting. In the next course, or in the middle of the mould, there may be short ends of boards, laid across so that they shall not come through, but be concealed also in the wall. These may be at two or three feet intervals, and crossing each other at the angles. This will serve much to equalize the pressure. When the wall is completed to the height of a story, boards of three or four feet in length should be placed on the pisé, in those places where the

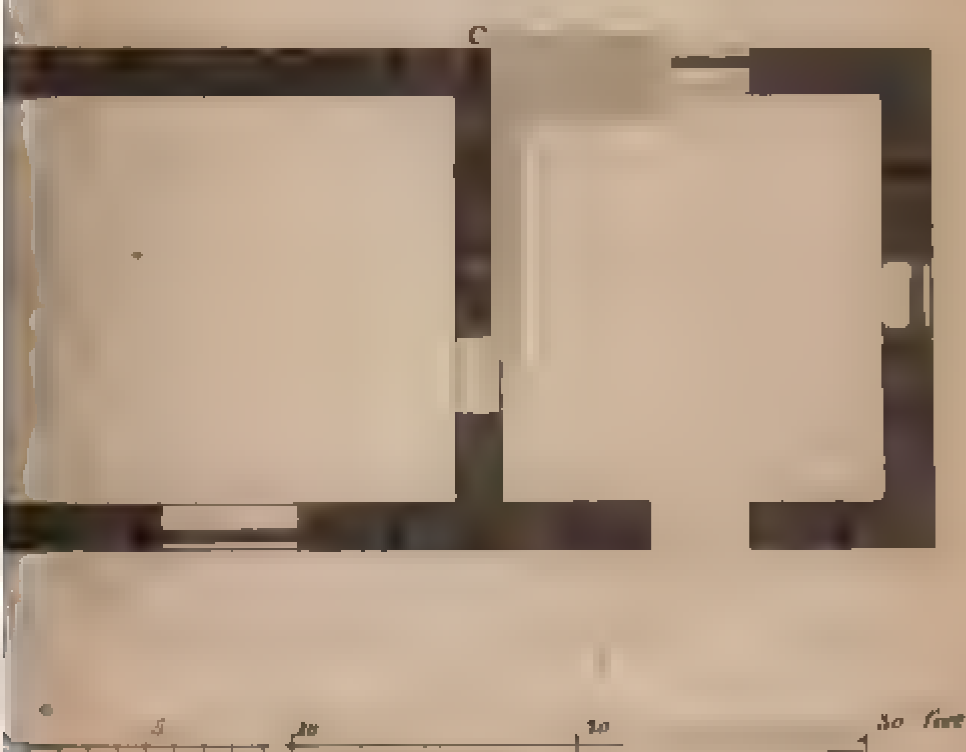
Cheap and durable Buildings of Earth



Cheap and durable Buildings of Earth

Fig. 14.

Fig. 15



the beams and rafters are to be fixed, which may be laid on as soon as the mould no longer occupies that place.

With respect to walls for very large enclosures, there may be two moulds and two sets of workmen to expedite.

Beside the advantage of strength and cheapness, this method of building possesses that of speed. To give an idea of the time that is necessary to build an house or an enclosure, a mason used to the work, can, with the help of his labourer, build in one day, four square yards, or thirty-six square feet of pisé; therefore six men, which is the necessary number to work the mould, will build in one day, twelve square yards,* or one hundred and eight square feet; or in one week, six hundred and forty-eight feet. By this it is easy to calculate the time necessary for building a house or wall of any dimensions. These facts, which have been proved by numberless instances, afford a criterion by which every one may determine the time that his house or wall will take in building, having first ascertained the number of feet it will contain. Thus if he wishes to have a wall built five hundred and forty feet long, and six feet high, with one mould and six men, it will finish eighteen feet running measure, or one hundred and thirty-eight square feet in a day. It will be completed in thirty days, comprehending in the whole, three thousand two hundred and forty square feet.† As to the expence, it being only the labour, except the mould and the materials (scarcely any thing), the five common labourers at 1s. per day, and the conducting builder at 2s. makes 7s. per day. It will be finished for £10. 10s.

Facts which shew how expeditiously this kind of building is performed.

Six men will build in a day a wall eighteen feet long and six feet high.

Three thousand two hundred and forty square feet for about ten guineas.

The plastering and rough-casting or dashing, should not be done for five or six months after the walls are built; and they should always be built between the months of March and October inclusive. To prepare the walls for plastering, indent them closely with the point of a hammer or hatchet.

Plastering, &c.

* This is the calculation as translated from M. Cointeraux; which to render more familiar to building in this country, is three and a half perches.

† One hundred and four perches.

VI.

VI.

Considerations on the Oxidation of Metals in general, and on that of Iron in particular. By M. THENARD.*

Degrees of oxidation in metals constant, determinable by the salts they form, and numerous.

THE object of the author in this memoir is to show, that the degrees of oxidation in metals are constant, and determinable particularly by the nature and properties of the salts they form. These different degrees of oxidation are frequently pretty numerous, as in antimony, iron, and manganese.

Colours of metallic salts do not indicate those of the oxides they contain. Iron has no yellow oxide, but green, red, and white.

He adverts to the principle but lately known to chemists, that the colours of salts do not always indicate those of the metallic oxides they include; and this principle he applies to the study of the different oxides and sulphates of iron. Though he does not admit the existence of the yellow oxide of iron, which has been adopted from the observation of some yellow salts of this metal; he distinguishes in it three degrees of oxidation, namely, the green oxide, the red oxide, and a third made known by M. Thenard himself, which is white, and less oxided than either of the other. This is the first obtained, when we decompose a fresh solution of sulphate of iron by means of an alkali. On this occasion we see a white oxide thrown down, which quickly turns green, and even red, by absorbing oxigen.

Each oxide produces two sulphates, making six in all.

This white oxide is capable of producing two different sulphates by combining with two different proportions of sulphuric acid: and as these two degrees of saturation are equally capable of taking place with each of the other oxides, we have six sulphates of iron, observes M. Thenard, very distinct from each other, and of importance to be known, on account of the various and delicate uses to which this salt is applied in the arts. The following are the names and characters of these six sulphates:

Acidulous sulphate of white iron.

1. Acidulous sulphate of white iron. This is the white oxide just mentioned, combined with a little sulphuric acid in excess. Its colour is a deep bottle-green. It is the sort most common in the shops.

* *Bulletin des Sciences*, August, 1805, p. 223.

2. Acid

2. Acid sulphate of white iron. This is of an emerald green. It contains a much greater excess of acid, and is rejected in almost all the arts in which sulphate of iron is employed. The acidulous may be converted into the acid sulphate by the addition of a little sulphuric acid; and the acid sulphate may be converted into the acidulous by heating it with iron filings. Acid sulphate of white iron.

Alkalis precipitate both these sulphates white: substances that easily part with their oxigen, as oxigenated muriatic acid, air, water, &c. decompose them, and precipitate a green or red oxide. Common characters.

3. Acidulous sulphate of green iron. This is made by combining sulphuric acid with green oxide of iron. This salt does not crystallize; and is red, notwithstanding the green colour of its oxide. Acidulous sulphate of green iron.

4. Acid sulphate of green iron. This is nearly colourless. It is obtained by the addition of a little sulphuric acid to the preceding sulphate; and crystallizes, though with difficulty. The crystals approach the emerald green of the acid sulphate of white iron; are neither efflorescent nor deliquescent; and absorb the oxigen of the atmosphere but slowly. Acid sulphate of green iron.

Both these sulphates are precipitated green by alkalis: the iron they contain is converted into the state of white oxide by the addition of iron filings, and to that of red oxide by oxigenated muriatic acid. Common characters.

5. Acidulous sulphate of red iron. M. Thenard calls this also neutral sulphate of highly oxided iron. It is yellow, completely insoluble, and consequently incapable of being crystallized. It precipitates in the form of a yellow powder from solutions of the acidulous sulphates of white or green iron. This salt has been taken for a yellow oxide of iron, different from the red and the green. Acidulous sulphate of red iron, or neutral sulphate of highly oxided iron. Mistaken for a yellow oxide.

6. Acid sulphate of red iron. This is obtained by dissolving red oxide of iron in sulphuric acid diluted with water. It contains a greater excess of acid than the other two acid sulphates; is almost colourless, but assumes a pretty deep red if its excess of acid be saturated by potash; and does not crystallize. Acid sulphate of red iron.

Such are the principal properties of the six sulphates of iron distinguished by M. Thenard. Most of the other acids act nearly in the same manner on iron, and the three degrees of oxida- Most of the other acids act nearly in the same manner on iron.

oxidation of iron mentioned above are equally observable in the gallates and prussiates of this metal.

Gallates of iron.

The gallate of white iron, which may be obtained by decomposing the deep green sulphate of iron, is itself colourless: the gallate of green iron is blue: the gallate of red iron is black. They may also be obtained by decomposing the acidulous or acid sulphate of green or red iron by means of the gallic acid.

Prussiates still more numerous, owing to the presence of prussiate of potash.

The combinations of iron with the prussic acid exhibit much more numerous varieties, which depend not only on the different oxides of iron already mentioned, but on the greater or smaller quantity of acid, and on the presence of the prussiate of potash that may remain combined with the prussiate of iron.

Prussiate of white iron.

The prussiate of white iron is that in which the iron is in the state of white oxide, and the oxide is in excess, owing to the excess of alkali contained in the prussiate of potash.

Green prussiate of iron.

The prussiate of green iron is the same prussiate as the preceding without excess of oxide.* They both contain too, as M. Berthollet has shown, prussiate of potash, which is strongly adherent to them.

Prussiates of green and red iron.

From the ferruginous salts with bases of green and red oxides of iron, two prussiates each are equally obtainable, one with, and the other without excess of oxide. The prussiates obtained with the green oxide are blueish; those with the red oxide are of a fine blue. The six prussiates above mentioned, are capable of exhibiting farther varieties in consequence of the prussiate of potash they may contain.

Improvements in manufacturing prussian-blue.

M. Thenard concludes his memoir with proposing some means of improving the fabrication of prussiate of iron, or prussian-blue. These consist, 1st, in turning to account the great quantity of ammonia formed by the calcination: 2d, in employing the most advantageous proportion of potash, which appears to consist in equal parts of blood and alkali: 3d, in adding iron during the evaporation, which facilitates the formation of the prussiate of potash: 4th, in causing the prussiate of potash to crystallize.

* Either M. Thenard here departs from the principles of his nomenclature, and means to call this the *green prussiate of iron*; or the abridger of his memoir has fallen into some mistake. T.

VII.

VII.

Abstract of a Memoir on the Dyes obtained from different Species of Clubmoss, Lycopodium. *Translated from the Swedish of* Dr. J. F. WESTRING, *of Nordkoeping, Physician to the King of Sweden, by* EUGENE COQUEBERT.*

THE numerous experiments Dr. Westring has made on the colouring properties of lichens, and the interesting discoveries that have resulted from his inquiries, are well known. Attempting to fix one of these dyes of a very fugitive nature, he bethought himself of employing as a mordant the species of moss known to botanists by the name of *lycopodium complanatum*;† he did not attain the object he had in view, but a result which he did not expect: for he discovered, that a very fine blue, considerably permanent, might be struck on wool or silk, by boiling it first with this species of lycopodium, and then steeping it in a slight infusion of brasil. **Lycopodium complanatum, gives a fine blue to wool and silk, with the addition of a little brasil.**

The wool treated in this manner was at least of as fine a blue as if it had been dyed with woad, or in what dyers commonly call the vat; and was so fixed, that on rubbing a piece of white linen with it, it did not stain it as many blue cloths do; that being rinsed in cold water, it did not impart to it the slightest blue tinge, and that it resisted the action of boiling soapsuds. **Equal to woad, and fixed.**

The only inconvenience of this dye is its being injured by all acids, even common vinegar, which redden it more or less: but it is easy to remove the spots thus produced by means of a weak alkali, which will restore the colour, and occasion no change itself. **Acids spot it, but the spots removed by alkali.**

The following manner of employing the lycopodium complanatum Dr. Westring has found to be the most simple and convenient:

Take a quantity of this moss, dried and chopped, nearly double the weight of the cloth to be died. Put them into a proper vessel, a stratum of the moss between every fold of the **Method of using it.**

* *Bulletin des Sciences,* August, 1805, p. 224

† The country people in Sweden sometimes use a species of lycopodium as a mordant. T.

cloth,

cloth, and pour on a sufficient quantity of water, which must be at least sufficient to cover the whole well. Boil them together for two or three hours, adding more water from time to time, to supply the place of what is wasted by evaporation. Take out the cloth thus prepared, wring it, and hang it up to dry without rinsing.

Afterward, when you would dye the cloth thus prepared, begin by rinsing it carefully in cold water; then put it into a well tinned copper, with cold river or spring water, and a small quantity of brasil; and boil it gently for half an hour or an hour, according as you would have the tint deeper or lighter. If too much brasil be used, the dye will have a violet hue.

After having taken it from the fire, rinse the cloth immediately in cold water. It is not necessary that the bath should have been made to boil; it is sufficient to keep it for a couple of hours at a heat of 60° or 70° of the centesimal thermometer (140° or 158° Farht).

None of the common mordants must be used.

The brasil may be mixed at once with a strong decoction of lycopodium: but in either case, care must be taken that none of the common mordants, either saline or astringent, are used, for they would alter the colour.

Recommended for the army clothing.

Dr. Westring conceives, that this process may be substituted for the common mode of dyeing the cloth used for troops, with a saving of expence.

The lycopodium is very common in the Swedish woods, so that it would furnish an article of exportation, beside an abundant supply for home consumption.

Lycopodium clavatum, answers the same purpose.

Dr. Westring has extended his experiments to the various species of this genus. He has found that the *lycopodium clavatum*, which is more common than the *complanatum*, may be used in the same manner with equal advantage. The blue it yields, when it is perfectly dry, is even deeper, which may render it preferable. Hitherto this moss has been applied to no use but that of making mats, except that the farina of the stamens has been sometimes employed medicinally.

Lycopodium annotinum yields grays. A good mordant.

The *lycopodium annotinum* does not yield a blue with brasil-wood, but several shades of gray, in which acids and alkalis have the effect mentioned above. This species affords a mean of easily fixing in cloth several colours, that have hitherto been found difficult to render permanent.

The

The *lycopodium selaginoides* is less common, and, like the *lycopodium selago*, yields no blue, but a fine gray, the shades of which may be varied, and which inclines to blue or violet. Lycopodium selaginoides and selago give grays.

Analogy led to the supposition, that the *lycopodium alpinum*, which covers the high mountains of Lapland like a carpet, and greatly resembles the *complanatum*, might likewise be used for dyeing blue. This Dr. Westring found to be the fact; and it appears, that the colour it affords is less injured by acids. Lycopodium alpinum gives a blue, less injured by acids.

Thus all the species of this genus will be of use in dyeing. Dr. Westring presumes, that they may be employed not only with brasil-wood, but with several other dyes, as substitutes for galls, and the salts used as mordants. He imagines too, that the barks of some of the trees indigenous to Sweden, might be found to answer as well as brasil with the lycopodium. The fresh bark of the branches of the ash yields with the *lycopodium complanatum* a changeable colour, which inclines to brown and blue, as Dr. Lindenstolpe announced as early as 1720, in a treatise on dyeing; but when this bark is green, it gives only a fine yellow, of no use as a dye. All the species useful as dyes, or as mordants. Ash bark with lycopodium complanatum.

M. Lasteyrie has received from Dr. Westring a pattern of wool dyed blue by means of the *lycopodium complanatum;* and this pattern has been shown to the Philomathic Society. Among these, the Dr. sent with his original memoir to the Patriotic Society of Sweden, there was some silk, which, treated in the manner above described, had taken a fine blue colour inclining to red, which the dyers have called *œil de roi*. If the quantity of brasil used be greater, the silk acquires a *puce* colour. Patterns. Silk, œil de roi, and puce.

Subjoined are some more facts taken from Dr. Westring's memoir, and his letter to M. Lasteyrie.

The *lichen parellus* is the only one of the lichens in which Dr. W. has perceived the property of affording a blue dye. To obtain it, all that is necessary, is to infuse this lichen in river water, without any mixture, at a temperature of 40° or 50° (104° or 112° Farht.). In three days, half an ounce of this lichen will have imparted a sufficient colour to a quart of water, and is capable of colouring three or four quarts in succession. But Dr. Westring could not fix this dye by means of any of the known mordants, or of the *lycopodium*. It even disappears as soon as the water is made to boil. Lichen parellus gives a blue, but not permanent.

The

Plumtree bark a fine carmelite.

The bark of the fresh branches of the plumbtree, taken off after the first frosts, has yielded a good dye of a fine carmelite colour. That of the Italian poplar, *populus dilutata*, whether fresh or dry, gives a permanent yellow both to wool and silk, and deserves to be employed in the great.

Populus dilatata a yellow.

This owing to the lycopodium,

This advantage is certainly owing to the preparation with lycopodium; for M. Dambourzey could obtain only a false colour of no permanence from the same bark used fresh. Yet he employed as a mordant the nitro-muriatic solution of tin, or composition, as it is called by the dyers; an addition which Dr. Westring has found to heighten the beauty of the dye.

and composition enhances the beauty.

Lycopodium with different lichens.

Wool prepared with lycopodium receives from the *lichen Westringii* a good dye of a fine orange colour, much superior to that given by annotta. The same colour is obtainable with the *lichen cinereus;* and a fine bright yellow with the *lichen chlorinus. Achart.* If the wool thus dyed be afterwards dyed with brasil, that which was prepared with the *lichen Westringii* becomes a very deep blue-black; and that with the *lichen chlorinus* of a fine green-black, or raven's-wing. With the *lichen vulpinus* the colour is of a fine lemon-yellow, which is changed by the addition of brasil to a blueish-green.

Cotton dyed with mahogany.

Dr. Westring says in his letter to Mr. Lasteyrie, that he has prepared with the *suretenia mahagoni*, mahogany, an aurora dye for cotton. He adds, that, having made some experiments with lichens sent by M. Lasteyrie from Auvergne, he found, that the *lichen parellus* contained no colouring matter, and that the red colour commonly ascribed to it was afforded by other lichens.

Lichen parellus no colour.

Pine bark an excellent tonic.

Dr. Westring has found, that the bark of the pine tree (Scotch fir?) is an excellent tonic: that it may be used with advantage in several convulsive diseases, even epilepsy; and that it may be substituted for the cinchona.

—bread made from it,

This bark, as is well known, is nutritious, and the inhabitants of the northern provinces of Sweden are sometimes obliged to make bread of it.

and from bog-moss, which contains much sugar

Bread too has been made in Iceland with the *sphagnum palustre*, which is white, and said to be little inferior in taste to common bread. A surgeon of Ulesborg has found a considerable quantity of saccharine matter in this species of moss.

VIII.

VIII.

On the supposed Discoveries of LAVOISIER. *In a Letter from* E. D.

To Mr. NICHOLSON.

SIR,

Edin. June 13*th*, 1806.

IN addition to my remarks on M. Lavoisier's claims to the discovery of *oxidation*, inserted in your last number, I now send you some other facts on that subject, and on acidification, together with such observations, as will, I hope, tend to ascertain pretty correctly the justice and true extent of his pretensions.

Additional remarks on M. Lavoisier's pretensions to chemical discoveries.

M. Lavoisier himself tells us, that in the year 1630, J. Rey, a physician at Bugue, in Perigord, combated the opinion of Cardan and others, concerning the cause of the augmentation of weight of metallic oxides: he shewed that it did not proceed from the condensation of the soot in the furnace, nor from the vessel, nor from any emantion of the charcoal, nor from the humidity of the air: but by *conclusive reasoning* he maintained, that the increase of weight arose from the air of the vessel, which attaches itself to the minutest molecules of the calx, in the same manner as water does to sand, adhering to the smallest grains, and rendering them heavy. These opinions of Rey were afterwards quite forgotten: and Boyle and Lemery attributed the augmentation of weight to the fixation of fire.*

The doctrine of John Rey, that metals acquire weight in oxidation from the air.

Mayow, in his tract de Sal-Nitro et Sp. Nit. Aereo, published in 1674, has these words, in speaking of the calcination of antimony: "Neque illud prætereundum est, quod antimonium, radiis solaribus calcinatum, haud parum in pondere augetur, uti experientiâ compertum est: quippe vix concipi potest, unde augmentum illud antimonii, nisi a particulis nitro-aereis, igneisque ei inter calcinandum infixis, procedat. Plane ut antimonii fixatio, non tam a sulphuris ejus externi absumptione, quam a particulis nitro-aereis, quibus flamma nitri abundat, ei infixis provenire videatur."†

Mayow ascribed this augmentation to nitro-aereal particles from the air.

In

* Phil. Journ. Jan. 1806, p. 82.

† Tractat. quinque, p. 28–9. (In English) Neither must it be overlooked, that antimony, calcined by the solar rays, is not a little increased

Dr. Hales not only proved the absorbed air to be the cause of increased weight of calcined metals; but his doctrine was noticed by others.

In my former communication, I stated that Dr. Hales, from the most decisive experiments, shewed that the increase of weight in calcination arose from the *attraction* and *condensation* of air: and that his conclusion obtained the notice of philosophers, is sufficiently evident from the following passage taken from the treatise of the very learned and ingenious Bishop Berkeley: " Fire," he says, " collected " in the focus of a glass operates in vacuo, and therefore is " thought not to need air to support it. Calx of lead hath " gone off with an explosion in vacuo, which Niewentyt and " others take for a proof that fire can burn without air. But " Mr. Hales attributes this effect to air enclosed in the red-" lead, and perhaps too in the receiver, which cannot be per-" fectly exhausted. When common lead is put into the fire, " in order to make red-lead, a greater weight of this comes out " than was put in of common lead: therefore the red-lead " should seem impregnated with fire. Mr. Hales thinks it is " with air." *

Bayen's inference that metals are oxided by air.

In the same year, viz, 1774, in which M. Lavoisier published his experiments on oxidation, M. Bayen delivered the following opinion: " Les expériences que j'ai faites me force de " conclure que dans la chaux mercuriale dont je parle, le " mercure doit son *état calcaire*, non à la perte du phlogistique " qu'il n' a pas essuyée, *mais à sa combination intime avec le* " *fluide élastique*, dont le poids ajouté a celui du mercure est la " seconde cause de l'augmentation de pesanteur qu'on observe " dans les précipités que j'ai soumis à l'examen." It was in consequence of hearing Bayen's paper read, says Dr. Thomson, that Lavoisier was induced to turn his attention to the subject."†

It

increased in weight, as is found by experiment: for it is scarcely possible to be conceived, whence that increase of the antimony can proceed, unless from the nitro-aereal and fiery particles fixed in it during calcination. It is plainly seen, that the fixation of the antimony arises not so much from the separation of its sulphur, as from the nitro-aereal particles with which the flame of nitre abounds, and which become fixed in it (viz. the antimony).

* Siris, 2d. edit. an. 1774, p. 91.

† Jour. de Phys. 1774. p. 295. The first memoir of Bayen, published in the Journal de Physique, for Feb. 1774, is intitled *Sur le*

Phlogis-

It is manifest, therefore, from the foregoing detail of facts, that Rey first rightly attributed the weight acquired in the oxidation of metals to the attraction of air: that Mayow next sup- The discovery was first made by Rey, and afterwards by Mayow, Hales, and Bayen.

Phlogistique, and contains a very elaborate examination of the facts relating to sulphur and the metals, upon which that doctrine was established. His second, *On some Mercurial Precipitates*, was published in April following. The precipitate alluded to in the text, was prepared with nitrous acid. In his third memoir, published in the same work, for Feb. 1775, among other experiments, he states the reduction of precipitate per se (or mercury oxided in the air), and says that the weight of the air he obtained from it was very nearly the same as that lost by the reduction. He, therefore, *doing justice by refering to John Rey*, ascribes the calcination to that air having been absorbed. In the month preceding (viz. Jan. 1775) M. Bayen, with high commendation, gives an abstract, with the full table of contents, of the work of John Rey, who was directed to the fact of the increased weight of the oxide of tin by *Brun*, apothecary at Bergerac.

M. Lavoisier's memoir, "On the nature of the principle which "combines with metals during their calcination, and which aug-"ments their weight," was read at the Royal Academy on the 26th April, 1775, and was published in Rozier's Journal for May, in the same year. In a note, the author says that the first experiments relative to that memoir were made above a year earlier, and that those upon mercury precipitated per se were first tried by the burning-glass, in November 1774, and afterwards made with all the requisite precautions at the end of Feb. 1775. In the experiment here related, the precipitate was reduced, by fire, in a closed glass vessel; and the properties of the air disengaged, are detailed with perspicuity and conciseness. He considers it as more fit for combustion, and more respirable than common air, of which he takes it to be a part.

It seems proper to remark in this place, that Dr. Priestley expelled air from precipitate per se by solar heat, Aug. 1, 1774, and was extremely surprized at the vigorous combustion it produced: that in October following, being at Paris, he often mentioned his surprize at this kind of air to M. Lavoisier, M. Le Roy, and other philosophers in that city: and that this month immediately preceded the very November in which M. Lavoisier informs us that the same experiments were *d'abord tentées au verre ardent* by himself, without making the least mention of Priestley. See Priestley on Air, in 3 volumes, vol. ii. p. 109, and Rozier's Journal before cited, v. 429.—W. N.

 ported

ported the same opinion, and added, that it arose from a peculiar part (the nitro-aerial) of that air: that Hales by experiment shewed the same condensation of air to take place, which Bayen* also by experiment afterwards confirmed. Lastly, it was proved by Hales, that the air, previously condensed in the process of oxidation, was liberated when the oxided metal was again submitted to heat: and Lavoisier afterwards observed, that "on operating the reduction of litharge in close vessels, "with *Hales' apparatus*, there was at the moment of the passage of the calx into the metallic state a disengagement of "air in considerable quantity, at least 1000 times greater in "volume than that of the air employed."† No one, however, but Mayow as yet supposed that the oxidation was effected by a particular part of the air only; nor was it till after the brilliant discovery of oxigen gas by Dr. Priestley, that M. Lavoisier, in repeating his experiments, found that it truly depended on the combination of that gas, and the weight acquired by the metal corresponded to that which the air had lost. Hence then the steps leading to the *theory of oxidation* have, like those in most other physical discoveries, been slow and successive; and M. Lavoisier did not advance one step beyond his predecessors, until he was made acquainted with what they were ignorant of,—viz. the true composition of atmospheric air.

—and it was not till after the discovery of oxigen by Priestley, that Lavoisier followed Mayow in ascribing oxidation to it.

Facts observed and experiments made, are of equal authority. John Rey commanded both, and drew sagacious conclusions from them.

That M. Lavoisier should deny his countryman Rey the merit of discovery because he attained to his conclusion by the force of reasoning, independent of experiment, is not a little singular; for as you, Sir, justly remark, "between the observation of well established facts, and the making of direct experiments, there seems to be no essential difference."‡ If, indeed, experiment alone could entitle any one to the claim of discovery, and it were the only mode by which truth could be established, what would become of the sciences of the natural historian and astronomer, for they *observe* only, but do not *experiment*; and yet the science of astronomy has attained to greater certainty and perfection than any other can boast. Experiment does not supply the place, much less supersede

* System of Chemistry. vol. i. p. 83, 1st edit.

† Phil. Jour. loc. cit.

‡ Tractat. quinq. p. 2—4.

obser-

observation; it only aids its deficiencies, corrects its errors, or hastens its results: and it is surely of but little importance, whether the experiment be conducted by the spontaneous movements of nature, or by putting her to the torture, as Bacon says, by the efforts of human ingenuity, provided the observation of its phenomena be equally exact and the conclusion be with equal accuracy obtained. But in truth, Rey did make experiments; for what was the burning of a metal in a vessel but making an experiment; and what was the conclusion he drew, contrary to the opinion of preceding authors, but the result of a sagacious observation of the phenomena which that experiment exhibited? He went half way in the discovery of oxidation; and Dr. Hales completed it, by shewing that the condensed air was again liberated by heat. M. Lavoisier's claims go no farther than having ascertained the facts with greater accuracy, and rendered them of more extensive application.

The theory of acidification is stated with considerable perspicuity by Mayow and the earlier chemists.

With regard to the *theory of acidification*, the claims of M. Lavoisier are not much better founded; for the following circumstances will shew that others divide with him that honour. Mayow long ago proved that nitre consisted of an alkaline salt derived from the earth, and of an acid spirit, and that the contact of air with the soil was essential to its production.* At first he considered the acid spirit to be derived wholly from the air, but afterwards from its great density, held that only its more active part was obtained from that source.† Boyle having proved that something essential to combustion existed in the air, Mayow called this something the igneous particles of the air, and contended that these same particles existed in nitre; for that in *vacuo* sulphur would not burn, but when mixed with nitre, it would burn either in vacuo or under water.‡ He afterwards concludes that the aerial part of nitre is nothing else but these igneo-aereal particles, and that they reside not in the alkaline base, but in the acid spirit of the nitre, to which the caustic nature of that spirit is owing.§

* Tractat. quinq. p. 2—4.

† Ibid. p. 11.

‡ Ibid. p. 13.

§ Ibid. p. 19.

Dr.

Other facts concerning acidification.

Dr. Hales remarked that acid sulphureous fuel attracts and *condenses* air, from which it may be inferred that he had actually detected an acid in the combustion of sulphur; but the acid principle he seems to have considered as residing in the sulphur and not in the air. I have not by me the late Professor Robison's edition of Dr. Black's Lectures, but I think I remember to have there seen, that Dr. Rutherford, Professor of Botany in this University, was led, from experiment, to form the same conclusion concerning the formation of sulphuric acid, as M. Lavoisier afterwards maintained; and that nothing but the rooted prejudice of Mr. Robison and others prevented Dr. Rutherford from anticipating M. Lavoisier in this theory of acidification.

Lavoisier, with great advantages of previous discovery by others, has shewn much ability in general arrangement and induction, but little disposition to display those prior claims.

Next in order came M. Lavoisier, who having repeated the experiments of Boyle and Hales on the calcination of metals, and ascertained that oxigen in all cases combined with them, extended his views to the combustion of certain inflammable substances, and found the product, instead of calx, to be a fluid or gaseous substance, possessed of acid properties. Undoubtedly the confirmation and extension of these facts are highly creditable to the industry and sagacity of Lavoisier: but from what has been already stated, it appears that Mayow first shewed that, to the constitution of the acid spirit of nitre, a certain portion of the air of the atmosphere was necessary; that Hales had in all probability found this acid by the combustion of sulphur; and that Rutherford had not only done the same thing, but had drawn the proper conclusion from it. Be it remembered also, that the great fact essential to complete the explanation of acidification, viz. the discovery of oxigen gas, was known to Lavoisier, but at the time unknown to all these authors; and that, in truth, oxidation and acidification, effected by combustion, are only particular examples of that more general law which M. Lavoisier so successfully laboured to establish,—viz. that in every case of combustion, oxigen combines with the burning body, and forms various compounds according to the nature and composition of that body. The nature, however, of these compounds, the phenomena which attend their formation, and all the variation of circumstances with which, and under which, they take place, have not hitherto been distinctly traced out and ascertained; and until this be

be done, we must, I fear, look in vain for any thing like a *complete theory of combustion.*

But whilst some substances, as sulphurated hydrogen, are found to possess acid properties, though they contain no oxigen; and many inflammable and metallic bodies unite with oxigen during combustion, and yet have nothing of the character of an acid; we must either deny that oxigen is universally the acidifying principle, or that the ordinary characters by which acids are distinguished are arbitrary and false. "If we lay it "down as an axiom that oxigen is the acidifying principle, we "must either include among acids a great number of bodies "which have not the smallest resemblance to those substances "which are at present reckoned acids, or exclude from the "class several bodies, which have the properties of acids in "perfection. The class of acids being perfectly arbitrary, "there cannot be such a thing as an acidifying principle in the "most extensive sense of the word."* But if the acidifying principle be unknown, it must be held premature in M. Lavoisier to claim the discovery of the *theory of acidification;* and his merits, therefore, in this case will consist not in forming a just and general theory, but in improving and extending our knowledge of the facts from which, perhaps, a true theory may hereafter be deduced.

The theory of combustion, as well as of acidification, yet imperfect.

I have sometimes thought, that it might be well to indulge M. Lavoisier and his associates in their present unqualified claims to the theories of modern chemistry, and of physiology founded on chemical principles, from a conviction that the duration of these theories will be temporary only, and that many parts of them ere long will undergo a complete revolution; and that, therefore, with the fall of the theories, the claims would necessarily cease. It is the introduction of the new nomenclature that has chiefly supported these claims; and their authors seem to have imagined that by giving a *new name* they established a right to the *thing* which it was meant to designate. But as if to expose and punish their injustice and presumption, later discoveries are daily proving their theories are false and insufficient, and reducing their new names to mere arbitrary terms, little better, in some respects, than those which they supplanted. Oxigen is not proved to be the acidifying

The new nomenclature of chemistry has caused various chemical theories to be admitted without sufficient examination.

* Thomson, ii. 5, edit. 1.

prin-

principle, as its etymology would imply: azotic is not the only gas which takes away life; and purified charcoal does not form carbon, as Lavoisier supposed. But the *facts* on which these theories have been raised will remain, when the theories shall have passed away; and of these, the discoveries of Scheele and Priestley exceed in ingenuity, diversity, and number, those of M. Lavoisier and all his associates; and for originality, precision, and importance, what have they to produce at all comparable to the discoveries of Cavendish and Black. Whatever may be the fate of other republics, it is devoutly to be wished that that of letters and of science may for ever stand; and when its rights are thus openly invaded, it cannot, surely, be thought unbecoming in us, the countrymen of Bacon, Boyle, and Newton, to stand forward in its defence, lest the shades of those immortal names rise up in judgment against us!

I am, Sir,

Your obedient servant,

E. D.

XI.

Facts towards forming a History of Gold. By Professor PROUST*.

Oxigen requisite to the solubility of gold.

TO ascertain the quantity of oxigen which gold requires for its solution in acids, is a point essential to be determined in the history of this metal, and I found it attended with more difficulty than I had expected.

800 p. muriatic acid, and 200 nitric, dissolve 187 of gold.

Six hundred grains of muratic acid at 12° of Baumé's hydrometer, and 200 of nitric acid at 40°, dissolved by the assistance of heat 144 grains of gold. Having added 200 grains of muriatic acid to the solution, it took up 43 grains more of gold; consequently 1000 grains of aqua regia, made of four parts of muriatic acid and one of nitric, of the strength respectively indicated by the gravities noted above, are capable of dissolving 187 grains of gold. The nitric acid being of no use here but for the oxidation of the muriatic, it is evident, that

The muriatic, the real solvent, should predominate.

* Translated from the Journal de Physique, Feb. 1806, vol. lxii. p. 131.

The

the latter, which is the real solvent of the gold, should predominate in the aqua regia. The same things take place in the solution of platina.

Platina comports itself like gold.

Muriate of gold crystallized.

To obtain the muriate easily in a crystallized state, it is advisable to keep an excess of gold in the solution, and to add muriatic acid from time to time, till it is perceived to act on the gold no longer. By proceeding thus the nitric acid is exhausted, and at the end there remains none to disturb the crystallization.

Extremely deliquescent.

The solution evaporated to a certain point gives a lamellated crystallization, but a coagulated one if it be concentrated too much. This muriate is so liquefiable, so difficult to obtain dry, and of course without risking considerable loss, that it scarcely ought to be taken out of the retort, if we have no other object in view but to exhibit it. In summer it becomes liquid in the morning, and crystallizes towards evening, and passes through this alternation during the continuance of the hot weather.

Acerb and slightly bitter.

The taste of the pure muriate is ascerb with a little bitterness, but without that after taste of metal, which render the solutions of silver, copper, &c., so disagreeable.

Soluble in alcohol.

The muriate of gold is perfectly soluble in spirit of wine. This solution assisted by heat experiences no change; the alcohol is not converted into ether; distillation separates them, and the muriate is recovered unchanged.

Decomposed by heat.

This muriate being distilled gives out abundance of water and oxigenated muriatic acid. The gold remains spongy and without lustre at the bottom of the retort. The vapours carry over some of the muriate of gold, which is found in the receiver; but very little, as Boyle observed. The decomposition of the muriate of gold exhibits the same appearances in every respect as that of the muriate of platina: both yield oxigenated acid and pure metal.

Auriferous Ether.

Sulphuric ether dissolves it,

Sulphuric either takes the muriate from the solution of gold and leaves the nitric acid alone. The crystallized muriate too dissolves in it with the greatest facility, and without residuum.

but will evaporate and leave the muriate unchanged.

The auriferous ether on exposure to the air loses its solvent, and is reduced to a yellow, acerb liquid, which is always pure

muriate

C. Hoffmann first mentioned it.

Baumé first proposed gilding with it.

muriate. C. Hoffmann was, I believe, the first who, in his dissertation on the vinous vitriolic acid, made known this action of ether on the solutions of gold. Baumé appears to me to have been the first who proposed the use of the auriferous ether for gilding watch-work. Within these few years it has been announced as well fitted for gilding figures on iron and steel. I shall recite here not my success, but attempts extremely unsuccessful; which render it incumbent on the author of this proposal to explain himself more clearly, if he would render a service to the arts, and to many amateurs, who complain of having lost both their gold and their labour.*

Attempts to gild on steel with it unsuccessful.

The ether that has become coloured by standing on a solution loaded to the highest degree, is far from containing as much gold as is requisite for gilding with success. By means of a siphon with a bulb I drew off the colourless liquid beneath the ether, and replaced it by fresh solution: thus the ether becomes of a deeper colour and more loaded. On the third or fourth change the appearances alter, for the auriferous ether no longer swims at the top, but sinks to the bottom with the weight and consistency of oil of cinnamon: on the contrary, it is the nitric acid that floats, and that must be drawn off by the siphon.

Having at length well saturated the ether, and considering my success as certain, I began to trace letters on polished steel, some with a pen, others with a pencil; the strokes exhibited gold, as might be expected from the application of its muriate to metal so easily decomposed; but I must say, that by no means I could contrive to give them the quantity of gold, or the continuity, consistence, and lustre, that I wished. The gilding was very different from that of Solingen. We shall not be surprized at this, if we analyze the effects of the gilding, for we shall at once discover, that a single stroke of this ether applied on steel immediately produces four different effects, three at least of which are opposite to the end proposed.—To precipitate gold, to precipitate muriate of iron, to lay bare the carbon of the steel, and to take off the polish from every point touched, are the effects produced.

Causes of its failure.

* Prof. Proust certainly had not seen Mr. Stodart's description of the process, Philosophical Journal, vol. xi. p. 215, as appears from the account he gives of his own. T.

It

It occurred to me to plunge the steel into water the moment I had traced the figures, and afterwards to dry it; presuming I should thus diminish the inconveniences arising from the muriate of iron; but the figures did not acquire from this more adhesion, or more lustre. The palm of the hand applied gently to polish them, rubbed them off immediately.

It was to as little purpose that I dried the steel, after it was gilded and washed, with a heat sufficient to burn the hand. The gold indeed was thus rendered more firmly adherent, but friction would not give it more lustre, because, however the ether may be loadod with it, it never deposits enough on the figures to cover the blackness of the metal, and to give that continuity of parts, that consistence, and that reflection of light on which the brilliancy of gold depends. Finally, this gilding, as it is now before me on the plates with which I made my experiments, is not even equal to what may be produced by a solution of sulphate of copper. If such were the results of an ether loaded with gold, and from which I might have promised myself some success, what is to be expected from an auriferous ether prepared according to the common receipts?

Various precipitates of Gold.

Gold precipitated by sulpurated hydrogen, washed and dried, is nothing but a mixture of sulphur and pure gold. Heating it in a retort is sufficient to separate the metal from the sulphur. Consequently there is neither a sulphuret nor a hydrosulphuret of gold. **Gold precipitated by sulphurated hydrogen:**

The sulphureous acid precipitates it pure. The gold is in such a state of division, that I conceived at first it might be employed for painting on enamel, or for gilding; but the metallic particles were quickly susceptible of an attraction, that collected them together, consolidated them, and made them assume the consistency of a tenacious, though spongy substance. In this state nothing more is to be done with them. **—by sulphureous acid:**

Of the precipitation of Gold by the sulphate of Iron.

With the solution of this salt we succeed much better, and the result is a fine powder of a purple-red colour, the tint of which, however, is nothing like the purple of Cassius. Being washed in acidulated water to free it from iron, it is to be kept **—by sulphate of iron.**

under water, because in this state it is proper for experiments that require an easy and prompt solution of this metal.

Gives a deep purple to porcelain.

This gold applied on porcelain gives a deep purple. We shall return to the state of the gold in this colour.

Partly soluble in marine acid.

Marine acid of 12° boiled on this powder of gold, very evidently dissolves some of it, and acquires a yellow colour. A slip of tin put into it produces the purple in an instant. Gold, therefore, assisted by the affinities that favour iron, zinc, &c., is capable of decomposing water. Thus the marine acid, contrary to the received opinion, is capable of attacking gold and silver, as it does so many other metals.

Gold capable of decomposing water.

This precipitate partly soluble in strong nitric acid;

Nitric acid of 40° boiled on this gold dissolves some of it likewise, and becomes coloured.

—but not by that used in parting.

An acid of 36° dissolves some too, but so little, that it is scarcely to be detected by means of tin. With an acid of 32°, like that employed in parting, it may be doubted whether any gold can be taken up; particularly as the cornet is far from exposing so many points to the attack of the acid as the powder in question.

Solution of gold precipitated by phosphorus gas, but not by phosphorus acid.

Hydrophosphorated water precipitates the solution of gold. That in which phosphorus is kept does the same, but the effect is owing exclusively to the gas, for the phosphorus acid has no action on this solution in less than ten or twelve hours.

Precipitation by Alkalis.

Precipitated by potash.

Potash purified by alcohol precipitates from the muriate of gold a powder, that is at first yellow, and then violet, if we operate with a large quantity of water, but which appears black when it has been washed and dried. Nothing is so capricious as this preparation. An excess of alkali, saturation, a boiling heat, are insufficient to render us masters of it. The liquors always remain more or less impregnated with gold.

An uncertain preparation.

It frequently happens, that the precipitation goes on till the next day; but instead of adding to the black powder, it covers it with a metallic pellicle, or even gilds the vessel in a very brilliant manner. I have kept one in this state as an object of curiosity.

A mixture of oxide and reduced gold.

If the black powder have been washed and dried with the gentlest heat, what we should expect to be a pure oxide, is nothing but a mixture of oxide and reduced gold. This is what militates

militates against our being able by these means to ascertain the degree of oxidation of this precious metal.

If muriatic acid be applied to this powder, it dissolves the oxide, and leaves the pure gold, which is always more abundant. Nitric acid of 40° dissolves only a few atoms of the oxide, and it must be assisted by heat. This solution is of a slight yellow colour; and if it be diluted with water, the gold separates from it of the colour of fulminating gold. This precipitate still retains the state of an oxide, accordingly the muriatic acid dissolves it immediately. Action of acids on it.

The aqueous sulphuric acid dissolves some of it also, but less than the preceding. It is seen by the violet colour it assumes, if a few drops of muriate of tin be added. To conclude, carbonates are not in any respect more advantageous for the precipitation of gold; which has compelled me to give up this point, repeating the words of Bergman: "all gold is "precipitated with difficulty, so that I am uncertain of the "weights." Carbonates not preferable to alkalis. Gold, difficult to precipitate.

Fulminating Gold.

A hundred parts of fulminating gold passed through sulphurated hydrogen, washed and heated, left seventy-three of pure gold. A hundred parts of gold consequently give about 137 of fulminating gold. If any means of appreciating the ammonia that attaches to the oxide could be found, the oxidation of the gold could easily be deduced from this. Fulminating gold.

Kunckel observed, that the oxide of gold obtained by means of alkalis, and moistened with ammonia, became fulminating. Orschal too must be reckoned among the number of those who have been near falling victims to its detonation. An agate mortar, in which he was rubbing this dangerous oxide, flew into splinters under his hand. He received no wound, but he adds that he felt a sensation, as if a musket loaded with sand had been discharged full in his face. According to him, Raymond Lully experienced a similar accident. Dangers attending it.

Orschal too used fulminating gold to give a purple colour to glass. It even appears, that this use of it was known before his time. Hence it might have been concluded at that period, that tin was not a necessary ingredient in the purple colour. Used to colour glass purple.

Analysis of the precipitate.

I put a hundred grains of the precipitate into a little glass retort over the flame of a lamp. Scarcely had a few minutes elapsed before a rapid stream of white vapours spouted out, forming a thick cloud, which I took care not to inhale. I judged, that a hasty disoxidation of the gold might have produced this kind of detonation; and in fact the gold was found to be completely disoxided. The retort was coated with mercurius dulcis mixed with corrosive sublimate.

Detonated.

Analysis repeated.

I repeated the distillation, throwing the precipitate by small portions at a time in to a matrass previously weighed and placed over a lamp. The results being less tumultuous than before, allowed me this time to observe better what passed; but it was impossible for me to appreciate the humidity, and consequently the proportion of the oxigen to the gold.

The oxigen in this precipitate, however, was much more abundant, as far as can be conjectured, than in the former; for 100 grains having afforded of

Results.

Mild and corrosive muriate	16,
Gold	58,
Total	74,

it is evident, that, as the 26 wanting to make up the 100 could not contain more than about 8 parts of water, the oxigen must have been in the proportion of 12 parts to 58 of gold; which would give the proportions of gold 100, oxigen 31. I am inclined, however, to consider this as more certain than the preceding; because, as I applied a boiling heat to the first precipitate, part of the gold may have been more or less disoxidated; a circumstance which I took care to avoid in the preparation of the second. But unquestionably it would be premature to attempt to establish any theory on these facts. It is necessary to examine and re-examine them, but my occupations prevent me from doing this at present. I shall only say, that the mercurius dulcis which here accompanies the oxide of gold, did not proceed from any portion of oxide at a maximum, that my mercurial solutions might have retained. What then was the origin of this mercurius dulcis? What could be the occurrence of affinities, that reduced the sublimate to the condition of mercurius dulcis, and united it thus to the oxide of gold?

100 gold to 31 oxigen.

Father inquiries necessary.

Whence came the mild muriate?

I shall

I shall conclude these details with a property of this precipitate much more extraordinary than those that have been mentioned.

If a few grains be heated on paper over the flame of a candle, they soon melt and explode, emitting their puffs of white smoke before they are reduced to the state of pure gold. But if it be first mixed with a little flowers of sulphur, triturating them together with the point of an ivory spatula, and then heated gently over the candle, it detonates very easily, and with as sharp a noise as fulminating gold. Extraordinary property of this precipitate.

The first of my two precipitates, in which I suspected there was less oxigen than in the second, detonates notwithstanding as well as this.

The oxide of gold obtained by means of potash, mixed with sulphur and heated in the same manner, melts obscurely, but without the least tendency to detonation. The detonation of the preceding oxides is a constant property that never fails. If the mixture be scattered about, the detonation equally takes place; but when the precipitate is well collected together, the report is single, and consequently very loud. After the detonation nothing is found between the papers but gold in a state of division.

Now if we reflect on a result thus singular, we shall find gold here rendered fulminating by what destroys this property in ammoniacal fulminating gold; and without the assistance of ammonia we bring it to produce effects, the explanation of which must necessarily shake the very theory of fulminating gold. What influence can the two muriates of mercury have in this detonation? This remains to be inquired. Singularity of this fulminating powder.

(The conclusion in our next.)

XII.

Note from Thomas Young, *M.D. F.R.S., &c. recommending the Translation of a Memoir of M. Laplace. With some Remarks.*

To

To Mr. NICHOLSON.

DEAR SIR,

I take the liberty of recommending for insertion in your Journal, a paper of M. Laplace on "Capillary Tubes," published in the Journal de Physique, for January last; as I imagine it would be satisfactory to your readers to compare it with the "Essay on the Cohesion of Fluids," which you have done me the honour to reprint. As far as M. Laplace has pursued the subject, he has completely confirmed my conclusions, although by a very different mode of calculation: his reasoning appears, however, to me to be defective, for want of attending to the force of repulsion, which in most cases exactly balances that of cohesion. I had contented myself with determining by an approximate construction the form assumed by the surface of a fluid in a cylindrical tube of moderate diameter; I was in hopes that so consummate a mathematician as M. Laplace would have attempted a direct solution of the problem: but he has left it wholly untouched. I was also anxious to find a confirmation of my conclusions respecting the relation between the mutual force of attraction of a solid and a fluid, and the angle formed by their surfaces: but my expectations were again disappointed. The inferences which I had made from the experiments of Taylor, Achard, and Morveau, are such as might have been deduced without much difficulty, either from M. Laplace's theory or from mine: if they had been so fortunate as to attract M. Laplace's attention before his memoir was read to the Institute, he would perhaps have confirmed and extended them with his usual accuracy and ingenuity.

I am, Dear Sir,

Your very obdient humble servant,

THOMAS YOUNG.

Welbeck-street,
28th May, 1806.

Errata.

Page 83, line 15, for .0054 read .054.
—— — —— 21, for .1 read 1.
—— 159, —— 21, for .1 read 1.

Abstract

Abstract of a Memoir on the Theory of Capillary Tubes. By M. LAPLACE.*

THIS memoir, destined to appear among those of the first class of the Institute, is preceded by the following analysis of the theory it contains.

Clairaut was the first who submitted the phenomena of capillary tubes to strict and accurate analysis, in his *Traité sur la Figure de la Terre.* But his theory, though exposed with the elegance which characterizes that excellent and important work, leaves to be desired a compleat explication of the chief of these phenomena; namely, why the elevation of the fluid above its level in tubes of the same matter, is inversely as their diameters. This great geometer is contented to observe, without proving it, that there must be an infinity of laws of attraction which, when substituted in his formulas, give that result. I have long sought to supply what is wanting in the theory of Clairaut. New researches have at length conducted me not only to ascertain the existence of such laws, but have likewise shewn that every law, according to which the attraction ceases to be perceptible at any perceptible distance, must give an elevation of the fluid in the inverse ratio of the diameter of the tube; and the result is a complete theory of this description of phenomena.

The theory of capillary tubes by Clairaut —imperfect.

Clairaut supposes that the action of the capillary tube is sensible upon the infinitely thin column of the fluid which passes through the axis of the tube. I differ from him in opinion in this respect, and think with Hawksbee, and many other philosophers, that the capillary attraction, like the refractive force and all the chemical affinities, is not sensible, except at imperceptible distances. Hawksbee has observed, that in glass tubes, whether they be very thin or very thick, water rises to the same height whenever the interior diameters are the same. The cylindrical zones of the glass which are at a sensible distance from the interior surface, do not therefore contribute to the ascent of the water, though in each of them separately taken, this fluid would rise above its level. A very simple experiment also proves the

Clairaut supposed the capillary ttraction extended to the axis of the tube: But this is not consistent with the facts.

* Read to the French National Institute, 23d Dec. 1805, and translated from the paper communicated by this author to the Journal de Physique, lxii. 120.

That the attraction of a glass tube on water does not act at any perceptible distance is shewn by greasing it.

truth of this principle. If the interior surface of a tube of glass be covered with an extremely thin coating of any greasy substance, the capillary effect will be destroyed as to sense. Nevertheless the tube always acts in the same manner upon the column of fluid in the axis; for the capillary attraction must be transmitted through bodies in the same manner as is observed with regard to gravity and the attractions and repulsions of magnetism, and even of electricity. Newton, Clairaut, and all geometers who have subjected this class of attractions to computation, have proceeded upon that hypothesis: since therefore the capillary attraction is destroyed by the interposition of a coating of fat matter, however thin it may be, it must follow that the action of the tube will be insensible at any sensible distance.

Mercury attracts and adheres to a glass tube; but in general it is repelled by a thin coating of water.

The following phenomenon affords an additional proof of the principle here announced. It is known that by strong ebullition of mercury in a capillary tube the fluid becomes elevated to the level, and even above the level if the boiling be continued. This phenomenon appears to me to depend upon the thin coating of water which in the ordinary state lines the inner surface of the tube and weakens the mutual action of the glass and the mercury; an action which becomes more and more manifest in proportion as the thickness of that coating is diminished by the heat of boiling. In the experiments which I made with M. Lavoisier upon barometers by boiling the mercury for a long time in them, we caused the convexity of its interior surface to disappear; we even succeeded in rendering it concave; but we always restored the effect of the capillarity by introducing a drop of water into the tube. If we consider the extreme thinness which the aqueous coat must have, particularly when the tube and the mercury has been well dried, which process is not sufficient to destroy the capillarity, we may form a judgment that the action of the glass on this fluid is not sensible but at insensible distances.

The author's process of analysis, or the theory of capillary tubes.

Proceeding on this principle, I determine by the formulas of my treatise de Mécanique Céleste, the action of a mass of fluid terminated by a concave or convex spherical surface upon an interior column of fluid included in an infinitely narrow canal which passes through the axis of that surface. By this action I understand the pressure which the fluid included in the canal

canal would exercise, by virtue of the attraction of the entire mass, upon a plane base situated in the interior of the canal, perpendicular to its sides, at any sensible distance whatever from the surface, that base being taken for unity. I show that this action is less or greater than if the surface were plain; less if the surface be concave; greater if the surface be convex. Its analytical expression is composed of two terms; the first much greater than the second, expresses the action of the mass terminated by a plain surface, and I think that on this term depend the phenomena of the adherence of bodies to each other, and of the suspension of the mercury in a barometer tube, at an elevation which is three or four times greater than would arise from the pressure of the atmosphere. The second term expresses that part of the action which is due to the sphericity of its surface; it is positive or negative, accordingly as the surface is convex or concave. I shew that in each case this term is in the inverse ratio of the radius of the spherical surface. Thence I conclude the general theorem, namely, that in all the laws wherein the attraction is not sensible but at insensible distances, the action of a body, terminated by a curve surface, on an interior canal infinitely narrow and perpendicular to that surface in any point whatever, is equal to half the sum of the actions on the same canal, of two spheres which should have for their radii the greatest and the smallest of the radii osculators of the surface at that point. By means of this theorem and the laws of the equilibrium of fluids, we may determine the figure which a fluid mass animated by gravity or weight must take. I shew that in a cylindrical tube of considerable diameter, the section of the surface of the fluid, by a vertical plane, is a curve of the genus of those which geometers have called elastic, and which are formed by an elastic plate or blade bended by weights; this results from the circumstance, that in that section, as in the elastic curve, the force due to the curvature is reciprocal to the radius osculator. If the tube be very narrow, the surface of the fluid approaches the more to that of a spherical segment as the diameter of the tube is smaller. I afterwards prove, that in different tubes of the same matter, these segments are nearly alike; whence it follows that the radii of their surfaces are very nearly proportional to the diameters of the tubes. This similitude of the

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the spherical segments will be evident, if we consider that the distance where the action of the tube ceases to be sensible, is imperceptible; so that if, by means of a very powerful microscope, we could succeed in rendering it sensible to the amount of one *millimetre* (or one twenty-fifth of an inch English), it is probable that the same amplifying power would give to the diameter of the tube an apparent magnitude of several metres. The surface of the tube may therefore be considered as being very nearly plain in a radius equal to that distance; the fluid in that interval will therefore fall or rise as to that surface very nearly as if it were plain: beyond that space, the fluid being no longer subjected as to sense to any power but that of weight and its own action upon itself, its surface will be extremely near to that of a spherical segment, of which the extreme sides, being those of the surface at the limits of the sphere of sensible activity of the tube, will be very nearly alike inclined to the horizon in the different tubes; whence it follows that all these segments will be very nearly similar.

The near coincidence of these results gives the true cause of the ascent or depression of fluids in capillary tubes in the inverse ratio of their diameters. If through the axis of a tube of glass, we imagine a canal infinitely narrow, which being recurved a little below the tube, shall proceed to terminate at the plane and horizontal surface of the water of a vessel in which the lower extremity of the tube is plunged, the action of the water of the tube on this canal will be less, on account of the concavity of its surface, than the action of the water of the vessel on the the same canal; the fluid must therefore rise in the tube to compensate this difference; and as this is from what has been shewn in the inverse ratio of the diameter of the tube, the elevation of the tube above its level must follow the same ratio.

If the fluid be mercury, its surface within a capillary tube of glass is convex; its action on the canal is therefore stronger than that of the mercury of the vessel, and the fluid must be depressed in the tube on account of this difference, and consequently in the inverse ratio of the diameter of the tube.

The attraction of capillary tubes has not, therefore, any influence on the elevation or depression of the fluids which they include, except by determining the inclination of the first planes of

of the surface of the interior fluid, which are extremely near the sides of the tube, upon which inclination the concavity or convexity of that surface and the length of its radius depend. If by the effect of rubbing the interior fluid against the sides of the tube the curvature be increased or diminished, the capillary effect will increase or diminish in the same proportion.

Laplace's theory of the elevation and depression of fluids by that effect of attraction which is called capillary.

It is interesting to ascertain the radius of curvature of the surface of water included in capillary tubes of glass. This may be known by a curious experiment, which shows at the same time the effects of the concavity and convexity of surfaces. It consists in plunging in water to a known depth a capillary tube, of which the diameter is likewise known. The lower extremity of the tube is then to be closed with the finger, and the tube being taken out of the water, its external surface must be gently wiped. Upon withdrawing the finger in this last situation, the water is seen to subside in the tube and form a drop at its lower base; but the height of the column is always greater than the elevation of the water in the tube above the level in the common experiment of plunging it in water. This excess in the height is owing to the action of the drop upon the column, on account of its convexity; and it is observable that the increase in the elevation of the water is more considerable the smaller the diameter of the drop beneath. The length of the fluid column which came out by subsidence to form the drop, determines its mass; and as its surface is spherical, as well as that of the interior fluid, if we know the height of the fluid above the summit of the drop, and the distance of this summit from the plane of the interior base of the tube, it will be easy to deduce the radii of these two surfaces. Some experiments lead me to conclude that the surface of the interior fluid approaches very nearly to the figure of an hemisphere.

Clairaut has made this singular remark, namely, that if the law of attraction of the matter of the tube upon the fluid differs only by its intensity from the law of the attraction of the fluid upon itself, the fluid will be elevated above the level, while the intensity of the former of these attractions exceeds the half of the intensity of the latter. If it be exactly the half, it is easy to show that the surface of the fluid in the tube will be horizontal, and that it will not be elevated above the level. If these

two

Laplace's theory of the elevation and depression of fluids by that effect of attraction which is called capillary.

two intensities be equal, the surface of the fluid in the tube will be concave and hemispherical, and the fluid will rise above the level. If the intensity of the attraction of the tube be nothing, or insensible, the surface of the fluid in the tube will be convex and hemispherical, and the fluid will be depressed below the level. Between these two limits, the surface of the fluid will be that of a shperical segment, and it will be concave or convex accordingly as the intensity of the attraction of the matter of the tube upon the fluid shall be greater or less than the half of that of the attraction of the fluid upon itself.

If the intensity of the attraction of the tube upon the fluid surpass that of the attraction of the fluid upon itself, it appears probable to me that the fluid in that case, attaching itself to the tube, will form an interior tube, which alone will raise the fluid, of which the surface will be concave and hemispherical. I presume that this is the case with water in a tube of glass.

After having considered fluids terminated by spherical surfaces, I consider them as terminated by cylindrical surfaces. This case is that of a fluid included between two planes very near each other, and having their lower extremities plunged in a vessel containing the same fluid. I find by analysis that the fluid ought to rise or be depressed accordingly as the cylindric surface of the fluid is concave or convex, and that this elevation or depression also follows the inverse ratio of the distance between the planes. I find also that the elevation or depression is equal to that which would take place in a cylindrical tube of which the internal semi-diameter should be equal to that distance. Having obtained this result by analysis, I proposed to M. Hauy to verify the same by experiments, and he found it perfectly conformable to those which at my request he made. And afterwards, upon revising what has been written on the capillary action, I saw that these experiments had already been made with great care in the presence of the Royal Society of London, under the eyes of Newton, and that their result is exactly conformable to that of the analysis. This may be seen by the following passage in his Optics, an admirable work, in which that profound genius has anticipated beyond the times in which he lived, a great number of original views which have been confirmed by modern chemistry. "And of the same kind" (says he, question 31.) " with these experiments are those which

" follow

"follow: If two plane polished plates of glass (suppose two pieces of a polished looking-glass) be laid together, so that their sides be parallel and at a very small distance from one another, and their lower edges be dipped into water, the water will rise up between them. And the less the distance of the glasses is, the greater will be the height to which the water will rise. If the distance be about the hundredth part of an inch, the water will rise to the height of about an inch; and if the distance be greater or less in any proportion, the height will be reciprocally proportional to the distance very nearly. * * * * * . And if slender pipes of glass be dipped at one end into stagnating water, the water will rise up within the pipe, and the height to which it rises will be reciprocally proportional to the diameter of the cavity of the pipe, and will equal the height to which it rises between two panes of glass, if the semi-diameter of the cavity of the pipe be equal to the distance between the planes or thereabouts. And these experiments succeed after the same manner in vacuo as in the open air, (as hath been tried before the Royal Society), and therefore are not influenced by the weight or pressure of the atmosphere."

Laplace's theory of the elevation and depression of fluids by that effect of attraction which is called capillary.

The capillary phenomena of inclined planes and of conical and prismatic tubes are so many corollaries of my analysis. Thus it is observed that a short column of water in a conical tube, open at both ends and kept horizontal, is moved towards the summit of the tube; and from what has been explained, it is clear that this must be the event. In fact, the surface of the fluid column is concave at its two extremities; but the radius of its surface is smaller at the end next the summit than at the end next the base; the action of the fluid on itself is therefore less on the side next the summit, and consequently the column must tend that way. But if the column of fluid be mercury, its surface will then be convex, and its radius also less towards the summit than towards the base; but on account of its convexity, the action of the fluid on itself is greater towards the summit, and the column must be carried towards the base of the tube.

We may balance this action by the proper weight of the column, and keep it suspended in equilibrio, by inclining the axis of the tube to the horizon. A very simple calculation shews that

Laplace's theory of the elevation and depression of fluids by that effect of attraction which is called capillary.

that if the length of the column be very small, the sine of the inclination of the axis will then be very nearly in the inverse ratio of the square of the distance of the middle of the column from the summit of the cone; and this also takes place, if instead of causing a drop of the fluid to move in a conical tube it be made to move between two planes forming a very small angle between them.' These results are entirely conformable to experience, as may be seen in the Optics of Newton (query 31.).

Calculation also teaches us, that the sine of the inclination of the axis of the cone to the horizon, is then, very nearly, equal to a fraction, of which the denominator is the distance of the middle of the drop from the summit of the cone, and its numerator the height to which the fluid would rise in a cylindrical tube, having for its diameter that of the cone at the middle of the column. If two planes which include a drop of the same fluid form between them an angle equal to double the angle formed by the axis of the cone and its sides, the inclination to the horizon of a line which equally divides the angle formed by the planes, need be only half that required in the axis of the cone, in order that the drop should remain in equilibrio.

The preceeding theory likewise gives the explanation and measure of a singular phenomenon, presented by experiment. Whether the fluid be elevated or depressed between two vertical planes, parallel to each other, and plunged in the fluid at their lower extremities, their planes tend to come together. Analysis shews us that if the fluid be raised between them, each plane will undergo, from without, inwards, a pressure equal to that of a column of the same fluid, of which the height would be half the sum of the elevations, above the level, of the points of contact of the interior and exterior surfaces of the fluid with the plane, and of which the base should be the parts of the plane comprised between the two horizontal lines drawn through those points. If the fluid be depressed between the planes, each of them will in like manner undergo from without, inwards, a pressure equal to that of a column of the same fluid, of which the height would be half the sum of the depressions below the level of the points of contact of the interior and exterior surfaces of the fluid with the plane,

and

and of which the base should be the part of the plane comprised between the two horizontal lines drawn through those points.

Laplace's theory of the elevation and depression of fluids by that effect of attraction which is called capillary.

In general, if we compare the theory which I expose, to the numerous experiments of philosophers on the capillary action, we shall see that the results obtained by those experiments are deducible not by vague and uncertain considerations, but by a train of geometrical reasoning, which appears to me to leave no doubt of the truth of this theory. I am desirous that this application of analysis to one of the most curious objects of natural philosophy may prove interesting to geometers, and excite them to multiply more and more these applications, which unite the advantage of giving certainty to physical theories, and adding to the perfection of the analytical art, by the frequent demand for new artifices of calculation.

Note (by the Author).

The demonstration of the preceding theories will be published in one of the succeeding volumes of the Insitute. The following results of analysis may serve to direct those who may be disposed to deduce the principal themselves.

Let us denote by $\phi\ (f)$ the law of the attraction of a fluid particle upon another particle, placed at the distance f; $\phi\ (f)$ decreasing with an extreme rapidity, while f increases, and being insensible for every sensible value of f. Let us also designate by $c - \Pi\ (f)$ the integral $\int df.\ \phi\ (f)$ taken from $f = o$, c being the value of that integral, when f is infinite; $\Pi\ (f)$ will in like manner decrease with an extreme rapidity, and will be also insensible for all the sensible values of f. Let us also denote by $c' - \Psi\ (f)$ the integral $\int f\, d.f.\ \Pi\ (f)$, c' being its value when f is infinite; $\Psi\ (f)$ will be likewise insensible for all the sensible values of f. Lastly, let us denote by K and H the integrals $2\ \pi \int dz.\ \Psi\ (z)$ and $2\ \pi \int z\, dz.\ \Psi\ (z)$ taken from z nul to z infinite, π being the semi-circumference of which the radius is unity. It will be seen by the analysis of No. 12, of the second book of La Mécanique Céleste, that the action of a sphere of which the radius is b, upon the fluid included in a canal infinitely narrow, perpendicular to its surface, is $K + \frac{H}{b}$. By this action I understand the pressure which the fluid of the canal would exert by virtue of this action, upon a base perpendicular to the direction of the canal, placed in its interior at any sensible distance whatever, from the surface of the body, and taken for unity. This would also be the expression of the action of a body terminated by a sensible segment of a sphere whose radius is b; which results from

Laplace's theory of the elevation and depression of fluids by that effect of attraction which is called capillary.

from the consideration that the attraction is not sensible but at insensible distances. If the surface, instead of being convex, is concave, b must be made negative, and then the action becomes $K-\frac{H}{b}$. In the case of a plane or where b is infinite, it becomes reduced to K.

These attractions are of the same description as those on which the refraction of light depends, and which I have considered in the Nos. 2 and 3, of the second book of my Mécanique Céleste. That which renders them independent of the dimensions of bodies, is that it is indifferent to take the preceding integrals, from zero to infinite, or from zero to a sensible value of the variable quantity.

The theorem relative to the action of any body whatever, upon an interior canal, infinitely narrow and perpendicular to its surface, is demonstrated by observing that at each point of the surface, we may conceive an osculator ellipsoid which confounds itself with the body, so that the difference of the actions of these two bodies upon the canal is insensible; and it is easy to prove that the action of an ellipsoid upon a canal which passes through one of its axes, is equal to the half sum of the actions of two spheres, which should have for their radii the greatest and the smallest of the radii osculators of the surface of the ellipsoid at the extremity of that axis. Calling, therefore, these two radii, b, and b', the action of the body will be $K+\frac{H}{2}.\left(\frac{1}{b}+\frac{1}{b'}\right)$ In the case of a cylindrical surface b is infinite, and the action becomes $K+\frac{H}{2b'}$. The difference between this action and that of a body terminated by a plane surface is therefore $\frac{H}{2b'}$, and consequently the half of what it would be if the surface of the body were spherical and of a radius equal to b'. This is the reason why a fluid is raised or depressed between two parallel planes, only half as much as in a cylindrical tube equal in diameter to the measure of their distance.

XIII.

Processes for making cheap and durable Paints with Fish-Oil. By MR. THOMAS VANHERMAN, *No 21, Mary-le-bone Street, Golden Square.**

Advantageous colours in oil.

HAVING applied a great portion of my time, for several years past, to discover a method of preparing a cheap and du-

* Addressed to the Society of Arts, who awarded him the silver medal and twenty guineas. See their Transactions for 1805.

rable

rable composition for the defence and preservation of all work exposed to the inclemency of the weather, I have now the satisfaction of laying before the Society for the Encouragement of Arts, &c. specimens of some of the above colours ready prepared for use, which will, I flatter myself, be found superior to all others, for cheapness and durability, equal to any in beauty, and not subject to blister or peel off by the sun.

The vehicle made use of for the said paints is fish-oil, the preparation of which is so simple, that when known, gentlemen who have large concerns to paint, may have this composition of any colour manufactured, and laid on by their labourers. I have sent a bottle of the prepared oil; also, a number of patterns, of various colours. The highest price of any, does not exceed three-pence per pound, and many of them so low as two-pence, in a state fit for use. I have likewise sent a pot of white-lead which has been ground with prepared fish-oil, and which, when thinned with linseed-oil, surpasses any white hitherto made use of for resisting all weathers, and retaining its whiteness. I hope my humble endeavours will merit the approbation of the Society, before whom, I will, at any time they shall please to appoint, make the various experiments they may require.

The vehicle is fish-oil.

—and is very cheap.

Instructions how to refine one ton of Cod, Whale, or Seal Oil, for painting, with the cost attending it.

Refining of fish-oil.

	£.	s.	d.
One ton of fish-oil, or 252 gallons,	36	0	0
32 gallons of vinegar, at 2s. per gallon,	3	4	0
12lbs. litharge, at 5d. per lb.	0	5	0
12lbs. white copperas, at 6d. ditto,	0	6	0
12 gallons of linseed-oil, at 4s. 6d. per gallon,	2	14	0
2 gallons of spirits of turpentine, at 8s. ditto,	0	16	0
	£43	5	0

252 gallons of fish-oil,
12 ditto linseed-oil,
2 ditto spirits of turpentine,
32 ditto vinegar,

298 gallons, worth 4s. 6d. per gallon.

Which

Which produces	£67	1	0	
Deduct the expense	43	5	0	
	£23	16	0	profit.

To prepare the Vinegar for the Oil.

Vinegar with litharge and sulphate of zinc, is agitated and left to stand with fish-oil. It thus becomes fit for painters' colours.

Into a cask which will contain about forty gallons, put thirty-two gallons of good common vinegar; add to this twelve pounds of litharge, and twelve pounds of white copperas in powder; bung up the vessel, and shake and roll it well twice a day for a week; when it will be fit to put into a ton of whale, cod, or seal oil; (but the Southern whale oil is to be preferred, on account of its good colour, and little or no smell) shake and mix all together, when it may settle until the next day; then pour off the clear, which will be about seven-eights of the whole. To this clear part add twelve gallons of linseed-oil, and two gallons of spirits of turpentine; shake them well together, and after the whole has settled two or three days, it will be fit to grind white-lead, and all fine colours in; and, when ground, cannot be distinguished from those ground in linseed-oil, unless by the superiority of its colour.

If the oil is wanted only for coarse purposes, the linseed-oil and oil of turpentine may be added at the same time that the prepared vinegar is put in, and after being well shaken up, is fit for immediate use without being suffered to settle.

The vinegar is to dissolve the litharge, and the copperas accelerates the dissolution, and strengthens the drying quality.

The residue, or bottom, when settled, by the addition of half its quantity of fresh lime-water, forms an excellent oil for mixing with all the coarse paints for preserving outside work.

Note.—All colours ground in the above oil, and used for inside work, must be thinned with linseed-oil and oil of turpentine.

☞ The oil mixed with lime-water, I call *incorporated oil*.

The

Painters' colours cheaply and well prepared with fish-oil.

The method of preparing and the expence of the various Impenetrable Paints.

First.—*Subdued Green.*

	£.	s.	d.
Fresh lime-water, 6 gallons	0	0	3
Road dirt finely sifted, 112 pounds,	0	1	0
Whiting, 112 ditto,	0	2	4
Blue-black, 30 ditto,	0	2	6
Wet blue, 20 ditto,	0	10	0
Residue of the oil, 3 gallons,	0	6	0
Yellow ochre in powder, 24 pounds,	0	2	0
	£1	4	1

This composition will weigh 368 pounds, which is scarce one penny per pound. To render the above paint fit for use, to every eight pounds add one quart of the incorporated oil, and one quart of linseed-oil, and it will be found a paint with every requisite quality, both of beauty, durability, and cheapness, and in this state of preparation does not exceed two-pence-halfpenny per pound; whereas the coal tar of the same colour is six-pence.

The method of mixing the ingredients for the Subdued Green.

First, pour six gallons of lime-water into a large tub, then throw in 112 pounds of whiting; stir it round well with a stirrer, let it settle for about an hour, and stir it again. Now you may put in the 112 pounds of road dirt, mix it well, then add the blue-black, after which the yellow ochre, and when all is tolerably blended, take it out of the tub and put it on a large board or platform, and with a labourer's shovel mix, and work it about as they do mortar. Now add the wet blue, which must be previously ground in the incorporated oil (as it will not grind or mix with any other oil). When this is added to the mass, you may begin to thin it with the incorporated oil in the proportion of one quart to every eight pounds, and then the linseed-oil in the same proportion, and it is ready to be put into casks for use.

Lead

Painters' colours cheaply and well prepared with fish-oil.

Lead Colour.

	£.	s.	d.
Whiting, 112 pounds, ...	0	2	4
Blue-black, 5 ditto, ...	0	1	8
Lead ground in oil, 28 ditto, ...	0	14	0
Road dirt, 56 ditto, ...	0	0	6
Lime-water, 5 gallons, ...	0	0	6
Residue of the oil, 2½ ditto, ...	0	5	0
Weighs 256 lbs.	£1	4	0

To the above add two gallons of the incorporated oil, and two gallons of linseed-oil to thin it for use, and it will not exceed 1¼*d.* per pound.

Note.—The lime-water, whiting, road dirt, and blue-black, must be first mixed together, then add the ground lead, first blending it with two gallons and a half of the prepared fish-oil, after which thin the whole with the two gallons of linseed-oil, and two gallons of incorporated oil, and it will be fit for use. For garden doors, and other work liable to be in constant use, a little spirits of turpentine may be added to the paint whilst laying on, which will have the desired effect.

Bright Green.

	£.	s.	d.
112 pounds yellow ochre in powder, at 2d. per lb.	0	18	8
168 ditto road dust, ...	0	1	8
112 ditto wet blue, at 6d. per lb.	2	16	0
10 ditto blue-black, at 3d. ditto, ...	0	2	6
6 gallons of lime-water, ...	0	0	6
4 ditto fish-oil prepared, ...	0	12	0
7½ ditto incorporated oil, ...	0	15	0
7½ ditto linseed-oil, at 4s. 6d. per gallon, ...	2	8	9
592lbs. weight.	£7	15	1

This excellent bright green does not exceed three-pence farthing per pound ready to lay on, and the inventor challenges any colour-man or painter, to produce a green equal to it for eighteen-pence.

Afte

After painting, the colour left in the pot may be covered with water to prevent it from skinning, and the brushes, as usual, should be cleaned with the painting knife, and kept under water. Painters' colours cheaply and well prepared with fish-oil.

A brighter green may be formed by omitting the blue-black; and

A lighter green may be made by the addition of ten pounds of ground white-lead.

A variety of greens may be obtained, by varying the proportions of the blue and yellow.

Observe that the wet blue must be ground with the incorporated oil, preparatory to its being mixed with the mass.

Stone Colour.

	£.	s.	d.
Lime-water, 4 gallons,	0	0	4
Whiting, 112 pounds.	0	2	4
White-lead ground, 28 pounds, at 6d. per lb.	0	14	0
Road dust, 56 pounds,	0	0	6
Prepared fish-oil, 2 gallons,	0	6	0
Incorporated oil, 3½ gallons,	0	7	0
Linseed-oil, 3½ ditto,	0	15	9
Weighs 293lbs.	£2	5	11

The above stone colour, fit for use, is not two-pence per pound.

Brown Red.

	£.	s.	d.
Lime-water, 8 gallons,	0	0	8
Spanish brown, 112lbs.	1	0	0
Road dust, 224lbs.	0	2	0
4 gallons of fish-oil,	0	12	0
4 ditto incorporated oil,	0	8	0
4 ditto linseed-oil,	0	18	0
Weighs 501lbs.	£2	0	8

This most excellent paint is scarcely one penny per pound. The Spanish brown must be in powder.

A good

Painters' colours cheaply and well prepared with fish-oil.

A good chocolate colour is made by the addition of blue-black in powder or lamp-black, till the colour is to your mind, and a lighter brown may be formed by adding ground white-lead.

Note.—By ground lead, is meant white-lead ground in oil.

Yellow is prepared with yellow ochre in powder, in the same proportion as the Spanish brown.

Black is also prepared in the same proportion, using lamp-black or blue-black.

To whiten Linseed-Oil.

Take any quantity of linseed-oil, and to every gallon add two ounces of litharge; shake it up every day for fourteen days, then let it settle a day or two; pour off the clear into shallow pans, the same as dripping-pans, first putting half a pint of spirits of turpentine to each gallon. Place it in the sun, and in three days it will be as white as nut-oil.

This oil, before it is bleached, and without the turpentine, is far superior to the best boiled oil, there being no waste or offensive smell.

THOMAS VANHERMAN.

From experiments made, it appears that fine sand will not answer the purpose of road dirt in painting, and that this dry dirt or dust collected in highways much travelled by horses and carriages, and afterwards finely sifted, is the article recommended, as possessing the properties required.

Enclosed you will find a letter from Mr. Hill, West Lavant, Sussex, builder, and surveyor to his Grace the Duke of Richmond, with his opinion respecting the painting of his Grace's house and premises, at Earl's Court, Little Chelsea; which was finished December, 1803.

The Letter.

SIR,

I have just received your letter dated the 15th instant, and am happy to find that your oil and colour business so well stands the test of others, as well as that of myself. The fish-oil composition you made use of, in all the painting you have done

done at Earl's Court, Kensington, for his Grace the Duke of Richmond, under my superintendance, in 1802-3, was fully equal, if not superior to any painting done in the usual way with linseed-oil, white-lead, &c. I have also the highest opinion of your coarse composition and fish-oil you made use of on the out-buildings, fences, &c. on the above premises; the great body and hard surface it holds out, must be of the greatest preservation to all timbers and fences, exposed to open air, and all weathers. It must also be of the greatest service on plastered stucco, external walls, &c.

Painter's colours cheaply and well prepared with fish oil.

If any father attestation from me, relative to the business you did at the above premises, can be of any service to you, you will command,

Sir, your obedient servant,

West Lavant, W. HILL.
Feb. 7th, 1805.

I beg leave here to subjoin a receipt for a constant white for the inside painting of houses; which paint, though not divested of smell in the operation, will become dry in four hours, and all smell gone in that time.

White Paint.

To one gallon of spirits of turpentine, add two pounds of frankincense, let it simmer over a clear fire until dissolved; strain it and bottle it for use. To one gallon of my bleached linseed-oil, add one quart of the above, shake them well together and bottle it also. Let any quantity of white-lead be ground with spirits of turpentine very fine, then add a sufficient portion of the last mixture to it, until you find it fit for laying on. If in working it grows thick, it must be thinned with spirits of turpentine.—It is a flat or dead white.

XIV.

Letter on the Properties of Tempered Steel. From a Correspondent. T. B.

To Mr. NICHOLSON.

SIR,

IN one of your Journals, I do not recollect which, you signified

Interesting properties of tempered steel.

signified your intention of giving in a future number, some ideas upon certain singular properties of tempered steel. A number of unexplained facts have for some time been known to the workers of steel-plate. As I am concerned in a manufactory of the kind, and in the daily habit of witnessing those curious and anomalous appearances, I thought you might in some measure profit by the following description of the changes which take place in the various processes of hardening, tempering, hammering, burnishing, &c.

Steel-plate hardened and then reduced to spring temper, lost its elasticity by hammering, grinding, &c.

I took a steel plate 30 inches long, 12 broad, and about ,04 thick; I hardened it in a composition of oil and tallow, and afterwards tempered it down to a spring temper; it was now so elastic as to recover its position after being bended; by hammering it to set it straight, it lost a part of its elasticity; after being grouud in the same manner as a saw, the elasticity became still less, having nearly returned to the same state as before hardened; it was then very uniformly heated 'till it became blue, it now recovered the whole of its elasticity; after being glazed bright upon a glazier coated with emery, the elasticity was found to be impaired, but in a less degree than when it was ground; the same effect was also produced by rubbing with emery or sand-paper, and also by burnishing; invariably the elasticity was recovered by bluing, and hence this is always the last operation in the manufactory of elastic steel-plate. Should you at some future opportunity favour the public with your opinion on this subject, and these hints have in the least assisted your inquiry, it will be the utmost wish of

—but recovered the whole spring by bluing.

Your humble and obedient servant,

Sheffield, T. B.

June 18, 1806.

XV.

Description of a process for clearing Feathers from their Animal Oil. By MRS. JANE RICHARDSON.*

Feathers are soaked in lime-water and afterwards drained, washed, and dried.

TAKE for every gallon of clean water, one pound of quicklime; mix them well together, and when the undissolved lime

* For which the Society of Arts awarded twenty guineas. From their Transactions, 1805. The attestations were very satifactory.

is

is precipitated in fine powder, pour off the clear lime-water for use, at the time it is wanted.

Put the feathers to be cleaned in another tub, and add to them a quantity of the clear lime-water, sufficient to cover the feathers about three inches, when well immersed and stirred about therein.

The feathers, when thoroughly moistened, will sink down and should remain in the lime-water three or four days, after which the foul liquor should be separated from the feathers by laying them on a sieve.

The feathers should be afterwards well washed in clean water and dried upon nets; the meshes about the fineness of cabbage-nets.

The feathers must from time to time be shaken upon the nets, and as they dry will fall through the meshes, and are to be collected for use.

The admission of air will be serviceable in the drying; the whole process will be completed in about three weeks; after being prepared as above mentioned, they will only require beating for use.

XVI.

Note from H. DAVY, ESQ. *F. R. S., &c. &c, on the Fluoric Acid in Wavellite.*

To Mr. NICHOLSON.

DEAR SIR, *Killarney, Ireland, June* 15.

I SHALL feel much obliged to you, if amongst the articles of intelligence in your Philosophical Journal, you would mention that I have found the acid which exists in minute quantity in the wavellite (the new fossile from Barnstable) to be the fluoric acid, in such a peculiar state of combination as not to be rendered sensible by sulphuric acid. Fluoric acid in wavellite.

I am, Dear Sir,
with great respect,
Your obedient servant,
H. DAVY.

W. Nicholson, Esq.

 XVIII.

XVII.

SCIENTIFIC NEWS.

Prospectus of an Establishment to be called the London Chemical Society.

Chemical society.

IT has been observed, that those who cultivate any particular branch of experimental science are solicitous of associating with others engaged in similar studies; a common interest in the same subject of conversation excites a spirit of inquiry; thought gives rise to thought, and new ideas, collected in the friendly intercourse of society, often lead to investigations of the greatest importance. The student finds many difficulties removed which impede his progress by the ready information he obtains from men of higher acquirements, whilst those, who are skilled in chemical pursuits, frequently receive important observations from the mere lover of the science: to this may be added, that men, however great their learning or ardour may be for any particular branch of inquiry, yet, when deprived of the opportunity of communicating their ideas to others, not only become negligent and uninterested in improving the stock of knowledge they already possess, but are seldom solicitous about the further cultivation of their mental powers.

From a conviction of these truths, a number of gentlemen, who have a taste for philosophical chemistry are determined to form themselves into a society, in which the talents of a number may be united, and become extensively useful to each other, by mutual communication of their views, their labours, and acquisitions. That their endeavour may prove as interesting as possible, particularly to those promoters of chemical science who cannot devote much time to the perusal of literary journals and periodical publications, arrangements will be made to collect, as speedly as possible, all the chemical news which issue from the laboratories of other operators, both at home and on the continent; and correspondences will be established

tablished to obtain the earliest and best information respecting whatever shall offer itself as new and important in the departments of chemistry, of natural philosophy, and the arts and manufactures, which are dependent on these branches of knowledge. To keep pace with the existing state of chemical science, the intelligence thus collected shall be regularly detailed in their respective meetings; and a book of reference kept as a register, containing the growing mass of philosophical information, which will be laid on the table for the use of the members; together with all those publications and academical journals of repute, which exhibit the transactions of ingenious men in every part of the world. Chemical society.

The views of this society however will not be confined to the mere detail of literary intelligence and chemical conversations; a principal part of their labour will devolve to the practical department of the laboratory. To accomplish this as perfectly as possible, all the interesting discoveries, which from time to time enrich the domain of chemistry, and particularly those complicated, expensive, and difficult experiments, which can be repeated by few individuals only, shall be exhibited in their own laboratory; being persuaded, that important experimental inquiries, when once witnessed, seldom fail to excite that degree of ardour which gives increasing energy to scientific research.

From this the Chemical Society will direct their attention to all such original and specific experiments, as may individually be proposed, and the results they afford shall be minuted in the journal of the laboratory, kept for that purpose, and afterwards published in such a manner as may be directed. These inquiries will embrace whatever is deemed worthy of experimental research in the extensive departments of philosophical, practical, and technical chemistry. It is perhaps needless to state, that their laboratory will be open for the analysis of ores, soils, manures, and such substances in general as are found in the British dominions, and are deemed of private or public importance.

And, as it is certain that the progress, as well as the accurate and extensive ideas, which the cultivators of chemical science may acquire, are greatly facilitated and promoted, by attending to the manipulations, and processes of the practical chemist;

Chemical society.

chemist; it is likewise intended, that all the multifarious operations of the laboratory, shall be regularly employed for obtaining from the crude materials of nature, all those substances which the society requires as instruments of research, or as specimens of truths, as well as those articles used in the chemical arts, and by manufacturers and artists. This part of the views of the Chemical Society will constitute a perpetual series of operations, well calculated to exhibit a summary exposition of all the general and particular processes of the scientific laboratory: a consideration highly important to the progress of real improvement.

To give effect to this undertaking, a regular laboratory is already fitted up, and an extensive collection of apparatus and instruments will be procured, to ensure those auxiliary advantages which are essential to the pursuit of the science.

Such are the outlines of the plan to which the views of the Chemical Society will be directed. A more particular detail of rules and proceedings would be premature and superfluous. It must nevertheless be remarked, that whatever encouragement the establishment may receive, the admission of subscribers, is for the present limited to sixty, and the annual subscription fixed at three guineas.

An unlimited number of gentlemen residing in the country may be admitted as subscribers, on paying one guinea annually, which shall entitle them to visit the society as members, whenever they reside in the capital, provided their stay in town does not exceed three months.

After the first meeting the admission of members shall be decided by ballot, and those who are not inclined to adopt the regulations, then agreed upon by the majority of the subscribers, shall have their subscription immediately returned.

A code of laws will be formed, and proper officers elected so as to form a regular society, which shall be denominated the London Chemical Society.

The admission of members is for the present confined to a committee, who on the present occasion, address the chemical public, and request, that such gentlemen as are desirous of becoming subscribers may favour them with their names, for which purpose a book is opened at their laboratory, *No.* 11, *Old Compton Street, Soho.*

Ancient

*Ancient Works in America, resembling Fortifications.**

The artificial works, best known by the name of fortifications, are daily discovered, in great numbers, and many of them of vast extent, in various parts of the United-States, particularly in the fertile countries adjacent to the rivers Ohio and Missisippi, and their branches. In some of the tumuli, or barrows, connected with these works, copper implements, of different kinds, have been found. So that there can be no doubt that the people who formed, or who possessed, these works, were acquainted with the use of copper. But how far this metal was in *general* use among them, we are not yet prepared to determine. This point, however, may be determined, at some future period. Ancient works in America.

Bishop Madison's ingenious speculations concerning the *design* of the works alluded to,† have induced some persons to suppose, that they were never intended to serve the purposes of fortifications. But for whatever purposes they were used, it is certain, that these works could never have been constructed by a people in the state of society in which the Europeans found the Indian inhabitants of the tracts of country in which the supposed fortifications are so abundantly distributed: and we seem to proceed with entire safety in asserting, that they must have been constructed by tribes, or nations, who were *extremely numerous.*

The Rev. Mr. Harris, of Massachusetts, has lately favoured the public with some additional observations concerning the design of these works, and concerning the people by whom they were erected.‡ But this gentleman's hypothesis on the latter subject is not, in any essential respect, different from

* From Barton's Philadelphia Medical and Physical Journal, Vol. ii. 1805.

† A Letter on the supposed Fortifications of the Western country, from Bishop Madison, of Virginia, to Dr. Barton. See Transactions of the American Philosophical Society. Vol. vi. Part 1. No. 26.

‡ Journal of a Tour into the Territory North-West of the Alleghaney Mountains, made in the Spring of the year 1803, &c. &c. Boston: 1805.

that

Ancient works in America.

that which the editor of this Journal published, several years ago, viz. first in 1787, and again in 1796.*

'Some time ago, I was in conversation with a Mr. Nathaniel Brittain, living in Mount-Bethel township, on the topic of some of our American antiquities. He told me, that a Mr. Gaston, and another person, whose name I have forgotten, who were formerly his neighbours, had emigrated to some of the western counties of this state (Pennsylvania), and a few years since paid him a visit, when he was informed by them, that, at some salt-lick, which afforded a small quantity of brackish water (I think he said on Gaston's land), under a belief, if they were to dig a hole to some depth into the earth, a greater quantity of salt-water might be acquired, they, accordingly, dug down some depth, when they came to the side of a rock, from whence the water seemed to filter; that on clearing the earth from the rock, they found an old pot (I forgot wether of iron or earth), a shovel, and some tubes, through which the water appeared to have been conveyed.

At another place, at some flat near a river, a man began to dig a well, and after working to some depth, he came to a large flat stone. This he worked out, and found it to cover an old walled well, with water at the bottom.

I should think these to be subjects worthy of the inquiry of your friend B*****, and, if the reports were found to be true, they would make a curious addition to his work on American Antiquites.

Mr. JOHN ARNDT.

Letter to Mr. John Heckewelder, dated Easton (Pennsylvania), March 16th, 1800.

* See Observations on certain parts of Natural History, &c. &c. London: 1787;—and Papers relative to certain American Antiquities, &c. &c. Philadelphia: 1796. 4to.

A

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OF

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AND

THE ARTS.

AUGUST, 1806.

ARTICLE I.

Outline of the principal Inventions by which Timekeepers have been brought to their present Degree of Perfection. Received from a Correspondent.

TWO considerable rewards having been lately granted by the Board of Longitude for improvements in timekeepers,* it becomes interesting to science to point out the progress which the art of chronometry has made for some years, and to establish to the real authors the property of their respective inventions, as far as evidence can be collected from books or credible reports: And this is the more important to truth, because the account of those rewards has been publicly stated in terms so positive, and with pretensions so high, that those unacquainted with the true history might be induced to think that something attempted in vain before, had been actually performed by those claimants; and that machines had

The rewards lately given by the commissioners of Longitude, render the history of chronometrical inventions interesting; —more especially on account of justice.

* To Mr. Arnold for his father's improvements, and to Mr. Earnshaw for his own. See Nicholson's Journal, vol. xiii.

now for the first time been produced, capable of determining the longitude at sea.

General description of machines for measuring time.

The machines which, for centuries, have been commonly used to measure time, consist of a movement, or train of wheels, drawn by a weight or spring, and a regulator, the object of which is to keep the motion of the train within the required degree of uniformity. A continual rotatory motion, which constantly tends to accelerate, is thus corrected by means of an alternate motion; while the power which carries round the movement restores, also, to the regulator the action lost by friction and other causes. The mechanism, by which the two principal parts act on one another, is called the escapement; and this most admirable contrivance may be reckoned the distinguishing characteristic of the modern art of timepiece making. It is not the object of the present inquiry to trace the history of the successive alterations and improvements made in the construction of clocks and watches upon that principle; but briefly to mention such inventions as have been proposed from the middle of the seventeenth century, and have had a direct influence on the progress of chronometry.

The train and the regulating part.

The escapement is the great character of modern clocks.

Great improvement of the train by the remontoir,

A very ingenious invention to improve the movement, or that part of the timepiece which is corrected by the regulator, is the *remontoir*. The action of the movement on the regulator suffers continual alterations, by the inequalities which proceed from the nature of the weight or spring, and by those which occur in the friction of the pivots and teeth of the wheels. In order to prevent those alterations from affecting the regularity of the going of the machine, the usual weight or spring has been employed, only to wind up, at very short intervals of time, a secondary power, which may thus be supposed to be uniform, and is the one which acts immediately on the regulator, by means of the escapement. This contrivance seems to have been executed about 1600,* but Christian Huygens is the first † to whom we are indebted for an explanation of this kind of mechanism, with a weight; and he probably conceived that idea without any previous hints from others. Leibnitz, however, a little afterwards ‡ published the

—which is a contrivance for winding up very frequently that power which acts on the regulator: the action is thus more uniform. Invented 1660 —explained by Huygens, also by Leibnitz.

* Histoire de la Mesure du Temps, par F. Berthoud, vol. ii. p. 41.

† Horologium Oscillatorium, 1673, p. 13.

‡ Philosophical Transactions, 1675, No. 113.

inven-

invention of a spring remontoir, as a new thing; and Dr. Hooke on this occasion* asserted, that he had also known that way ever since A. 1660; but, as he had never declared it to anybody, he could not say that it had been taken from him. The principle of the remontoir has since been adopted by J. Harrison and some others, but it has never become general; and it is supposed that we actually possess more simple means of attaining the same advantages.

—claimed by Hooke:

— and adopted by Harrison.

The greatest step ever made in the improvement of the regulator of timepieces was the application of the pendulum for that purpose; and the merit of the invention cannot be denied to Huygens, who executed and explained it in the most satisfactory manner in his *Horologium Oscillatorium*, though certainly he was not the first who conceived the idea of employing the pendulum to measure time, nor even perhaps the first who thought of attaching it to a clock. The complete practical benefit of the pendulum was not, however, the result of the profound investigations of that great philosopher to procure cycloidal vibrations; and its accomplishment is due to the invention of the anchor and dead-beat escapement, which, permitting only narrow vibrations, obviated the inconveniences observed when the pendulum, suspended on a thread with cheeks to modify the vibrations, was used with the old recoil escapement. Huygens also invented the application of a pendulum with conical or circular motion, and the theory and contrivance, used by him for that purpose, do great honour to his genius; but the success did not answer his expectation, and it does not appear that any attempt has been since made to render those principles useful in practice.

Great improvement *of the regulator* by the application of the *pendulum*:

—ascribed chiefly to Huygens.

Cycloidal vibrations.

Anchor escapement.

Conical pendulum.

Huygens constructed a timekeeper for finding the longitude at sea, which is the first practical attempt of that kind that was attended with any degree of success; though the notion of determining the longitude by that method was proposed so early as the beginning of the seventeenth century by Gemma Frisius,† and followed by Metius and others. A timekeeper of the construction of Huygens was tried by Major Holmes in 1664,

The first timekeeper for sea, by Huygens, with a pendulum.

* Philosophical Transactions, 1675, No. 118.

† De Principiis Astronomiæ et Cosmographiæ, 1530.

who gave a favourable account of it;* and some other trials were afterwards made in France and Holland with various success, which the author † attributes to the bad management of those who had charge of the machines, but which we may venture to ascribe principally to the nature of their construction. Huygens' timekeeper was maintained by a spring, regulated by a pendulum, and the whole was suspended in such a manner as was supposed most proper to procure the indispensable stability.

Timepieces with a pendulum are best for land use:

Timepieces with a pendulum regulator are certainly the most perfect, when they are kept in a fixed situation; and, for that reason, these are the only sort used in astronomical observatories. But external motion is so contrary to the regularity of their performance, that no sea chronometer has been since attempted to be constructed upon that principle. The balance regulator remained, as affording the only method by which the desired uniformity might be obtained in portable machines; and the great improvement made in that regulator, by the addition of a spiral spring, may be considered as one principal cause of the perfection which has been since attained in them. The first invention of attaching a spring, to give to the balance by its elasticity a power which renders the action of this sort of regulator similar to that of gravity in the pendulum, is undoubtedly due to Dr. Hooke, though it is not so clear whether he ever applied it in the shape of a spiral, as has been so long practised since. F. Berthoud, in his *Histoire de la Mesure du Temps*, (vol. i. pp. 134 to 141), gives a body of extracts from several works relative to this subject; and concludes, that Dr. Hooke only applied a straight spring to the balance, and that M. Huygens improved upon that idea, and contrived the spiral spring, which is more favourable to the vibrations of the balance. M. Huygens, indeed, applied in France a balance spring, the account of which has been published in the Philosophical Transactions for 1675, No. 112; but Dr. Hooke, in the *Postscript* to his Description of Helioscopes,‡ asserts that the hint was taken from the experiments he had made in 1664,

—those with a balance for sea.

The balance spring was invented by Robt. Hooke.

Historical remarks, &c. on the balance spring.

* Philosophical Transactions, 1665, vol. i. p. 13.

† Horologium Oscillatorium, 1673, p. 17.

‡ Lectiones Cutlerianæ, 1679.

in

in Gresham College, where *he explained above twenty several ways by which springs might be applied to do the same thing;* and complains of Mr. Oldenbourg, secretary to the Royal Society, for his conduct and supposed partiality to the Dutch philosopher. Mr. Oldenbourg justified himself against that accusation immediately; but it is worthy of remark, that in his account of the matter laid before the public,* while he mentions that Dr. Hooke's application of the spring to the balance had failed repeatedly, and gives several reasons to shew the fairness of his proceedings, no difference of figure is stated by him to distinguish the manner of applying that principle by the two competitors; and, as this difference would have proved a very strong ground of exculpation to Mr. Oldenbourg, his silence upon the subject affords reason to suppose that it did not exist, and that Dr. Hooke had, before Huygens, actually applied, or shewn the method of applying, the balance spring in the shape of a spiral.†

Advantages of jewelling the working parts of timepieces.

In relating the progress of timepiece making, we must not omit mentioning the use of precious stones, particularly rubies, to form the holes in which the pivots of the wheels turn, and the pallets upon which the action of the teeth is exercised. These jewels, by the high polish given to them, reduce the quantity of friction; and, not being liable to the wear which takes place in metal rubbing upon metal, the machine with that addition, not only becomes more durable, but acquires a degree of uniformity in the motion of the pieces, which is very favourable to the regularity of its going.

The inventor of jewelling not well known: it is ascribed to Fatio.

It does not seem easy to discover, with sufficient certainty, the date and author of the application of jewelling to clocks and watches. F. Berthoud says ‡ that the art of perforating rubies

* Philosophical Transactions for 1675, No. 118.

† It is asserted (Supplement to the Encyclopædia Britannica, article watchwork, vol. ii p. 785) that Dr. Hooke first applied the straight spring, and then the cylindrical or helical spring, such as has been employed by the late Mr. Arnold, which he afterwards gave up for the flat spiral spring; but I have not been able to discover the proof of this statement.

‡ Supplement au Traité des Horloges Marines, Introduction, p. viii. note.

(percer

(percer des rubis) was proposed in France, during the regency, by M. Fatio, a Genevese, who, not meeting with encouragement there, came to England, where his secret was received and adopted; and he refers for his authority to H. Sully's *Regle Artificielle du Temps*; but I have not found this account in the edition of this book dated 1717, at Paris; which is the only one to which I have access: the passsage alluded to may probably appear in the edition of 1726, published after the death of the author, by Julien le Roy. Be this as it will, the art of jewelling, of which Harrison availed himself with judgment, has been ever since, and continues to be, a material article in the construction of timekeepers.

—and was since used by Harrison, &c.

Henry Sully laboured in making time-pieces for sea.

The above-mentioned Henry Sully, an Englishman, who settled in France in the beginning of the last century, may be esteemed the first of those great artists who have carried the manufactory of watches to the high degree of perfection, which it still maintains in that country. He laboured, with uncommon skill and perseverance, in making timekeepers for sea on a new plan, of which he made a trial at Bordeaux in 1726; but, the author dying immediately afterwards, his construction remained useless, and has never been copied or improved since that time. Indeed the method, according to which he intended to effect the isochronism, by means of a lever suspended on a thread, or flexible wire passing over curved cheeks, to modify the vibrations of a vertical balance, though not the same in principle, is so similar in its inconveniencies to Huygens' pendulum, that we cannot wonder if it has failed before, nor expect that it will ever be useful for portable machines in future.*

His regulating part has never since been used, and is exceptionable.

Sully made many improvements, and in particular he introduced friction rollers

Sully made and published in his works a variety of ideas and inquiries; but the principal practical improvement which he seems to have at first proposed, and has since his time been frequently employed in timepieces, particularly in France, is the application of rollers to diminish the friction of pivots in timekeepers.

John Harrison obtained the reward of the

Before Sully's death, John Harrison had probably made some progress in his labours for the improvement of time-

* Sully published an explanation of his timekeeper, in a book intituled, Description abregee d' une Horloge de nouvelle Invention, pour la juste mesure du Temps en Mer. 1726.

pieces,

pieces. That extraordinary man, having produced the first machines, which, upon repeated trials, met with success, to the extent required for the great reward offered by parliament, must be reckoned the father of modern chronometry; and his long and active career has proved extremely useful by stimulating with so bright an example other artists to similar endeavours. The principles of Mr. Harrison's watches are well known; and, as most parts of his construction have been superseded by more simple contrivances, we shall only mention the principal inventions of which he appears to be the author, and which are still used by the watchmakers of the present day. British parliament for his timepiece.

The going fusee is one, among those inventions,* which has proved the most generally useful in practice. By this simple mechanism, the main spring, while the watch is going, acts on an intermediate short spring, which Harrison calls the secondary spring, and is constantly kept bent to a certain tension by the former; and, when the watch is winding up, and the principal spring ceases to act, the secondary spring being placed in a ratchet wheel, which is hindered from retrograding by a click, continues the motion without alteration. Other contrivances have been proposed, and executed, to make timepieces go while winding up; but none which, like this, combines the advantage of simplicity, and the property of providing a supplementary power, which is equal to that of the main spring when its action ceases. And it is to be observed, that the utility of the going fusee, which has induced manufacturers to introduce it into all good watches, is peculiarly important in those timepieces which have not the power of setting themselves in motion, as is the case with the best modern escapements. Harrison invented the *going fusee.* Description of this mechanism: —its advantages: —and great utility in chronometers.

Harrison invented also a compensation for the effects of heat and cold, which at the time was perfectly new, and has led to the improvements made afterwards in that essential requisite of timekeepers. Harrison's compensation for the effects of heat and cold.

* We have heard that this piece of mechanism was first invented by a maker of kitchen jacks; and, if so, it is not impossible but that Harrison might have benefited by this contrivance, before his application of it to timekeepers.

The

The balance and its spring are more affected by temperature than the pendulum.

The alterations to which the length of the pendulum is liable by the different degrees of heat and cold, affect the going of clocks with that sort of regulator; and watches, with a balance, are still more subject to irregularity from that source; because not only the balance expands or contracts, according to the rise or fall of the thermometer, but the regulating spring itself, while it suffers similar changes, becomes weaker or stronger; so that, from these causes, a timepiece must go slower or faster in too great a proportion to be overlooked or neglected.

Graham first compensated this in the pendulum by two metals.

Graham* is the first who thought of applying two metals of different expansibility, to correct the errors proceeding from the variation of temperature in a pendulum; but as he seemed to have had in view to effect it immediately, without the aid of mechanism, he was obliged to fix on steel and mercury, these being the metals which offered to him the greatest difference of dilatation and contraction.

Harrison's gridiron pendulum.

Harrison, by multiplying the bars, increased the total length of the two metals acting on one another, without exceeding the limits of the pendulum; and thereby produced a sufficient compensation with brass and steel in the compound, or gridiron pendulum, which has been almost universally adopted ever since.

His expansion curb for a balance spring:

This contrivance could not be easily applied to balances; but Harrison, following still the principle of the different expansibility of metals, applied it in a manner which had not been thought of before, and made it act on the spiral spring, in order to produce the desired compensation in the regulator. This method is described

—composed of two plates of different metals rivetted together.

as follows:* "The thermometer kirb is composed of two "thin plates of brass and steel rivetted together in several "places, which, by the greater expansion of brass than steel "by heat, and contraction by cold, becomes convex on the "brass side in hot weather, and convex on the steel side in "cold weather; whence, one end being fixed, the other end "obtains a motion corresponding with the changes of heat "and cold, and the two pins at the end, between which the "balance spring passes, and which it touches alternately as "the spring bends and unbends itself, will shorten or lengthen "the spring, as the change of heat and cold would otherwise

* Philosophical Transactions, 1726.

* Principles of Mr. Harrison's Timekeeper, p. xii. notes.

"requi-

"require to be done by the hand in the manner used for regu-
"lating a common watch."

This kind of compensation has been since applied in other ways; but the method, in general, is liable to some material objections, on account of its altering the length of the balance spring, and the difficulty, perhaps impossibility, of effecting with it an accurate adjustment. Mr. Harrison himself was aware of these objections, and expressed the well known observation,* *that if the provision for heat and cold could properly be in the balance itself, --- the watch--- would then perform to a few seconds in a year.* By the watch, Mr. Harrison meant his own *longitude timekeeper;* and we have now sufficient reason to believe, that he overrated its merits, though the construction had been improved with the desired invention, upon which he set so great a value; but that assertion, which has been repeated † and strengthened by men of learning and good judges of mechanics, shews, at least, the importance of the desideratum, which seemed to be still wanting to complete the perfection of chronometry.

Objections to compensation on the spring.

Harrison proposed to put the compensation in the balance itself, but did not contrive any mean of effecting it.

Pierre le Roy, eldest son and successor to Julien le Roy, the companion and friend of Henry Sully, had the merit of accomplishing that great desideratum. In the chronometer, which was presented to the king of France the 5th August 1766, and obtained the prize of the Academy of Sciences of Paris the 31st of the same month, that author executed a compensation in the balance, which he has fully explained in his description of that machine.‡ This compensation is composed (Fig 1, Pl.VII.) of two thermometers, *t* K *t* A K, of mercury and spirits of wine, made each in the form of a parallelogram, except in the upper branch, which bears the

Peter le Roy completely effected this purpose at Paris, in 1766.

—by mercurial thermometers in the balance of a chronometer, which obtained the academical prize of that year.

* A Description concerning such Mechanism as will afford a nice, or true Mensuration of time, &c. By James Harrison, 1775, p. 103.

† See Mr. Ludlam's letter to Dr. Maskelyne, in the Report from the committee to which Mr. Mudge's petition was referred, pp. 96 and 97.

‡ Mémoire sur la meilleure Maniére de mesurer le Temps en Mer, qui a remporté le Prix double au Jugement de l'Academie Royale des Sciences. Contenant la Description de la Montre à Longitudes, presentée à sa Majesté le 5 Août, 1766. Par M. le Roy, Horloger du Roi. pp. 41 to 44. This Memoir accompanies the account of Cassini's voyage in 1768, published in 1770.

ball

ball containing the spirits of wine, and is a little bent downwards; the mercury is in the lower part, and the vertical branch of the tube, A K, is open at the upper end. These two thermometers are placed opposite one another, the axis of the balance being in the same plane with the centrical lines of the tubes; and the thermometers and balance are solidly attached together, and form a well poised and steady regulator. At the middle temperature of the atmosphere, the quicksilver stands at K A *t* K; but, when an increase of heat occurs, the alcohol, by its expansion, forces the mercury from the exterior branch, *t* K *t*, towards A K, and a portion of the mass of the regulator contracts by approaching the centre. On the contrary, if the variation consists of an additional degree of cold, the mercury moves towards the exterior branch, and the weight towards the circumference of the balance becomes greater. Thus, if the thermometers are well adjusted, the effects of all the changes of temperature in the balance will be compensated, and the regulator will act with the same uniformity as if its figure were not liable to such alterations.

Peter le Roy did not know of Harrison's expansion curb when he made his thermometrical balance.

P. le Roy invented and executed this method of compensation before he became acquainted with Harrison's contrivance of the compound metallic thermometer; and he avows,*

* Mémoire sur la meilleure Manière de mesurer le Temps en Mer, &c. p. 56 and 57.—This being an interesting point, I shall subjoin a literal copy of the passage of P. le Roy's memoir, in which that author states his manner of making the compensation balance.

——quand j' ai cherché à compenser l'effect du chaud et du froid par des lames de cuivre et d' acier, rivées ensemble, comme M. Harrison, j' ai tenté, non de changer la longueur du spiral, mais de faire approcher ou éloigner par ce moyen, du centre du balancier, une partie considerable de sa circonférance. Pour cela, j' ai employé un balancier (fig. 2) composé de deux demi-cercles formés chacun d' une lame de cuivre et d' une d' acier, réunies comme dans le thermomètre de M. Harrison.

L' effect répondoit assez a mes intentions, j' ai même observé au moyen de l' index *i*, conduit par des semblables lames *ll* (fig. 3), que par le froid et le chaud, le mouvement de ces lames suivoit assez exactement la marche du thermomètre : il en résultoit une compensation de la chaleur et du froid, dont on pouvoit augmenter ou diminuer l' effect a volonté, en mettant plus ou moins de masse aux extremités des ces demi-cercles.——

that

that, if he had been in possession of it before he began this part of his labours, he would probably have made use of the same principle, but applying it in a very different manner, *videlicet*, to enlarge by cold, and contract by heat, the circumference of the balance, preserving the spring untouched. He proceeds to state how a balance may be constructed with compound metallic pieces, to effect the compensation, and gives a figure to elucidate his plan, of which Fig. 2, Pl. VII. is a copy; adding that, according to this method, the balance may be easily adjusted by means of small balls, or weights, which are to be attached to the ends of the metallic curves. The curves, being made of two plates of different metals, with the one most affected by variations of temperature at the outside, it is clear, that heat will move the balls placed at the extremities towards the centre, and that cold will move them in the contrary direction; producing, by this contraction or expansion, the same sort of compensation as that of the mercurial thermometer explained before. P. le Roy did not remain satisfied with the simple suggestion of this contrivance, but actually put it in practice; and employing a register, such as is represented in Fig. 3, ascertained by experiment that the mechanism performed well, and corresponded pretty exactly with the other thermometers. After all these investigations, he concludes, by giving the preference to his own mercurial thermometer, because he thinks it more accurate and steady, as well as more fit to secure, in all temperatures, an uniformity of weight to the whole circumference of the regulator, than the compound metallic balance; and, under those points of comparison, he may be right in his choice; but certainly the last thermometer seems better adapted for small and portable machines, and has answered, during repeated trials, so well, that we must believe it fully entitled to the favour which it has obtained in practice.

—but he gives a drawing of a balance of his invention on the principle of that curb; the arms or arcs of which act by flexure and are adjusted by moveable weights.

He proved the effect of such arms by experiment;

—but gave the preference to mercurial compensation.

The compensations in the balance, applied at present to the best chronometers, are essentially the same as that so well explained and published so long ago by P. le Roy; but, in this country, the invention has been generally ascribed to the late Mr. Arnold, who, in 1782, took a patent for it; and a degree of merit has been attributed to him on that account, proportioned to the supposed difficulty of the desideratum expressed by

Peter le Roy's invention is the same as is now used: but though so publicly declared in the face of the French government and academy in 1766, Arnold took an

English patent for it in 1782.

by Mr. Harrison. It would be hard, indeed, to find a similar instance of an invention, the first author of which may be so clearly ascertained, and from which a second inventor, if Arnold can be allowed as such, has derived so much credit.

Concerning the escapement.

We have not yet taken any notice of the improvements made in the escapement; because, after all the plans proposed for this most essential part of chronometers, the principle of what is called the detached escapement, is the only one now used; and, being established upon long experience, seems to merit the preference given to it over all the constructions proposed till now. We shall content ourselves with stating in a general manner the beginning and progress of that escapement.

Explanation of the bad effects of the maintaining power when connected constantly with the regulator

In all the escapements known till the middle of the last century, the escape wheel was in continual contact with the pallets belonging to the axis of the balance wheel; and the friction, proceeding from this circumstance, may be considered as a principal source of irregularity in the going of the watches. Suppose, that a regulator should be made so perfect as to be

A balance, tho' itself perfect, would not measure time if so connected

exactly isochronal, while vibrating in a free situation: that advantage would be diminished or lost as soon as it was placed in connection with a train of wheels; and the errors would be more or less, according to the nature and quantity of friction in the escapement. It would be, therefore, extremely useful to secure to the regulator a perfect liberty of vibration, except during the short intervals of time which may be necessary for the action of the escape wheel, to give it a new impulse. This

Peter le Roy also contrived the first detached escapement in 1748.

ingenious idea was also started by P. le Roy, and carried into execution by the same artist, in a model which he presented in 1748 to the Academy of Sciences of Paris, and is described in the collection of machines approved by that society for the same year.*

Description of Le Roy's escapement.

That escapement is represented in Fig. 4, Pl. VII.; G H is the escape wheel, the profile of which is shewn at *gh*, and TV the balance. The curved pallet, AE, is affixed to the axis under the balance; and on the same axis, but above the balance, and under the spiral spring, is attached the half-cylinder, C I, the end, C, of which is round, and placed in such a manner, that

* Vol. vii. No. 481, p. 385, intituled, Echappement à détents, inventé par M. le Roy, Fils aîné, Horloger.

a line

a line drawn from that point to the centre of the balance would form with the curve, A E, a mixed angle of about 80°. Q P G is an angular lever, turning on pivots at P, the branch of which, P X, when in contact with one of the teeth of the escape wheel, stops its motion; and the spring, R M, tends to keep it disengaged against the pin K. The effect of this construction is as follows:

The effect or action of Le Roy's escapement

An escape wheel is kept in repose by a lever detent.

The balance unlocks the detent and receives an impulse or stroke on a pallet thro' a part of every second vibration, and during great part of its course it is free and detached.

Suppose the action of the tooth, D, of the escape wheel to give tension to the spiral spring, and make the balance move round an arc more or less extensive; the end, C, of the half-cylinder or roller, at the same time, pushes the angular lever, Q P X, by its end, Q, making the end of the branch, P Q, rest upon the circumference of I C; and when the tooth, D, quits the pallet, A E, the tooth, G, comes in contact with the end, X, which then continues the motion of the angular piece, Q M X, till the tooth and the end, X, are completely engaged and remain at rest. In this situation, no part of the branch, P Q, touches the round part, C I; and the balance proceeds in its vibration as if it were insulated. Its velocity being soon destroyed, by giving tension to the spiral spring, the reaction of this spring brings it back with an accelerated motion, and at the second vibration the pallet, A E, comes into contact with the escape wheel, and by its action on the tooth next following D, causes the wheel to recoil, by a space, which may be equal to half the distance between two teeth. The branch, X, of the lever, in consequence of that motion, becomes disengaged out of the teeth of the wheel, and, by the action of the spring, R M, falls against the pin, K; after which, the escape wheel gives a new impulse to the balance, pushing the pallet, D E, in the contrary direction; and the vibrations proceed alternately, in the manner explained before.

It has been asserted, but not proved, that Dutertre invented a detached escapement earlier than Le Roy.

While we give this escapement as the first of that kind ever invented, it is proper to remark, that according to report,* Jean Baptiste Dutertre, the elder, a very skilful watchmaker at Paris, in the beginning of the last century, had thought of, or actually contrived, a detached escapement; but as his inven-

* Traité des Horloges Marines, par F. Berthoud, 1773, p. 97. Etrennes Chronométriques pour l'annee, 1759, par P. le Roy, p. 98.

tion,

tion, if true, was never published, nor even mentioned, till after other persons had produced their labours on the subject, we must ascribe to P. le Roy the original idea, as well as the first execution, of that ingenious construction; and in this opinion we are strengthened, by observing that the model of 1748 was received by the Academy of Sciences, both as new and advantageous; and that some years after, when a new detent escapement, by M. Platier, was submitted to the same society, the commissaries, M. Montigni, and M. Vaucanson, who examined it, and were certainly the most competent judges in such a matter, expressly declare in their report that M. le Roy was the first who ever thought of this sort of escapement.*

Description of another improved escapement by Peter le Roy.

P. le Roy contrived also another detached escapement, which is an improvement upon the former, but according to the same principle; and he applied it to the timekeeper which was presented to the Academy of Sciences in 1766, and afterwards tried at sea by order of the French government. In that construction, the escape wheel (Fig. 5, 6, and 7, Pl. VIII.) is made with teeth which are very light and at a considerable distance from one another, it being meant that their power should proceed from the length of the lever; and they act on the balance by means of a pallet, *p*, adapted to the circumference of the latter. The action of the escape wheel, except the time which is requisite to restore to the balance the power lost, once in every two vibrations, is suspended by a compound detent, D *c* H *e* F, (Fig. 6, 7, and 9), very different from the mechanism employed in the former escapement.

Its effect or action described.

The escape wheel being stopped by the detent at D (Fig. 6), the balance vibrates first from A to *i*, and afterwards from *i* to A. On this return, the balance, by means of a pin, placed on its upper face at *i*, pushes the arm or lever, F H; and then the arm, D H, gets out of the circumference of the wheel, and the arm, *e* H, coming into action, stops the following radius, K *r*, of the wheel which falls upon it. This disposition of the respective pieces, which Le Roy calls the *preparation*, is represented in Fig. 7. In the following vibration, the escape wheel restores to the balance its lost power, by means of the pallet, *p*, in this man-

* Observations sur la Physique, par M. l'Abbé Rozier, t. iii, part. i. Juin, 1774.

ner:

ner: A pin, which is placed as the preceding one, but in the under face or plane of the balance, pushing the arm C H, gets the arm, *e* H, out of the circumference of the wheel, and introduces D H; so that when the pallet, *p*, arrives at F, the wheel being free, the radius, F *r*, gives a new impulse to the balance, and impels the pallet, *p*, till it is stopped by the arm, D, of the detent, as in Fig. 6.

To prevent the detent from being displaced by the effect of external motion, a circular curb, *i* A, *i* A (Fig. 5, 6, and 7), has been adapted to the circumference of the balance, near each pin, which disengages the detent: but, the arms of the detent can only touch the corresponding curbs, in consequence of the most violent shakes.

The improved escapement of Le Roy's differs from his first. 1st.—It has less connection and no recoil in unlocking the detent.

The construction of this escapement principally differs from that of 1748 in these three points: 1st.—In the last escapement, the second vibration, or return, is permitted to be completed, and it is not till the balance comes again to move in the first direction that a new impulse is given to it, in the middle of the whole arc of vibration; while, in the former, the free return of the balance proceeds no farther than the place where it received the first impulse, where a new action is opposed to it. 2d.—In the second escapement, the pallet is situated near the outer circumference of the balance, with a view to render the impulse upon it more favourable to circular motion, without a consequent action on the pivots; while in the former escapement the pallet or edge of the half-cylinder is near the centre. 3d.—The mechanism of the detent in the new escapement, having no springs, is also different from that of the other, which depended upon that description of power. P. le Roy was led to contrive the new detent, because he wished to avoid the inconveniences arising from the use of the springs for that purpose, inconveniences which are considerable in his opinion, on account of the loss of power which takes place if the springs are strong, and of the uncertainty of their performance if they are weak.

2d.—The action affects the balance pivots less, because it is given at the circumference.

3d.—The detent has no spring.

All the modern escapements, which have been much used, are essentially the same as those of Le Roy's.

From the two preceding escapements of P. le Roy, are derived, without material improvements, (unless the spring detent or locking spring should be esteemed one), all the detached escapements which have been executed, to any considerable number, from that to the present time.

About

Mention of the detached escapement of Mudge; contrived in 1755, and different from that of Le Roy.

About the year 1755, according to Count de Bruhl, the late Mr. Thomas Mudge invented a detached escapement, and applied it to a watch which he made for the King of Spain, Ferdinand VI. This is the same escapement that was used by the late Josiah Emery in his chronometers, some of which have gone very well. It differs from the constructions which we have already explained, both in the detent and in the communication of the impulse, which in this mechanism takes place at every vibration; but the date will not suffer us to consider it as the first invention of the detached escapement.

The latter escapement of Mudge is not, properly speaking, detached, but it winds up springs, connected with the balance, every vibration.

Our design will not lead us to be more particular respecting the invention of another escapement applied by Mr. Mudge to the chronometers, for which he was rewarded by parliament. We shall merely observe, that the principle of that escapement is not free, and it could only be ranked among the detached escapements, in consequence of giving to that appellation a different sense from the usual meaning attached to the term. The peculiar mechanism of that machine consists in a kind of double remontoir, which is placed within the escapement, or beyond the whole of the train, and not antecedent to the escape wheel, as in the remontoirs of Huygens and Leibnitz; consequently the maintaining power of the timekeeper, through the train of wheels, acts only during a short portion of the vibration; but then the remontoir, or secondary power, which is composed of two springs, by means of their alternate winding and unlocking, is almost in constant action upon the balance. The author himself declared,* that this escapement could not, with propriety, be called detached; and it is rather surprizing, that F. Berthoud should,† notwithstanding, have placed it in that class, in the account he has given of it. His opinion of this construction seems, however, well founded; and we agree with him in thinking, that it is too complex, and requires too nice an execution, ever to become generally useful.

* Letters of Mr. Mudge, attached to the Description of his Timekeeper, 1799, p. 159. This escapement is also described in our Journal, quarto series, vol. ii.; and in the Phil. Trans. for 1794.

† Histoire de la Mesure du Temps. vol. ii. p. 44.

In

In the *Histoire de la Mesure du Temps*,* the invention of the detached escapement is ascribed to three different persons, who accomplished it separately, and upon distinct principles, without any communication with one another; P. le Roy, T. Mudge, and the author, F. Berthoud, himself. We have already noticed the labours of the two former, and it remains for us to state what right the last may appear to have to the honour he claims. The title of P. le Roy to priority cannot be invalidated, and what F. Berthoud has written upon the subject is of so much later date, and his ideas seem so closely derived from those of the preceding author, that it would not be fair to grant him the share he assumes in point of originality on this occasion. Berthoud published his *Essai sur l'Horlogerie* in 1763; but not one word is to be found in it, respecting the principle of the detached escapement, though P. le Roy had made it publicly known fifteen years before. This silence or omission in a treatise of two volumes in quarto, respecting a construction so remarkable, appears to be inconsistent with the subsequent pretensions of the author; and this circumstance, at least, must prevent our supposing that he had applied with much attention to the subject. It was not till 1773 that Berthoud took notice of the detached escapement, in his *Traité des Horloges Marines*, where he describes several constructions of that kind, giving them as the result of his own inquiries, upon which he says he had been engaged, ever since the beginning of his labours upon timekeepers for sea. In that work, Berthoud also states,† that in 1754 he made a model of a detached escapement, which he shewed to M. Camus, to be presented to the Academy of Sciences, when that learned gentleman told him, that M. Dutertre, the father, had had the same idea before.

Berthoud ascribes the invention of the detached escapement to P. le Roy, Mudge, and *himself*.

Reasons, shewing that Berthoud has no claim to the invention.

He took no notice of any detached escapement till 1773.

It might have been of service to him, to have been at the same time reminded of what P. le Roy had done in the same way; for the name of that artist is, in the whole, so studiously avoided, as to raise a suspicion of want of candour in the writer, who, on several occasions, was the declared rival of that great mechanic. It may also be remarked, that F. Berthoud was, even at that time, so far from judging properly of the detached escapement, that he concludes his book on marine time-

Berthoud avoided mentioning Peter le Roy.

* pp. 24 and 25. † p. 97, note b.

—and even then preferred the dead beat with ruby pallets to the free escapement.

keepers * with an article entitled, *De la préférence que l'on doit donner à l'échappement à repos à palettes de rubis, sur celui à vibrations libres ; constatée par des experiences décisives ;* while Le Roy, from the conviction of the accuracy of his construction, ventured to assert (in his *Précis des differentes Recherches*, &c. p. 37), that chronometers would, in future, be made, according to those principles, without material variations. Long experience has already justified M. Le Roy's ideas, and established, beyond doubt, the great advantages of the detached escapement; and F. Berthoud, in his history, published in 1802, has, at last, done something like justice to that great watchmaker, and his construction; but, at the same time, has associated himself to the honour of the invention of the detached escapement, in terms, which, so far from appearing well established, are rather in contradiction to the evidence afforded by an attentive comparison of his preceding works.

Peter le Roy asserted the superiority of the latter; and Berthoud, in 1802, admits his claim, but demands to share it

Of the different constructions of the detached escapement published by F. Berthoud, which indeed differ from one another merely in the contrivance of the detent, and are all made to act with springs, we shall only notice the kind which has been the most generally used both in Great Britain and France: Fig. 10, Plate IX. represents this escapement, as copied from the *Traité des Horloges Marines* 1773, Fig. 5, Plate XIX. The escape wheel, A, is stopped by the arm, B *d*, of the detent, *f* B *d*, while the balance vibrates in two directions: the detent moves on pivots, and is pressed by the spring, *a*. C is a circle, or wheel, attached to the axis of the balance, but of smaller dimensions, and has a pallet, *c*; which, when it turns from *c* towards *e*, acts on the arm, *f*, and disengages the escape wheel. At the same moment the pallet, *g*, which is placed within the thickness of the circle, C, and stands as high as the escape wheel, receives an impulse from the tooth, *i*. During this action, the pallet, *c*, quits the arm, *f*; and the detent, pressed by its spring, drops into the escape wheel, to meet the succeeding tooth, and keeps it at rest, after the communication of power is completed. The tooth, *i*, is, at this time of stopping, disengaged out of the pallet; and the balance, being free, finishes its vibration. When the balance returns in the direction from *e* to C, the pallet, *c*, acts on the back of the arm, *f*; but this part is flexible, and forms an in-

The construction of the detached escapement of Berthoud, which has been most frequently used. Description and drawing. The detent has a back spring, and there is no recoil

* Traite des Horloges Marines, p. 576.

clined

clined plane, over which the pin slides with little resistance, and without disturbing the detent. At the next vibration, the detent is disengaged, a new impulse is communicated as before; and the actions, already explained, continue to be performed in succession.

F. Berthoud thinks that construction the simplest and safest in practice, and gives it again as such in his later works,* with a little alteration in the arrangement of the pieces, as represented in Fig. 11, which is copied from Fig. 9, Plate XIII. of the *Histoire de la Mesure du Temps.*† In this construction, Berthoud attached a very delicate spring to the outside of the arm, *n*; which, projecting a little beyond the extremity, serves for the purpose of yielding in one vibration, and unlocking the detent in the next, instead of rendering the arm itself flexible. The additional arm, *k*, is only intended to stop the wheel, when the balance is taken out; and the other parts of the figure, after what has been said in the preceding paragraph, need no farther explanation.

The same escapement a little varied by Berthoud. It has the back spring and an unlocking spring.

In the construction of the detached escapement adopted by Berthoud, the impulse of the escape wheel is communicated to the balance, not on the circumference of the balance, as in Le Roy's second escapement, but on a circle, or pallet, situated considerably nearer the axis, as in the former escapement. The detent also acts by means of springs, as in Le Roy's first plan, and not by the sort of mechanism which that author thought preferable. On these two points, the practice of succeeding watchmakers has continued in conformity with those two retrograde steps; but, whether on account of real advantages, or merely from the greater facility of execution, need not, on the present occasion, be discussed.

Observation that Berthoud has not given his impulse to the balance rim, nor rejected springs, as Le Roy did.

We come now to the constructions used at present, by the English watchmakers; and shall begin with that of the late Mr. Arnold, as described in his statement, presented by his son to the Board of Longitude.

English constructions of free escapements

* De la Mesure du Temps, ou Supplément au Traité de Horloges Marines, &c. 1787, chap. iv.—Histoire de la Mesure du Temps, 1802, vol. ii. pp. 32 and 33.

† See also Fig. 8, Plate IV. of the Supplément au Traité des Horloges Marines.

Description of Arnold's escapement. He takes the wheel, detent, and pallet of Le Roy's first escapement, and unlocks, by a pallet, without recoil, as in Le Roy's second escapement; but the form and dimensions of the parts are very different, and the teeth of the wheel have a peculiar form; the detent moves by a spring joint instead of pivots, and it has an unlocking spring.

The teeth of the escape wheel (Fig. 12, Plate IX.) are of a cycloidal shape,* in the face part which is intended for action, the section of which, with those of the two other sides, form a sort of mixed triangle. B B *d* represents the detent, which is formed of a flexible piece or spring, bending between C and N; and in the part N B *d*, which is stronger than the other, is fixed the locking pallet, *a*, opposite an adjusting screw F. The pallet, projecting below the spring detent, locks upon the interior angle of the tooth; suspending the motion of the escape wheel, and leaving the balance to vibrate free, as pointed out in the preceding escapements. The action of the spring detent (for the joint of the detent is itself a spring) presses the locking pallet against the screw, F, except at the time of unlocking the wheel. A very delicate spring, N *e*, called the discharging, or unlocking spring (and also the tender spring), is attached by one end, N, to the spring detent, CBNB *a*; and, passing under the adjusting screw, F, extends a little beyond the extremity, *d*, of the detent itself. HHH is a circular piece attached to the axis of the balance and, *o*, the discharging pallet. This pallet, when the balance is in motion from *c* to *d*, presses against the end of the discharging spring, *n e*; and, carrying it together with the locking spring, B B *d*, disengages the locking piece, *a*, out of the internal angle of the tooth, with which it was in contact; and the escape wheel then communicates a new power to the balance, by its impulse on a pallet, *m*, which is fixed, or set, in the aperture of the circular piece. As soon as this is done, the spring detent, or locking spring, falls back to its position against the adjusting screw, F; and the pallet, by receiving or intercepting the next tooth, stops the motion of the escape wheel. When the balance returns from *d* to *c*, the unlocking pallet acts again on the extremity of the discharging spring, but, this being very delicate, gives way without disturbing the detent or locking spring; and the balance, after suffering a trifling degree of resistance by that contact, continues its free vibrations. At the next vibration, the unlocking takes

* As the descriptions of the escapements for which Arnold and Earnshaw have been rewarded, are not of considerable length in the present interesting communication, I have re-engraved the sketches, instead of referring to Plates 13, vol. xiii. and 2, 3, vol. xiv. of our Journal, where the full descriptions are given.—W. N.

place;

place; and the action of the escapement proceeds successively, as explained before.

The detached escapement used by Mr. Earnshaw is represented in Fig. 15, which is taken from his statement, presented to the Board of Longitude. This escapement is similar to that of Arnold's, already described, except in small variations, which will be easily perceived, on a comparison of the two figures. It is besides asserted, and it appears probable from every circumstance relative to these constructions, that the late Mr. Arnold had made use of this form of escapement long before Mr. Earnshaw, but that he laid it aside, in order to adopt the escapement with cycloidal teeth, which he esteemed far preferable. In the escapement we are now considering, the escape wheel is shaped as appears in the figure; and, on the inspection of this representation, it will be easily observed, that the teeth presenting a right line, and escaping by a sharp point, their action cannot be so smooth, and the wear of the whole must be greater, than in the construction with protuberant cycloidal teeth. The detent is of the same kind as the other, and only differs from it, in being stopped by the inside of the head of the adjusting screw, instead of the extremity of the screw itself, and unlocking outwards, and not towards the centre.

Earnshaw's escapement, —is in effect the same, and in form nearly the same as Arnold's, —who used it before him: —it is more subject to wear.

The two constructions, which may be considered as the same, differ from the French detached escapements, such as those of F. Berthoud, which we have already explained, in the detent. In the new detent, the pivots are abolished, and the centre of motion is established in the locking piece itself; which, for that purpose, is made flexible near the extremity by which it is fixed. The elasticity of the detent, or locking piece, supplies also the office of the auxiliary spring placed at a or u (Fig. 9 and 10), or the spiral spring, which has been sometimes applied to the axis of the pivots, to keep the detent in the proper situation.

The escapements adopted by Arnold and Earnshaw are not different from those of the French, except in the spring joint of the detent instead of pivots.

The pivots of the old detent are so slender, that its performance cannot be supposed subject to any considerable degree of friction; and watches, with that kind of detent, have been known to go very well. Some able artists, upon that account, think, that the new detent is only preferable to the other, because it saves work, and is less expensive; but while the spring detent is allowed to perform as well, if not better, than the

Probability that the spring detent may be superior to that with pivots, in cheapness only. —but it is as good, if not better, and

of the voyage to Jamaica, added for the same purpose the cycloidal pin, to regulate the balance spring; but this method of adjustment never appeared satisfactory or certain. P. le Roy, in his *Mémoire sur la meilleure Manière de mesurer le Temps en Mer*, rewarded in 1766, first announced distinctly the discovery of a general principle, proper to produce the isochronism, by means of the balance spring, which is expressed as follows: *That in every spring sufficiently long, a certain portion of it will be isochronal, whether long or short; that the length of this portion being found, if it be lessened, the long vibrations will be quicker than the short ones; and that on the contrary, if the length be increased, the small arcs will be performed in less time than the great arcs.* This important property of the spring, enabled P. le Roy to bring to a happy issue his labours for the improvement of chronometry; and the art is indebted to him for the practical utility of that discovery, as much as for the invention of the detached escapement.

Peter le Roy first distinctly announced that isochronism is producible by the spring.

He asserted, that a given length of spring will be isochronal; a less length will have the wider vibrations quicker, and a greater length the narrower vibrations quicker.

F. Berthoud appears again on this occasion as a rival of P. le Roy, and arrogates to himself the honour of the discovery of the isochronism, by means of the balance spring; but his proofs are unsatisfactory, and the dates of the respective labours of these authors are too well established to admit of any presumption favourable to his pretensions. F. Berthoud did not publish any researches, or even ideas, upon the subject, till 1773, which is the date of the *Traité des Horloges Marines*; where (Premiere Partie, chap. iv. art. ii.), with the same want of candour, as we have already remarked in the case of the detached escapement, he gives a very minute detail of his own inquiries and experiments, without even once mentioning those of his predecessor. When that author was, afterwards, obliged to take notice* of the accusations published against him by P. le Roy (in his *Précis des differentes Recherches qui ont été faites depuis plus de quarante Ans, pour parvenir à resoudre le fameux Problème des Longitudes par le Secours de l'Horlogerie)*, he refers to a passage of the *Essai sur l'Horlogerie* (vol. i. p. 168), to shew that he had, in 1763, laid the foundation of his discovery; but that passage signifies nothing, and can

F. Berthoud claims the discovery,

—but without any proof or probability.

Berthoud's defence in answer to le Roy,

—is unsatisfactory.

* In the Eclaircissements, &c servant de suite à l'Essai sur l'Horlogerie, et au Traité des Horloges Marines, 1775.

only

only prove that he intended to make experiments on the wide and narrow vibrations of the balance; and falls short of the hints contained in Dr. Hooke's Postscript, which we have already quoted. But though we cannot allow Berthoud the credit of originality, it is impossible to deny that his researches possess an eminent share of merit; and we have no doubt but that their publication has been of great service to artists, in that essential part of the construction of timekeepers. Berthoud found that the spiral spring, in order to be isochronal, must have an ascending force in arithmetical progression, and that this property may be effected, not only by the length of the spring, but by the number of coils, and the tapering or decreasing thickness from the centre to the extremity, &c. He adds besides the proportions of the tapering in many springs, which he had actually tried, and gives minute accounts of the experiments made with them in several timekeepers.

But the researches and labours of Berthoud are of great value, and importance

He discovered the law of the force of an isochronal spring, and the causes which affect it.

The late Mr. Arnold applied to the balance the cylindrical or helical spring, which had been employed long before to a variety of purposes instead of the spiral, which had been constantly used in watches since the time of Dr. Hooke and M. Huygens.* This is one of the articles of his patent of 1782, and the specification is contained in these words: "*The incurvating of the ends of the helical spring is attended with the property of rendering all the vibrations of equal duration, because the figure is always similar to itself.*" Whence it would appear, that provided the spring be made of that form, the vibrations cannot fail to be isochronal; but experience is contrary to that notion, and artists are obliged to attend to a variety of circumstances in the application of the helical, as well as that of the spiral spring. Mr. Arnold was asked by the Committee of the House of Commons, to which the petition of Mr. Mudge was referred,† "*What objections are there against the common spiral spring?*" To which he answered, "*That it is never a spiral, but when it is at rest; for the*

Arnold applied the cylindrical or helical spring to chronometers, (asserting its superiority to the spiral).

He affirms that it is isochronal, if the fixed end be bended inwards:—but this is not the fact.

* One of Harrison's watches had an helical spring. See Earnshaw's Disclosure; who says the machine is now at Greenwich. W. N.

† Report from the Select Committee, &c. ordered to be printed, 11th June, 1793, p. 81

 "*instant*

"*instant it begins to move, it assumes a figure that is not a* "*perfect spiral, both when the spring opens and closes, by the* "*contrary vibrations*." But this explanation is not conclusive, and requires some modifications. A spring, of whatever form it may be, only acts when its figure suffers an alteration, and for that reason a relative change must take place, as well in the cylindrical as the spiral, or any other spring. What is the shape most favourable to isochronism, is another question to be decided by very delicate experiments, which we do not know to have been ever made. At present, some watchmakers think that the helical spring does not possess any advantage with regard to that property; but as the opinion of other persons is in the affirmative, while all the manufacturers, as far as our knowledge goes, agree in considering the cylindrical form as more easily managed than the other, its application seems entitled to the merit of a practical improvement.

What may be the best figure of a balance spring?

That application* is ascribed by common report to Mr. Arnold, and we see no reasonable ground to dispute it to him, except the instance before noted, and the evidence of the late Josiah Emery, who declared before the Committee of the House of Commons, in the case of Mr. Mudge,† that he had read an account, in an English paper, of that sort of spring, under the name of cylindrical spring, about a year or two before Mr. Arnold took out his patent. This account was contained in an advertisement from Bow-street, relative to a number of watches that had been stolen in France and brought into England; but Mr. Arnold perhaps never saw it, and may have thought of the application of the helical spring to watches without previous hint or assistance.

Arnold may be considered as the first applier of the helical spring (as an improvement).

Mr. Earnshaw, in the explanation of his timekeepers, presented to the Board of Longitude, after noticing the insufficiency of the cylindrical spring, states that he had, by long perseverance, found how to make springs increasing in thickness to the outer end, in order to effect the isochronism of the vibrations. This method of obtaining isochronal vibrations had been long before explained by Berthoud, with regard to the spiral spring, in that part of his Treatise on Marine Time-pieces

Earnshaw denies the isochronism of the helical spring, and asserts that he had discovered the remedy of that defect—but it was done long before by Berthoud.

* See note 2, p. 7.

† Report from the said Committee, pp. 104 and 105.

pieces which we have already quoted; and we are rather surprized to learn that it cost Mr. Earnshaw so much trouble to discover it; but, according to his account, that property does not answer completely the object in view, and watches, with isochronal springs, well adjusted at first, will progressively lose in their rates, from the relaxation which takes place in them. The remedy for that evil Mr. Earnshaw declares himself to have discovered, by a continuance of the same stubborn application which he had bestowed on the former part of his labours; and that it consists in making the springs of such a shape as to gain in the narrow vibrations, about five or six seconds per day more than in the wide ones. The reason of this contrivance is explained by the author in the following passage of his statement (p. 10): "I find the common relaxation of balance springs to be about five or six seconds per day on their rates in the course of a year; therefore, if the short vibrations are made by the shape of the spring to go about that quantity faster than the long ones, and as the spring relaxes in going by time, so the watch accumulates in dirt and thickness of the oil, which shortens the vibrations, the short ones then being quicker, compensates for the evil of relaxation of the balance spring." But the whole of this explanation, though plausible at first sight, seems liable to considerable doubts and objections, and it would require a series of decisive experiments to prove the accuracy of the method, and the certainty of its application.

Gradual decay in the strength of springs, asserted by Earnshaw.

—who disclosed as a remedy that the narrow vibrations should be made the quickest.

This remedy doubtful.

Some skilful makers are not satisfied as to the reality of this relaxation of the balance spring, at least to the degree which is implied in the above reasoning; and none, we apprehend, will believe that the relaxation, supposing it to take place, will be so uniform as to admit of a remedy, fixed in all cases to the same quantity of difference between the wide and narrow vibrations. Is it probable that the effects of the relaxation, which proceeds from wear, can be ascertained by any other means than very long trials with each individual spring? Or is it to be supposed, that the progressive alteration of springs is of such a nature, as to be concluded from a short experiment, taking it for granted, that the variations will be always in proportion to the times? Some chronometers have been known

It may be questioned whether springs do fall off?

—or if so, whether uniformly?

—or otherwise determinable than by experiment?

Some chronometers gain on their rates;

and others continue steady

known to accelerate instead of retarding, during a considerable time; and there are some which have gone for years without any material alteration in their rates; a fact which strongly militates against the rule proposed, as an unfailing and universal method. What would have been the effect of introducing into those timekeepers the invariable excess of five or six seconds of the narrow above the wide vibrations, which, like a quack medicine, is to cure, without distinction of symptoms or subjects, all the disorders proceeding from the cause of relaxation.

Mr Earnshaw's remedy is like an application of the same medicine to all patients,—to the healthy as well as the sick.

The proposed remedy appears liable to the objection which naturally occurs on the slightest consideration of any compensation intended for circumstances which are contingent, and effects which cannot be anticipated. It is at first a source of irregularity, while there is no certainty of its proving an effectual correction of the supposed future changes. And again, even if the relaxation of the springs could be remedied by the shape, or any other essential requisite of the spring itself, it would still be hazardous to adopt a specific variation in that organ, on account of effects which might afterwards result from the different thickness of the oil or accumulation of dirt; two circumstances which are too variable and uncertain to appear susceptible of being counteracted by any regular process. Our arguments, however, ought to be allowed no other force than what can be derived from a clear general view of the subject; and we will admit, that, if Mr. Earnshaw should be able to produce a series of conclusive experiments in support of the accuracy of his suggestions, with certain rules for practice derived from them, his remedy for the relaxation of springs would then deserve to be reckoned a valuable addition to the art of making timepieces. In the mean time, the makers will do right to follow that method which is at present in general use among them, and continue their endeavours to accomplish the perfection of chronometers by the principle of isochronal vibrations.

It includes the considerations of changes in the oil or dirt in the machine; which are uncertain.

Mr. Earnshaw ought to exhibit facts and deductions, so as to give a practical rule.

—and till he does, his remedy is not advisable.

The isochronism of the balance affords the advantage of wide vibrations.

Among the great advantages resulting from the isochronism of the balance spring, is the facility which it affords to procure wide vibrations, and increase the power of the regulator. The irregularity proceeding from the springs, having their wide and narrow vibrations of different durations, was for a long time so great an obstacle to that practical improvement, that even the genius of Daniel Bernoulli, to whom we are indebted for very deep

deep researches upon the subject of chronometers, ascribed* the principal cause of the imperfections of watches to the wideness of the vibrations; and, while he assigned the small power of the balance, as the second cause of error, he found himself under the necessity of recommending † very short vibrations, in order to effect, in that way, the required equality of their duration.

This isochronism and the detached escapement do also destroy the irregularity arising from the first mover.

Another great advantage, proceeding from the isochronism of the balance spring, combined with the detached escapement, is the destruction of those variations which would arise from the main spring. From the first appearance of the invention of the detached escapement, we find P. le Roy attentive to that important object; and in 1748, when he presented his model to the Academy of Sciences, he tried it before that assembly with the main spring, at the two extremes, when the difference in the rate of going was found to be very small.‡ The author showed how to destroy even that small difference, and explained how all the vibrations might be rendered of the same duration.‖ P. le Roy having afterwards discovered the method, of effecting the isochronism, by means of the balance spring, trusted so confidently to that property, combined with his improved detached escapement, that in the chronometer for which he was rewarded in 1766, he laid aside the chain and fusee, and made the maintaining power act immediately on the train of wheels. In the "Succinct Account of the Attempts "of Messrs. Harrison and Le Roy, for finding the Longitude "at Sea, by P. le Roy;" an English translation of which, by a Fellow of the Royal Society, was published in 1768, in London; the author expressly states, that his watches, according to that construction, go thirty hours without winding up; and upon examining their rate of going in the first, and in the last fifteen

Peter le Roy, who had produced both so very early,

—did reject the chain and fusee, in 1766.

* Recherches, Mechaniques et Astronomiques, sur la Question proposée par l'Academie Royale des Sciences, pour l'Annee, 1745, la meilleure Maniere de trouver l'Heure en Mer. § xxii.

† The same § xxviii.

‡ Journal des Machines, 1748.

‖ See the description of that chronometer, in the Mémoire sur la meilleure Maniere de mesurer le Temps en Mer, par P. le Roy; attached to Cassini's voyage.

hours

An experiment of this kind afterwards made by Arnold

hours, no sensible variation is perceptible. The late Mr. Arnold also made a similar experiment at the house of the late Mr. Aubert, and in the presence of several ingenious gentlemen;* and the result evinced the success with which he had removed, by the same means, the errors arising from the inequalities of the maintaining power.

Concluding remarks.

The preceding statement is intended to afford a candid, though not a minute account of the progress of chronometry to the present day; as far as the steps, by which the art has advanced to its actual state of perfection, may be esteemed either inventions entirely new, or essential improvements in the methods known before. Those methods which do not admit of philosophical or clear description, and also such as have not yet been sufficiently established by experiment, are not considered as within the limits of this sketch; and we must observe the same silence with regard to improvements in mere workmanship, though in themselves of great practical value.

The mechanical problem of the longitude is greatly indebted to the skill of workmen;

We cannot, however, avoid noticing, that the leading principles, by which the mechanical solution of the problem of the longitude at sea has been effected, have derived a considerable part of their utility and success from the great perfection in the execution, which is a consequence of the flourishing state of the trade of watchmaking; and, under that point of view, we must

—who have been supported by our extensive commerce, &c.

thank the great wealth and maritime commerce of this country, for an extensive demand, which has not only promoted the manufactory of good watches, but created a new branch of trade, by a considerable demand for timekeepers for sea.

II.

Account of a Series of Experiments, shewing the Effects of Compression in modifying the Effects of Heat. By Sir JAMES HALL, *Bart. F.R.S. Edinburgh, &c. &c.*

(Concluded from page 212.)

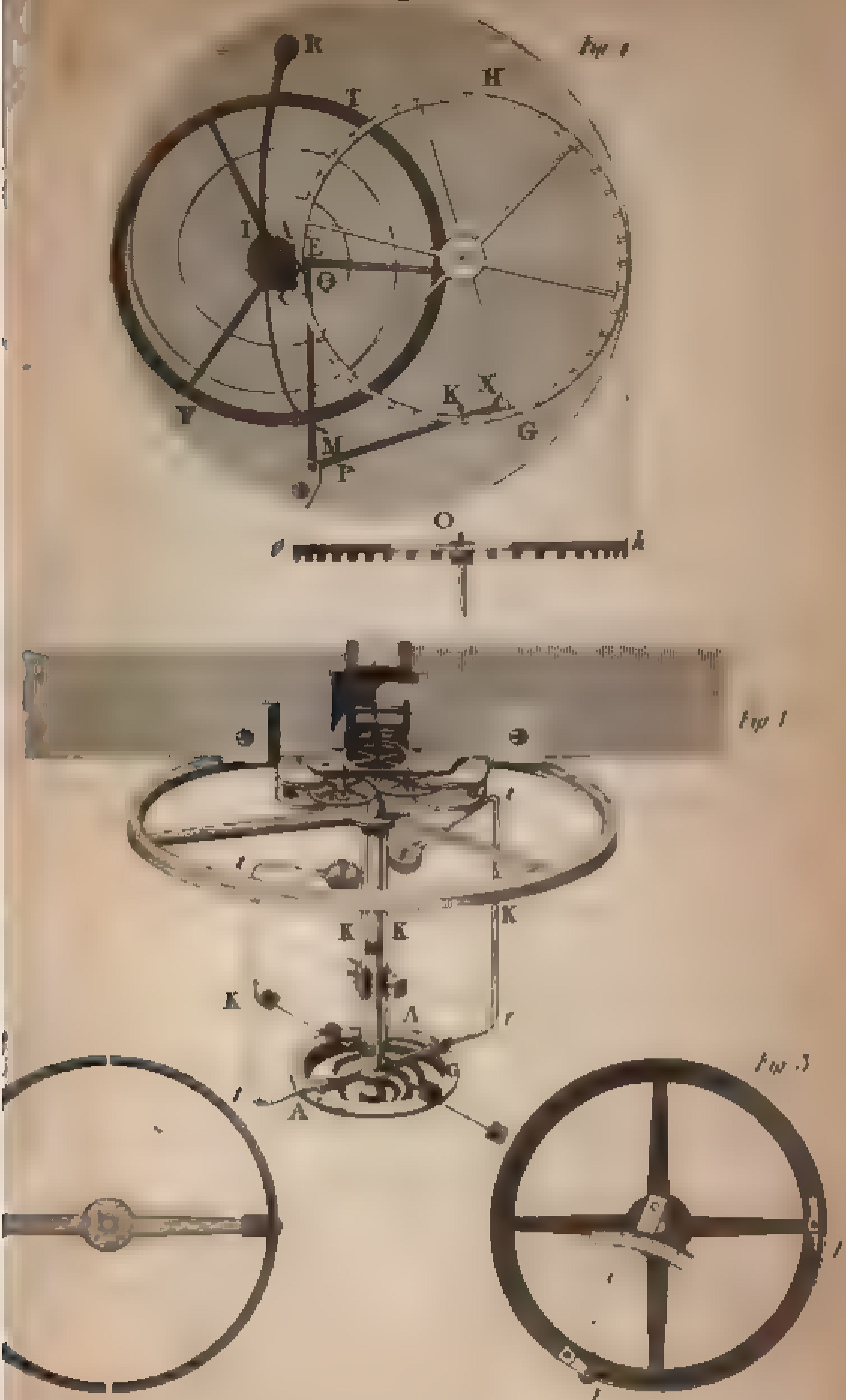
General obser-

WHAT has been said of the heat conveyed by internal vol-

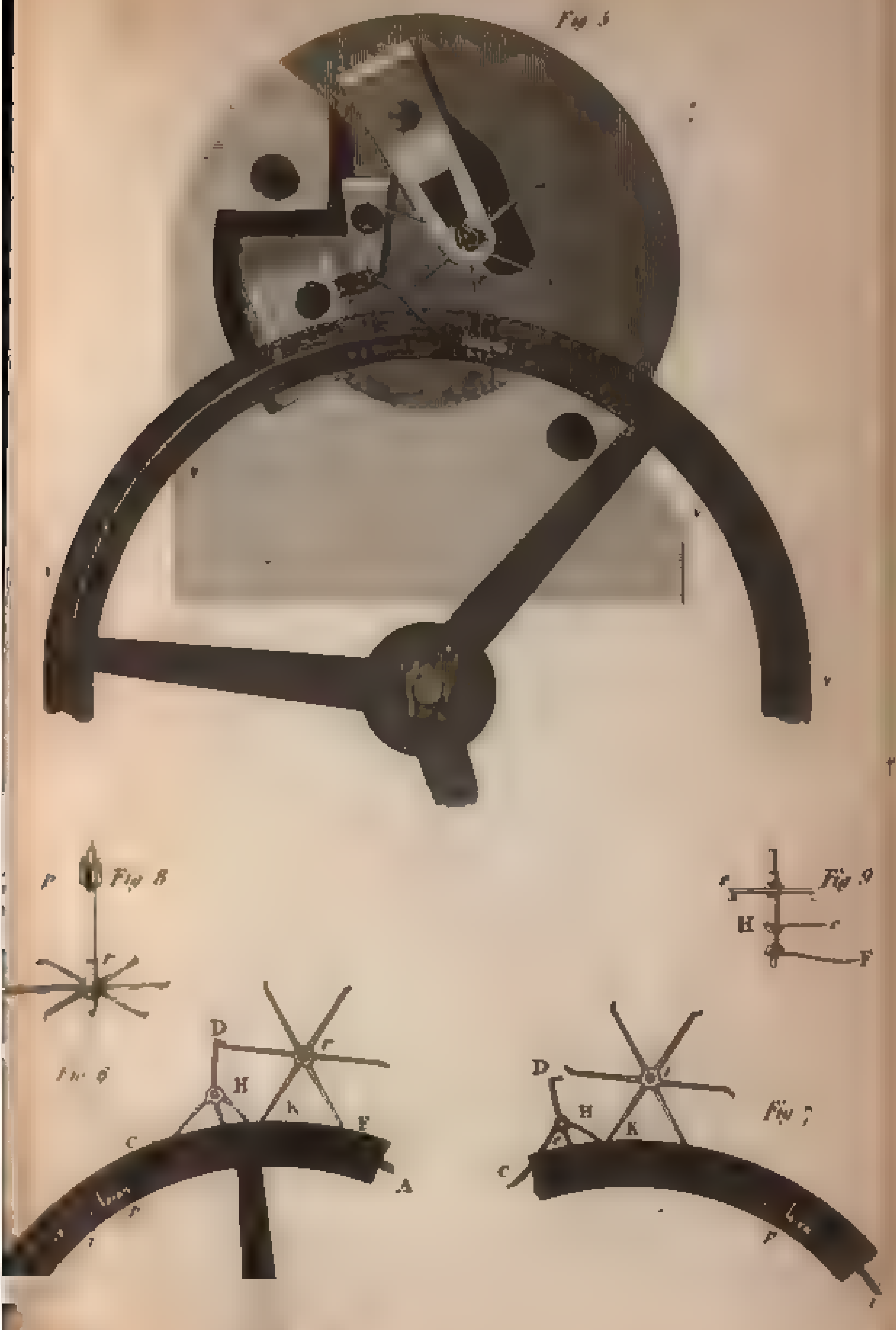
* That experiment is generally known, and has been recently mentioned in the Answer to Mr. Earnshaw, by Mr. Dalrymple. p. 60.

canic

Invention of N Chronometers



Invention of Chronometers



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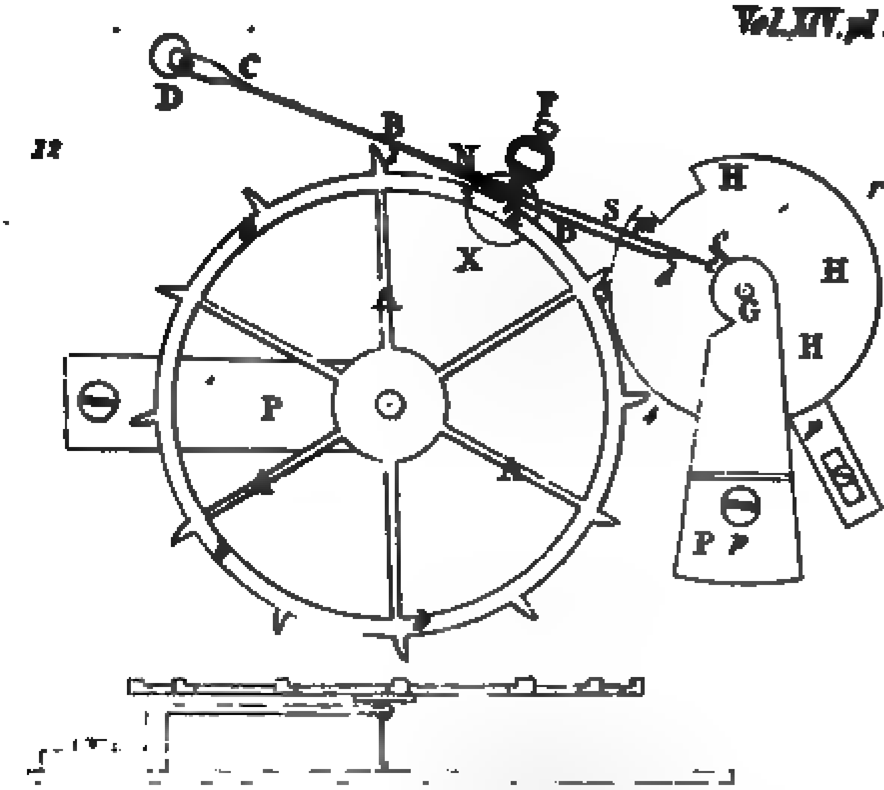

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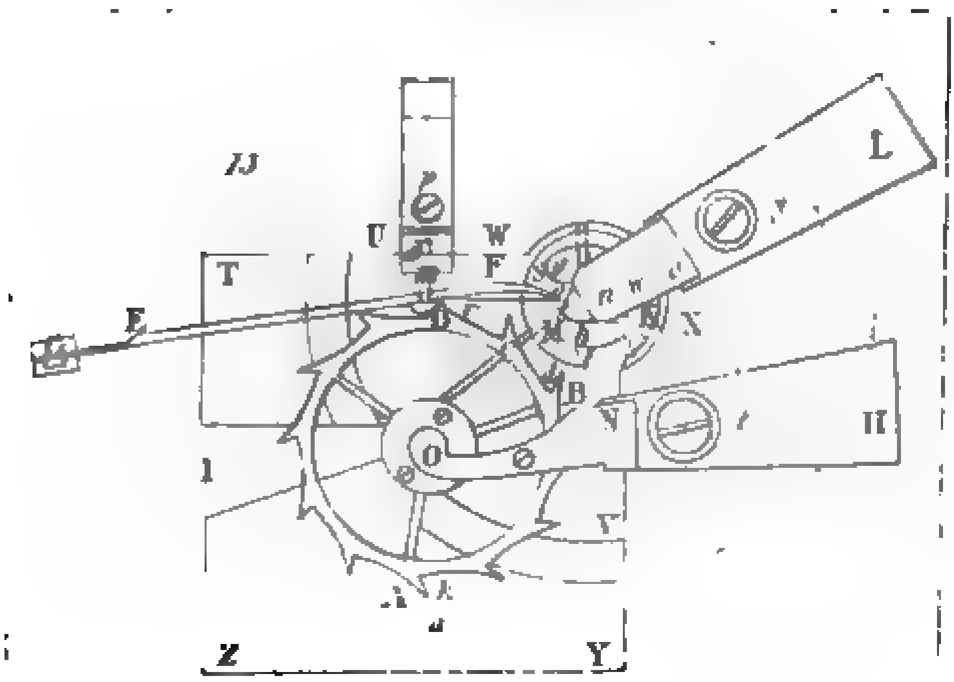

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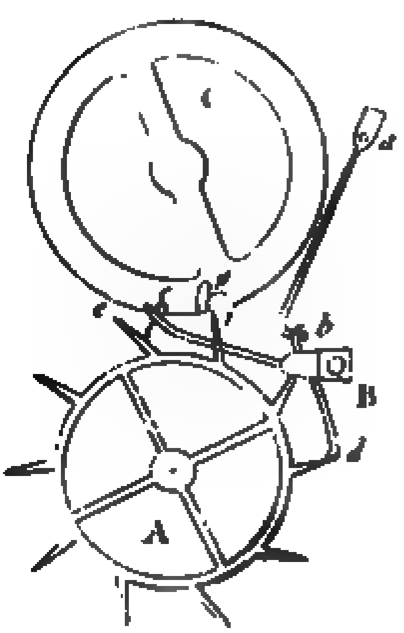

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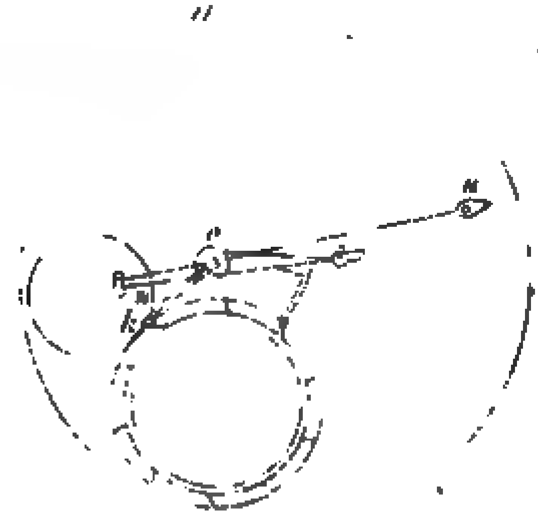

canic streams, applies equally to that deeper and more general heat by which the lavas themselves are melted and propelled upwards. That they have been really so propelled, from a great internal mass of matter, in liquid fusion, seems to admit of no doubt, to whatever cause we ascribe the heat of volcanoes. It is no less obvious, that the temperature of that liquid must be of far greater intensity than the lavas, flowing from it, can retain when they reach the surface. Independently of any actual eruption, the body of heat contained in this vast mass of liquid, must diffuse itself through the surrounding substances, the intensity of the heat being diminished by slow gradations, in proportion to the distance to which it penetrates. When, by means of this progressive diffusion, the heat has reached an assemblage of loose marine deposites, subject to the pressure of a great superincumbent weight, the whole must be agglutinated into a mass, the solidity of which will vary with the chemical composition of the substance, and with the degree of heat to which each particular spot has thus been exposed. At the same time, analogy leads us to suppose, that this deep and extensive heat must be subject to vicissitudes and intermissions, like the external phenomena of volcanoes. We have endeavoured to explain some of these irregularities, and a similar reasoning may be extended to the present case. Having shewn, that small internal streams of lava tend successively to pervade every weak part of a volcanic mountain, we are led to conceive, that the great masses of heated matter just mentioned, will be successively directed to different parts of the earth; so that every loose assemblage of matter, lying in a submarine and subterranean situation, will, in its turn, be affected by the indurating cause; and the influence of internal volcanic heat will thus be circumscribed within no limits but those of the globe itself.

vations on the operation and extent of the heat which is known to act in the interior of our globe.

A series of undoubted facts prove, that all our strata once lay in a situation similar in all respects to that in which the marine deposites just mentioned have been supposed to lie.

All our strata were once beneath the sea.

The inhabitant of an unbroken plain, or of a country formed of horizontal strata, whose observations have been confined to his native spot, can form no idea of those truths, which at every step in an alpine district force themselves on the mind of a geological observer. Unfortunately for the progress of geology, both London and Paris, are placed in countries of little

interest;

interest; and those scenes by which the principles of the science are brought into view in the most striking manner, are unknown to many persons best capable of appreciating their value. The most important, and at the same time, the most astonishing truth which we learn by any geological observations, is, that rocks and mountains now placed at an elevation of more than two miles above the level of the sea, must at one period have lain at its bottom. This is undoubtedly true of those strata of limestone which contain shells; and the same conclusion must be extended to the circumjacent strata. The imagination struggles against the admission of so violent a position; but must yield to the force of unquestionable evidence; and it is proved by the example of the most eminent and cautious observers, that the couclusion is inevitable.*

That the mountains have been elevated out of the sea, is much more probable than that the sea subsided from them.

Another question here occurs, which has been well treated by Mr. Playfair. Has the sea retreated from the mountains? or have they risen out of the sea? He has shewn, that the balance of probability is incomparably in favour of the latter supposition; since, in order to maintain the former, we must dispose of an enormous mass of sea, whose depth is several miles, and whose base is greater than the surface of the whole sea. Whereas the elevation of a continent out of the sea like ours, would not change its level above a few feet; and even were a great derangement thus occasioned, the water would easily find its level without the assistance of any extraordinary supposition. The elevation of the land, too, is evinced by what has occasionally happened in volcanic regions, and affords a complete solution of the contortion and erection of strata, which are almost universally admitted to have once lain in a plane and horizontal position.

Whatever opinion be adopted as to the mode in which the land and the water have been separated, no one doubts of the ancient submarine situation of the strata.

They were originally covered with other earth.

An important series of facts proves, that they were likewise subterranean. Every thing indicates that a great quantity of matter has been removed from what now constitutes the surface of our globe, and enormous deposites of loose fragments, evidently detached from masses similar to our common rock, evince

* Saussure, Voyages dans les Alpes, tom. ii. p. 99—104.

the

the action of some very powerful agent of destruction. Analogy too, leads us to believe, that all the primary rocks have once been covered with secondary; yet, in vast districts, no secondary rock appears. In short, geologists seem to agree in admitting the general position, that very great changes of this kind have taken place in the solid surface of the globe, however much they may differ as to their amount, and as to their causes.

Dr. Hutton ascribed these changes to the action, during very long time, of those agents, which at this day continue slowly to corrode the surface of the earth; frosts, rains, the ordinary floods of rivers, &c. which he conceives to have acted always with the same force, and no more. But to this opinion I could never subscribe, having early adopted that of Saussure, in which he is joined by many of the continental geologists. My conviction was founded upon the inspection of those facts in the neighbourhood of Geneva, which he has adduced in support of his opinion. I was then convinced, and I still believe, that vast torrents, of depth sufficient to overtop our mountains, have swept along the surface of the earth, excavating vallies, undermining mountains, and carrying away whatever was unable to resist such powerful corrosion. If such agents have been at work in the Alps, it is difficult to conceive that our countries should have been spared. I made it therefore my business to search for traces of similar operations here. I was not long in discovering such in great abundance; and, with the help of several of my friends, I have traced the indications of vast torrents in this neighbourhood, as obvious as those I formerly saw on Saleve and Jura. Since I announced my opinion on this subject, in a note subjoined to my paper on Whinstone and Lava, published in the fifth volume of the Transactions of this Society, I have met with many confirmations of these views. The most important of these are derived from the testimony of my friend Lord Selkirk, who has lately met with a series of similar facts in North America.

—which is thought to have been weathered and washed away, &c.

—but there are indications of vast torrents having swept it away.

It would be difficult to compute the effects of such an agent; but if, by means of it, or of any other cause, the whole mass of secondary strata, in great tracts of country, has been removed from above the primary, the weight of that mass alone must have been sufficient to fufil all the conditions of the Huttonian Theory.

This superincumbent weight would produce a compression sufficient to fulfil the Huttonian Theory.

Theory, without having recourse to th
when the two pressures were combin
been their united strength !

We are authorized to suppose, that,
in this situation, underwent the acti
have burnt long before the earliest ti
appears by the magnitude of some v
can scarcely be doubted, that their f
material cessation ever since the su
its present form. In extending tha
of still higher antiquity, when our str
sea, we do no more than ascribe perm
of nature.

The combination of heat and c
these circumstances, carries us to th
nian Theory, and enables us, upon i
the igneous formation of all rock
sites.

Enumeration of the effects of heat under this pressure in producing the various stony matters now found.

The sand would thus be changed
limestone ; and the animal and veg

Other beds, consisting of a mix
would be still more affected by the
tained iron, carbonate of lime, and
ture of various earths, would ente
netrating through every crevice tha
cases, reach what was then the sur
stitute lava : in other cases, it wo
rents, and constitute prophyry, basa
of that numerous class of substanc
under the name of *whinstone*. At t
lar quality, but of composition so
enter into a state of viscidity, such
in their progress towards fusion. I
though far from possessing the sam
susceptible of crystalline arrangem

* This state of viscidity, with its na
serving of great attention, since it aff
most important geological questions.
erted by some substances, in the act of

which, in this sluggish state, would be little disposed to move, being confined in its original situation by contiguous beds of more refractory matter, would crystallize, without undergoing any change of place, and constitute one of those beds of whinstone which frequently occur interstratified with sandstone and limestone.

In other cases where the heat was more intense, the beds of sand, approaching more nearly to a state of fusion, would acquire such tenacity and toughness, as to allow themselves to be bent and contorted, without laceration or fracture, by the influence of local motions, and might assume the shape and character of primary schistus: the limestone would be highly crystallized, and would become marble, or, entering into thin fusion, would penetrate the minutest rents in the form of calcareous spar. Lastly, when the heat was higher still, the sand itself would be entirely melted, and might be converted, by the subsequent effects of slow cooling, into granite, sienite, &c.; in some cases, retaining traces of its original stratification, and constituting gneiss and stratified granite; in others, flowing into the crevices, and forming veins of perfect granite.

The manner in which the strata were most probably heaved or raised up.

In consequence of the action of heat, upon so great a quantity of matter, thus brought into a fluid or semifluid state, and in which, notwithstanding the great pressure, some substances would be volatilized, a powerful heaving of the superincumbent mass must have taken place; which, by repeated efforts, succeeding each other from below, would at last elevate the strata into their present situation.

The Huttonian Theory embraces so wide a field, and comprehends the laws of so many powerful agents, exerting their

is well known. I have seen a set of large and broad crystals of ice, like the blade of a knife, formed in a mass of clay, of such stiffness, that it had been just used to makes cups for chemical purposes. In many of my former experiments, I found that a fragment of glass made from whinstone or lava, when placed in a muffle heated to the melting point of silver, assumed a crystalline arrangement, and underwent a complete change of character. During this change, it became soft, so as to yield to the touch of an iron rod; yet retained such stiffness, that, lying untouched in the muffle, it preserved its shape entirely; the sharp angles of its fracture not being in the least blunted.

influence in circumstances and in combinations hitherto untried; that many of its branches must still remain in an unfinished state, and may long be exposed to partial and plausible objections, after we are satisfied with regard to its fundamental doctrines. In the mean time I trust, that the object of our pursuit has been accomplished, in a satisfactory manner, by the fusion of limestone under pressure. This single result affords, I conceive, a strong presumption in favour of the solution which Dr. Hutton has advanced of all the geological phenomena; for, the truth of the most doubtful principle which he has assumed, has thus been established by direct experiment.

APPENDIX.

No. I.

Specific Gravity of some of the foregoing Results.

Concerning the specific gravities of bodies; particularly such as are porous: the aggregate being less heavy, specifically, than the parts separately taken.

As many of the artificial limestones and marbles produced in these experiments, were possessed of great hardness and compactness, and as they had visibly undergone a great diminution of bulk, and felt heavy in the hand, it seemed to me an object of some consequence to ascertain their specific gravity, compared with each other, and with the original substances from which they were formed. As the original was commonly a mass of chalk in the lump, which, on being plunged into water, begins to absorb it rapidly, and continues to do so during a long time, so as to vary the weight at every instant, it was impossible, till the absorption was complete, to obtain any certain result; and to allow for the weight thus gained, required the application of a method different from that usually employed in estimating specific gravity.

In the common method, the substance is first weighed in air, and then in water; the difference indicating the weight of water displaced, and being considered as that of a quantity of water equal in bulk to the solid body. But as chalk, when saturated with water, is heavier, by about one-fourth, than when dry, it is evident, that its apparent weight, in water, must be increased, and the apparent loss of weight diminished exactly to that

that amount. To have a just estimate, then, of the quantity of water displaced by the solid body, the apparent loss of weight must be increased by adding the absorption to it.

Two distinct methods of taking specific gravity thus present themselves, which it is of importance to keep separate, as each of them is applicable to a particular class of subjects.

One of these methods, consists in comparing a cubic inch of a substance in its dry state, allowing its pores to have their share in constituting its bulk, with a cubic inch of water.

The other depends upon comparing a cubic inch of the solid matter of which the substance is composed, independently of vacuities, and supposing the whole reduced to perfect solidity, with a cubic inch of water.

Thus, were an architect to compute the efficacy of a given bulk of earth, intended to load an abutment, which earth was dry, and should always remain so, he would undoubtedly follow the first of these modes: Whereas, were a farmer to compare the specific gravity of the same earth with that of any other soil, in an agricultural point of view, he would use the second mode, which is involved in that laid down by Mr. Davy.

As our object is to compare the specific density of these results, and to ascertain to what amount the particles have approached each other, it seems quite evident that the first mode is suited to our purpose. This will appear most distinctly, by inspection of the following table, which has been constructed so as to include both.

TABLE

TABLE OF SPECIFIC GR

I.	II. Weight in air, dry.	III. Weight in water.	IV. Weight in air, wet.	V. Difference between columns II.&III	VI. Difference between columns II. & IV. or absorption
1.	125.90	77.55	135.65	47.35	9.75
2.	9.94	6.13	9.99	3.81	0.05
3.	15.98	9.70	16.02	6.28	0.04
4.	5.47	3.33	5.48	2.14	0.01
5.	18.04	10.14	18.06	7.90	0.02
6.	6.48	3.74	7.10	2.74	0.62
7.	10.32	5.97	10.36	4.35	0.04
8.	54.57	31.30	55.23	23.27	0.66
9.	72.27	41.10	76.13	31.17	3.86
10.	37.75	21.15	38.30	16.60	0.55
11.	21.21	12.55	21.26	8.66	0.05
12. Marble	18.59	11.56	18.61	7.03	0.02
13. Chalk.	504.15	302.40	623.20	201.75	119.05
14. Average Chalk.	444.30	264.35	550.80	179.95	106.50
15. Rammed Powder.	283.97	—	—	—	—

Explanation.

Explanation of the table of specific gravities, &c.

Column I. contains the number affixed to each of the specimens, whose properties are expressed in the table.

The first eleven are the same with those used in the paper read in this Society on the 30th of August 1804, and published in Nicholson's Journal for October following, and which refer to the same specimens. No. 12. Is a specimen of yellow marble, bearing a strong resemblance to No. 3. No. 13. A specimen of chalk. No. 14. Shews the average of three trials with chalk. No. 15. Some pounded chalk, rammed in the manner followed in these experiments. In order to ascertain its specific gravity, I rammed the powder into a glass-tube, previously weighed; then, after weighing the whole, I removed the chalk, and filled the same tube with water. I thus ascertained, in a direct manner, the weight of the substance, as stated in column II., and that of an equal bulk of water, stated in column VIII.

Column II. Weight of the substance, dry in air, after exposure, during several hours to a heat of 212° of Fahrenheit.

Column III. Its weight in water, after lying long in the liquid, so as to perform its full absorption; and all air-bubbles being carefully removed.

Column IV. Weight in air, wet. The loose external moisture being removed by the touch of a dry cloth; but no time being allowed for evaporation.

Column V. Difference between columns II. and III. or apparent weight of water displaced.

Column VI. Difference between columns II. and IV., or the absorption.

Column VII. Absorption reduced to a *per centage* of the dry substance.

Column VIII. Sum of columns V. and VI., or the real weight of water displaced by the body.

Column IX. Specific gravity, by the common mode, resulting from the division of column II. by column V.

Column X. Specific gravity, in the new mode, resulting from the division of column II. by column VIII.

The Specific gravities ascertained by the new mode, and expressed in column X. correspond very well to the idea which is formed of their comparative densities, from other circumstances,

Explanation of the table of speci gravities, &c.

stances, their hardness, compact appearance, susceptibility of polish, and weight in the hand.

The case is widely different, when we attend to the results of the common method contained in column IX. Here the specific gravity of chalk is rated at 2.498, which exceeds considerably that of a majority of the result tried. Thus, it would appear, by this method, that chalk has become lighter by the experiment, in defiance of our senses, which evince an increase of density.

This singular result arises, I conceive, from this, that, in our specimens, the faculty of absorption has been much more decreased than the porosity. Thus, if a piece of crude chalk, whose specific gravity had previously been ascertained by the common mode, and then well dried in a heat of 212°, were dipped in varnish, which would penetrate a little way into its surface; and, the varnish having hardened, the chalk were weighed in water, it is evident that the apparent loss of weight would now be greater by 23.61 *per cent.* of the dry weight, than it had been when the unvarnished chalk was weighed in water; because the varnish, closing the superficial pores, would quite prevent the absorption, while it added but little to the weight of the mass, and made no change on the bulk. In computing, then, the specific gravity, by means of this last result, the chalk would appear very much lighter than at first, though its density had, in fact, been increased by means of the varnish.

A similar effect seems to have been produced in some of these results by the agglutination or partial fusion of part of the substance, by which some of the pores have been shut out from the water.

This view derives some confirmation from an inspection of columns VI. and VII.; the first of which expresses the absorption; and the second, that result, reduced to a *per centage* of the original weight. It there appears, that whereas chalk absorbs 23.97 *per cent.*, some of our results absorb only 0.1 or so low as 0.11 *per cent*. So that the power of absoption has been reduced from about one-fourth, to less than the five hundredth of the weight.

I have measured the diminution of bulk in many cases, particularly in that of No. 11. The chalk, when crude, ran to the 75th degree of Wedgwood's gage, and shrunk so much during the experiment, that it ran to the 161st; the difference amount-

amounting to 86 degrees. Now, I find, that Wedgwood's gage tapers in breadth, from 0.5 at zero of the scale, to 0.3 at the 240th degree. Hence, we have for one degree 0.000833. Consequently, the width, at the 75th degree, amounts to 0.437525; and at the 161st, to 0.365887. These numbers, denoting the linear measure of the crude chalk, and of its result under heat and compression, are as 100 to 83.8; or, in solid bulk, as 100 to 57.5. Computing the densities from this source, they are as 1 to 1.73. The specific gravities in the Table, of the chalk, and of this result, are as 1.551 : 2.435; that is, as 1 to 1.57. These conclusions do not correspond very exactly; but the chalk employed in this experiment, was not one of those employed in determining average specific gravity in the Table; and other circumstances may have contributed to produce irregularity. Comparing this chalk with result second, we have 1.551 : 2.575 so 1 : 1.6602.

Explanation of the Table of specific gravities, &c.

No. II.—TABLE,

CONTAINING THE REDUCTION OF THE FORCES MENTIONED IN CHAP. VII. TO A COMMON STANDARD.

I. Number of experiment referred to in Chap. VII.	II. Bore, in decimals of an inch	III. Pressure in hundred weights	IV. Temperature by Wedgwood's pyrometer	V. Depth of sea in feet.	VI. Ditto in miles.	VII. Pressure, expressed in atmospheres.
1	0.75	3	22	1708.05	0.3235	51.87
2	0.75	[illegible]	25	1708 05	0 3235	51.87
3	0.75	10	20	5693.52	1.0783	172 92
4	0.75	10	31	5693 52	1.0783	172.92
5	0.75	10	41	5693.52	1.0783	172 92
6	0.75	10	51	5693.52	1 0783	172.92
7	0.75	10	—	5693 52	1.0783	172 92
8	0.54	2	—	2196.57	0.4160	66.71
9	0.54	4	—	4393.14	0.8320	133.43
		8.1	—	8896 12	1.6848	270.19
10	0.75	3	21	1708 05	0 3235	51.87
11	0.75	4	25	2277.41	0 431	69 70
12	0 75	5	—	2846.76	0.53[illegible]	86.46

Explanation.

Explanation of the Table of forces, &c.

Column I. contains the number of the experiment, as re- red to in the text. Column II. The bore of the barrel used, decimals of an inch. Column III. The absolute force app to the barrel, in hundred-weights. Column IV. The temp ture, in Wedgwood's scale. Column V. The depth of at which a force of compression would be exerted equal to t sustained by the carbonate in each experiment, expressed feet. Column VI. The same in miles. Column VII. Co pressing force, expressed in atmospheres.

Both Tables were computed separately, by a friend, Mr. Jardine, and myself.

The following data were employed:

Area of a circle of which the diameter is unity, 0.7855

Weight of a cubic foot of distilled water, according to Pr fessor Robison, 998.74 ounces avoirdupois.

Mean specific gravity of sea-water, according to Bl 1.0272.

Mean heighth of the barometer at the level of the 29.91196 English inches, according to Laplace.

Specific gravity of mercury, according to Cavendish Brisson, 13.568.

III.

Catalogue of thirty-one Specimens, shewing the Result of S JAMES HALL's *Experiments on the Effects of Heat mo fied by Compression; which were deposited by him in British Museum, on the 29th of June 1806.**

Specimens of results of bodies exposed to strong heat under compression, by Sir James Hall.

NUMBERS 1, 2, 3, 4, 5, 6, and 7, were all produced separate experiments from pounded carbonate of lime.

Number 1, was obtained in 1799. It is a firm stone, requi a smart blow of a hammer to break it. It was enclosed in a c tridge of paper, the mark of which it still bears. The othe

* Communicated to the editor by the author.

Specimens of results of bodies exposed to strong heat under compression, by Sir James Hall.

are still harder and more compact, approaching nearly in these qualities to common limestone. Numbers 2, 4, and 7, possess a degree of semitransparency, most remarkable in Number 4. And all of these specimens exhibit an uneven fracture, approaching to that of beeswax and marble. Their colours are variously, though slightly, tinged with yellow and blue. In particular Number 3, which, though produced from common white chalk, resembles a yellow marble. Numbers 3, 5, and 6, have taken a tolerable polish. Number 7, contains a shell introduced along with the pounded chalk, and now closely incorporated with it. Along with Number 3, is a specimen (A. 3.) of common yellow marble, bearing a strong resemblance to the artificial stone.

Numbers 8, 9, 10, and 11, all formed from pieces of chalk exposed unbroken to heat and pressure. Number 8 is remarkable for a shining grain and semitransparency.

Numbers 9 and 10, shew parallel planes like internal stratification, which has often appeared in calk in consequence of the action of heat, though nothing of the kind could be seen in the native mass. Number 11, very compact, and of a yellow colour.

Numbers 12 and 13, examples of welding, in which the pounded chalk has been incorporated with a lump of chalk upon which it had been rammed, so that their joining is hardly visible in the fracture.

Numbers 14, 15, and 16, shewing the fusion of the carbonate well advanced, with a considerable action on the porcelain tube. In Number 15, the rod of chalk is half melted, and a yellow substance produced by a mixture of the carbonate with the porcelain. Number 16, is a lump of chalk in a state indicating softness, a piece of porcelain which lay in contact with it having sunk a little into the substance of the carbonate.

Numbers 17 and 18, and all the following numbers, being delicate, are enclosed in tubes of glass, and fixed with sealing-wax on little cups of wood. Number 17, formed from pounded chalk, shows in one part the most complete formation of spar, with its rhomboidal fracture, I have ever obtained. The carbonate having lost some of its carbonic acid, had crumbled so much in its essential parts by the action of the air, that the crystallization was no longer visible, and I had given up the

specimen

Specimens of results of bodies exposed to strong heat under compression, by Sir James Hall.

specimen for lost, till some time in July 1804, when employed in examining these results, in order to show them in the Royal Society of Edinburgh, a mass of the carbonate broke in two, and exhibited the fracture now before us, nearly in as good a state as it was originally. I immediately enclosed it in a glass tube, and sealed it up with wax; so that I have hopes of preserving it; and it still continues intire, though now sealed up for a year and a half. Number 18, likewise from pounded chalk, is perfectly fresh and intire, though made more than two years ago; it shows some beautiful clear crystals of spar in parallel plates, but they are so small as to require the use of a glass.

Numbers 19, 20, and 21, show examples of fusion and action on the tubes. In Number 19, a shell is finely united to some pounded chalk. In Number 20, the mass, originally of pounded chalk, is sinking upon itself, and acting at the same time upon the tube; the fracture of the carbonate, in its pure parts, shewing brilliant facettes of crystallization. In Number 21, the carbonate in a state like the last; the compound of porcelain and carbonate shewing its liquidity by penetrating the tube, so as to form a distinct view of a dark colour, and then spreading on its outside to a considerable extent, terminating with the black line, alluded to in the account of the experiments.

Numbers 22, 23, and 24, give proofs of intire fusion. In Number 22, we see two porcelain tubes, enclosed for preservation in a glass tube; the end attached to the little wooden cup must be held downwards, to show the position in which the experiment was made. The innermost porcelain tube stands with its muzzle upwards, and the outermost covers it in the inverse position. The carbonate was contained in the inner tube during the action of heat; the barrel failed suddenly, and the carbonate has boiled over the lips of the inner tube, running down, as here appears, almost to its bottom: thus proving, that immediately previous to the failure of the apparatus, the carbonate had been in a liquid state. Number 23, two mases of carbonate welded together in a complete state of froth. The substance shining, and semitransparent.

Number 24, two separate masses exposed together to heat; one from pounded chalk, now in a state quite like the last, the other put in as a lump of chalk dressed flat at both ends, and a letter cut at each end (as done in many of the experiments).

B

It is in a shining and almost transparent state; at one end the flat form and the letter are still visible; the other end is completely rounded in fusion, with a glassy surface.

Specimens of results of bodies exposed to strong heat under compression, by Sir James Hall.

Number 25, shews the substance produced by the combination of carbonate of lime with pure silex. Part of the porcelain tube in this specimen is filled with pounded silex; which having a very feeble agglutination, is supported by some sealing-wax. Upon the silex, during the experiment, had lain some carbonate of lime, the lower part of which had united with the silex, producing a semitransparent substance, with a delicate tinge of blue. The termination of this compound, as it had advanced downwards into the silex, shews the round and mammillated form of chalcedony.

Number 26, result of an experiment with heat and compression, made July 22, 1805, with some pure carbonate of lime, prepared by Mr Hatchett. The carbonate was enclosed in a small tube of platina, and was thus secured against all contamination.

Number 27, result of an experiment made likewise in platina, with a fragment of a perriwinkle shell: the form of the shell is still visible, though the substance is glazed by semifusion. Along with it, on the same stand, is a small drop like a pearl, formed by the intire fusion of one of the fragments; and a portion of a shell of the same kind, in its natural state, is introduced, in order to show what change had taken place during the experiment.

Number 28, is a specimen of coal produced from horn; it is a shining black substance, exactly resembling pitch, or black sealing-wax: it was formed in a low red-heat, and in circumstances of compression; by which, while some of the volatile parts of the original were allowed to separate, others were retained. It has thus acquired a jet-black colour, while it retains its inflammability, and burns with bright flame.

Number 29. likewise produced from an animal substance, flannel. In this case, none of the compound parts of the original substance seem to have separated from it, owing either to less heat or greater closeness, than in the last case. The consequence has been, that the original colour has undergone much less change, being of a yellow-red. At the same time the substance has been in a state of fusion, and has assumed a polish

Specimens of results of bodies exposed to strong heat under compression, by Sir James Hall.

polish by moulding itself on the glass into which it had be[en] pressed. This result seems to bear some analogy to the su[b]stance called resinasphaltum, described by Mr. Hatchett.

Number 30, is a piece of wood, partially converted into co[al] by heat and compression. In some parts, the substan[ce] entirely resembles pitch, being full of large and shining air hol[es;] in others, the fibres of the wood are still distinctly visible. Th[e] whole is jet-black, and burns with a bright flame.

Number 31, is a specimen of the substance like wool, form[ed] in several of these experiments by the exudation of the fusi[ble] metal through the barrel of iron, the metal in a liquid st[ate] spouting to a considerable distance, and depositing this su[b]stance upon any obstacle opposed to the stream.

IV.

Enumeration of several Cases of Ships which have been struck b[y] Lightning; the destructive Effects having acted in a vertic[al] Direction towards the Centre of the Earth, and never horizon[tally]. In a Letter from JAMES HORSBURGH, *Esq.*

To Mr. NICHOLSON,

SIR, *Walworth, July 2, 1806.*

Introductory letter.

I HAVE taken the liberty to enclose some remarks on a fe[w] ships that have been struck by lightning. I was induced to d[o] so, because it has never come within the reach of my knowledg[e] that the yards of any vessel have been injured by lightning.

It may be probable, that the electric matter never acts in [a] horizontal direction, destructively. But I must own that I ha[ve] little knowledge of the subject here mentioned. And it [is] principally to point out, how seldom the yards of ships ar[e] injured by lightning, that I have taken this liberty.* If yo[u]

* Authentic facts seldom fail to afford instructions to those sci[enc]es which are grounded immediately on experiment. That th[e] yards of ships are seldom or never injured by lightning, appears t[o] arise not from their horizontal position, but from their lying out o[f] the circuit or immediate path of the lightning from the clouds to th[e] earth.—W. N.

thin[k]

think any part of these remarks deserving of a place in your valuable Journal, you will greatly oblige

Your most obedient servant.
JAMES HORSBURGH.

P.S. It may be observed, that ships navigating in seas or places where lightning is frequent, should be careful to place no cargo of an inflammatory nature near the masts; for the electric matter is frequently conducted by them into the hold. This happened to the King George, in Canton River: and a few years ago, the Royal Charlotte, with all her crew, were blown to atoms: this happened at Diamond Harbour, in the river Hooghley, during a night when much thunder and lightning prevailed. She had a great quantity of gun-powder placed forward in the hold, (said to have been stowed close forward around the mast), and it was supposed that her foremast had been struck by lightning; which probably was conducted by the mast into the hold among the gun-powder, and produced the dreadful explosion, which was heard at a great distance, and the concussion felt several miles. Very few fragments of the ship were visible next morning.

Dreadful consequences of lightning, conducted by ships' masts.

Enumeration of Ships struck by Lightning, &c.

In June 1792, returning from China by the Mindora passage, in the Anna, when we were in latitude 13° N°, and about $2\frac{1}{2}$° from the west coast of Luconia, a squall of wind from the south-west was experienced, followed by heavy rain, with much thunder and lightning. At this time a loud explosion burst over the ship, the lightning first embracing * the pole of the main-top-gallant-mast, tore it and the mast into small fragments, in its passage downwards: then embracing the head of the top-mast in its descent, tore it into pieces. From the topmast, it continued its direction down the mainmast, on one side of the mast, tearing away the hounds, and injuring the mast greatly, particularly where there was any iron-work. About eight feet above the deck, the electric matter was attracted from the main-

The masts of the Anna destroyed or injured by lightning.

* I suppose the word "*embracing*" to denote the visible passage of the lightning, as it *surrounded* the parts here and elsewhere spoken of.—N.

mast

mast by a large iron thimble near it, which was fixed to the mizen-stay: this thimble it scorched black, and cut a part of the stay; from whence its direction was no longer preceptible.

The greased parts were shattered, but the blacked parts remained uninjured.

All the parts of the topmast and top-gallant-mast, which were scrapped and greased, were split into a thousand pieces; but neither the heads of these masts, nor the caps, where they were covered with blacking, received any injury.

The yards were not injured.

None of the yards which were fixed to the masts, four in number, received the smallest damage from the lightning, nor any of the sails touched by it. Four men which were sitting under the top, to shelter themselves from the rain, had the hair of their heads and eyebrows a little singed, but received no further injury. The colour of this lightning appeared to be a pure white.

The main-masts of a snow destroyed, and not the yards.

In June 1788, a snow, in Bombay Harbour, belonging to the Honourable East India Company, was struck by lightning. It embraced the main-topmast head, rent that mast in pieces, and the mainmast was split from head to heel, and rendered unserviceable; whilst the main, and main-topsail yards that were fixed aloft on the masts, did not receive any injury.

Similar events.

In July or August 1792, a ship from Bombay, bound to China, was struck by lightning in Malacca Strait, near Prince of Wales Island. It embraced the masts forward, and destroyed them; but none of the yards which were fixed on these masts received any injury.

The King George, country ship of Bombay, had her foremasts destroyed by severe lightning, and was set on fire.

About September 1793, the King George, a large ship, belonging to Bombay, proceeding up Canton River, was struck by lightning. It embraced the fore-top-gallant-mast head, shivered the masts in the fore part of the ship, killed the people in the fore-top, and some of those on deck which stood near the foremast. Although the fore-topmast and top-gallant-mast were much perforated by the lightning, and in danger of tumbling down, none of the yards were injured. The electric matter was conducted by the foremast into the hold imperceptibly; for no traces of it were visible where it penetrated below the deck; notwithstanding, when no danger was apprehended, the ship was perceived to be on fire forward about seven hours

The lightning set fire to resinous goods stowed near the foremast.

after she was struck by the lightning. There was stowed in the hold, near the foremast, olibanum, myrrh, and sandlewood; the olibanum, being an inflammatory, resinous substance, was set

set on fire by the electric matter; and the fire communicated from it to the sandlewood, placed in a large quantity over the olibanum; which had produced a great mass of fire in the hold before it was discovered, when it forced itself through the decks and burnt the ship to the water's edge, although every exertion was made to save her. The ship was destroyed.

In August 1804, the Bombay frigate, belonging to the Honourable East India Company, at anchor in Malacca Road, was struck by lightning. It embraced the centre masts of the ship, and rendered the mainmast unserviceable; although the yards received no injury. The sails were set on fire by the lightning, notwithstanding they were wet by the heavy rain which continued to fall at the time, and assisted the exertions of the crew in extinguishing the fire. The Bombay frigate had her masts injured, and her sails set on fire; but the yards not damaged.

In July 1804, the ship Page, at anchor in Malacca Road, was struck by lightning. It first embraced the fore-top-gallant-mast head, shivered this mast and the fore-topmast in pieces, and continued its vertical direction down the foremast, tore and rent it greatly, without injuring any of the yards, which were fixed across on these masts. This happened a little after midnight; at which time we were about thirty miles distant from Malacca, and perceived much distant lightning in the direction of it, although none prevailed where we were. On the following night, there was a hard squall from the coast of Sumatra, with much thunder, lightning, and rain. The lightning this night was very vivid, accompanied with a loud hissing noise over the town, but fortunately it did no damage during the night, excepting that the flag-staff of the fort (in the morning) was found injured by it. Ship Page had her masts greatly injured; but not her yards.

In September 1804, the ship Ardassier, at anchor in Malacca Road, had her main-topmast and top-gallant-mast destroyed by lightning; but none of the yards or caps received injury. The same befel the Ardassier.

In September 1802, the ship Daniel, distant from Malacca about nine or ten miles, had her fore-topmast and top-gallant-mast destroyed by lightning, during a squall of wind and rain. The topsail and topmast rigging, although wet with rain, were set on fire by the lightning; and, with the topmast, were cut away to save the ship. The yards were not injured by the lightning. Ship Daniel's fore-topmast, &c. set on fire by lightning; but the yards not affected.

Similar effects of lightning in the ship Trident;

His Majesty's ship Trident, in India, (about 1803), lost her main-topmast and top-gallant-mast by lightning. The yards received no injury.

—the Britannia;

There are at times dangerous lightnings near the Cape of Good Hope. A few years ago, the Honourable East India Company's ship Britannia, returning home from Bengal, was struck by lightning near the Cape. It lodged in the centre of the foremast, and set fire to it, whilst lying to in a storm: the violence of the flames was soon so great, that it was found impossible to extinguish them; and the only remedy left to save the ship and crew from inevitable destruction, was to cut away the burning foremast, which was effected; then it fell clear of the ship, over her lee, in a body of fire.

—and the Bombay Castle.

The Honourable East India Company's ship Bombay Castle, about 1801, returning from China, was struck by lightning near Algoa Bay, to the eastward of the Cape of Good Hope. The lightning entered into the head of the foremast, and descending without making any visible perforation, burst out in fire near the centre of the mast, below the rigging. Every exertion was made to extinguish the fire, without effect: the mast was then cut away, which saved the ship.

General observations.

From the foregoing observations on ships which have been struck by lightning, it may be remarked: 1st.—That it appears always to embrace one of the mast heads at first, and descends downwards. 2d.—That the parts of masts which are covered with tar and blacking, are not so liable to be rent by the lightning as the parts where they are clean scraped, or scraped and covered with tallow. 3d.—That the yards are seldom or never damaged by lightning; although the masts to which they are fixed may be rent into pieces by it. Whether it may be owing to the horizontal position of the yards across the masts, or their being covered with blacking, or a coat of black paint, that is the cause of the lightning not injuring them, when the masts are destroyed, must be left for those skilled in electric phenomena to determine.

V.

Letter from G. S. GIBBES, M. D. *shewing that the Bath Waters contain a much greater Portion of Iron than has hitherto been supposed.*

To Mr. NICHOLSON.

SIR,

The sand that rises with the Bath waters contains iron in black particles,

I BEG leave to send you an account of some experiments I have lately made on the Bath waters; and as the result appears to me singular, I could wish to know whether any circumstance of the same kind occurs elsewhere.

It is well known that many black particles are mixed with the sand which is brought up by the waters of Bath, which particles are attracted by the magnet.

And so likewise does the residuum of the waters.

I have lately evaporated large quantities of the Bath waters, and I have obtained a great deal of their solid contents. Upon examining with a microscope the dry residuum thus obtained, I find numberless black particles interspersed through it, and from the circumstance of the magnet acting on those in the sand, I was induced to present the magnet to the black particles of the residuum obtained by evaporation from the water. The result was, that the magnet acted very forcibly on these black particles. By passing the magnet through the powdered residuum, it became charged with these particles of iron; and by brushing them off from the magnet, and again presenting them to its influence, they were again acted on by it, and rose to the magnet from a considerable distance. I repeated the experiment, by evaporating twenty-six gallons of the water of the King's bath, in a brass vessel; and no instrument or vessel was used during the process that was made of or contained the least portion of iron. I obtained 2252 grains of residuum. This residuum was every where interspersed with black particles, and all these particles were very forcibly acted upon by the magnetic influence.

The Bath water deposits iron: 1st.—Carbonate.

Iron is deposited in three different states by the Bath waters: 1st.—It tinges the glasses which are made use of for drinking the water at the pumps, of a yellow golden colour, which

which can be scraped off. Th
was united with carbonic acid,
on the sides and bottom of th

—2d —Pyrites or sulphuret.

2d.—It forms pyritical incrus
channels of the baths; in the
united with sulphur, as they
air and moisture. 3d.—It is d

—3d —Black sand or metallic.

in black particles, which are
of these particles appeared of
ciently so to allow me to deter

From the above experimen
much care, it appears that iron
metallic state, or nearly app
that the small portion of iro
bonic acid, and which is dep

The Bath waters contain more iron than has hitherto been supposed.

glasses used for drinking the wa
bonate or oxide, is the only po
timated as the quantity of iron
quantity is very inadequate to
waters produce on the human c

I am,

Bath, July 4th, 1806.

V

Letter of Inquiry respecting th
without previous Education a
respondent, R. B. *With* a
tor.

To Mr. NI

SIR,

Remarkable fact that brutes swim naturally, and men do not.

IT has always appeared to m
animal when thrown into deep
ficulty; but that the human s
into a like situation, is sure t
vidual has been previously edu

I have occasionally conversed with many intelligent reasoners on the subject, and must confess that none of the observations I have heard are such as give me satisfaction. By some, it has been remarked, that the superior degree of sensibility of mind in man, compared with brutes, disqualifies him from making the proper exertions in a situation of such novelty and alarm; and others have adverted to the very great difference between the ordinary habits of quadrupeds and men; the former requiring only to perform their usual process of walking when in the water; whereas a man must adopt an unusual position and actions in order to swim. Neither of these remarks appear to me to be well founded. A man of the utmost courage and determination on board a ship, finds those powers of little effect when he is left alone to struggle without skill in the ocean. And though in our artificial method of swimming, (taken from the frog, and very unlike the methods practised by the Asiatics), a man does act very differently from his manner of walking when on the land; yet it will not be pretended that he would sink, if he were to rely on his ordinary walk, as brutes do upon theirs. Upon various former occasions, I have experienced your readiness to discuss such subjects as your correspondents have pointed out. This subject is not, I am sure, unworthy of notice, since the most valuable lives might perhaps be saved, merely by knowledge and information respecting an art which, I strongly suspect, has been thought to require much more practice in its acquirement than it really does, or which, perhaps, may be as natural to us as to other animals.

Questions: Whether man be drowned from terror, or by his usual habits not being adapted to swimming?

Probably from neither.

Importance of this inquiry.

I remain, Sir,

Your constant reader,

R. B.

Observations on Swimming, in Reply to the preceding Letter. W. N.

That the practice of swimming is pleasant, healthy, and highly useful, will be admitted by every one. They who cannot swim, are ever ready to deplore their incapacity with all its serious consequences; and those who know their own powers, in that element which so often serves to convey us from place to place, must be highly sensible of the ease and comfort such a consciousness bestows. It is not generally apprehended, that the

Great advantages of the practice of swimming.

It may be learned with very great facility.

the art of swimming may be learned almost at a single ti and that man is excepted from the general privilege of anim in this respect, only from a circumstance which it is fully his power to command; so that if a man, who falls into de water, can have recollection and resolution enough to avoid single bad habit (too likely indeed to present itself), he require no more instructions to enable him to save his life th a dog or any other animal in like circumstances.

A man is no more liable to be drowned than a dog.

Dr Franklin taught swimming at one single lesson.

I have not at this time in my possession any writing on t art of swimming, except the letter of Dr. Franklin (LIV. the quarto Exper. and Obser.), which has been deservedly printed in many of our periodical works. The Doctor, in t account of his own life, speaks of himself as a first-rate sw mer. He asserts that he taught the art of swimming to a yo man of the name of Wygate, and another friend of his, in t g[illegible] few hours; and that he had it in contempla to [illegible] a swimming school at London, in the summer 1726. His method, as described in the letter before ment [illegible] as follows:

His instructions for becoming familiar with the water.

“ The practice I mean is this. Chusing a place where the wa
“ deepens gradually, walk cooly into it till it is up to yo
“ breast; then turn round, your face to the shore, and thr
“ an egg into the water between you and the shore. It w
“ sink to the bottom, and be easily seen there, as your wat
“ clear. It must lie in water so deep as that you cannot reach
“ to take it up but by diving for it. To encourage yourself
“ order to do this, reflect that your progress will be from dee
“ to shallower water, and that at any time you may, b
“ bringing your legs under you and standing on the botto
“ raise your head far above the water. Then plunge under
“ with your eyes open, throwing yourself towards the egg, a
“ endeavouring, by the action of your hands and feet against t
“ water, to get forward till within reach of it. In this attem
“ you will find, that the water buoys you up against your i
“ clination; that it is not so easy a thing to sink as you ima
“ gined; that you cannot, but by active force, get down to t
“ egg. Thus you feel the power of the water to support yo
“ and learn to confide in that power; while your endeavou
“ to overcome it and to reach the egg, teach you the mann
“ of acting on the water with your feet and hands, whi

“ actio

action is afterwards used in swimming to support your head higher above water, or to go forward through it." page 474.

The Doctor proceeds to make several observations on the natural tendency of the human body to float in the water; at the same time that he advises his friend not to trust to the chance that he may have presence of mind sufficient to take advantage of those hints in a case of real danger, but to learn to swim. The substance of these remarks is: 1st.—That the limbs and head of an human body are heavier than fresh water; but the trunk is lighter, merely because the lungs are inflated. 2d.—That the head is heavier than sea water; but the limbs and trunk are lighter. 3d.—That a person throwing himself on his back in sea water, will float with his face clear, so as to breathe with ease. 4th.—That in fresh water the legs will gradually sink, and the body will float in an erect posture. 5th.—That, in this posture, if the head be held in its natural position, the surface of the water will reach higher than the nostrils, and perhaps a little above the eyes.* 6th.—That if the head be leaned quite back, the nose and mouth will be always above the water and free for breathing, and the body will rise and fall on every such inspiration and expiration.

Dr. Franklin's observations on the natural floatage of the human body.

I am rather surprized at the Doctor's direction about the egg, and the eyes open under water; because it seems as if he thought the submerged experimentalist could see the egg. I know several respectable persons who affirm that they can see objects under water; but as long as we are assured that distinct vision requires that light shoul dcome to a focus on the retina, and that a very considerable part of the refraction performed by the eye is effected at the convex surface of the cornea, intercedent between that substance and the air, we must refuse our credit to such assertions. Besides which, experiment will easily clear up the matter to those who know nothing of optics. For every one may prove by trial, with a bason or tub, that vision under water is scarcely more distinct than that which might be had by looking through a quill or a piece of rough glass.

The Doctor seems to have supposed that man can see under water. This is contrary to optical science,

When I was a boy (in 1770), I had an opportunity of amusing

—and to fact.

* I have always found the nostrils below the surface, and the eyes above it; and I believe there is no great difference in different persons. N.

Instance of an object that could not be seen by the eye, submerged in very clear water.

ing myself by alternately swimming in a deep fresh-water stream, and in the sea, at the Island of Joanna, where both waters are remarkably clear, and are separated by a shingly bank, over which the fresh water runs in a shallow stream. The ground on the sea side consisted of large rolled stones: but as the ship's boat was then on shore taking in water, I assisted in getting the casks out of the fresh water into the sea; for which purpose I had buckled my shoes on my feet. One of my buckles happening to get loose, fell between the stones, in about five feet water, where I saw it very clearly. Nothing seemed more easy than to dive and bring it up; and this I did repeatedly, with my eyes open. But I could never see it when my head was under the surface: and though I continued my efforts, and remained obstinately under the water, feeling about for an object, apparently so well within my reach, I was obliged at last to abandon it.

Another instance in Harlem lake, where the eye, under water, could not distinguish the setting sun, nor the fingers of the hand held near the face.

Numberless proofs, that vision cannot be performed by the human eye under water, would have prevented my thinking more on the subject, if the assertions before mentioned had not solicited my attention, and led me to make a direct experiment. Many years after the event at Joanna, I was swimming in the Harlem lake. The sun was almost setting; the water clear; and the bottom a firm sand, gradually deepening from the shore. I walked onwards till the depth exceeded three feet, and then sat down on the sand, where, consequently, I was completely submerged, my face being opposite the setting sun. My eyes were open; the water appeared strongly illuminated, but no image of the sun was to be distinguished. On holding up my hand and spreading the fingers, the hand itself became discernible as an indistinct object, as soon as it was brought within a foot of my face. The fingers could not be counted at half that distance, in any other manner than by reckoning the successive obscurations as the hand was passed across the eyes. All other objects were too confused to be

The stories of divers, &c. not credible.

discerned or known. Whence it appears, that all the stories of wonderful divers, who could descend into the sea and bring up small objects, such as jewels and trinkets, must be considered as fabulous.

Dr. Franklin's method of learning to

Dr. Franklin's method of learning to swim, by struggling to descend to the bottom, is better calculated to give courage than skill,

swim, gives confidence, but not skill.

skill; but at the same time it must be allowed, that he who has acquired the former, will require very little of the latter to become a swimmer. I have, nevertheless, remarked, that those boys who were the most daring at plunging into the water before they could swim, have mostly arrived at the art later than others who have attended with some care to the method of striking their arms and legs.

Method of practising the stroke, &c.

I have known several persons who, after acquiring the method of striking the arms separately, so as to have gained confidence to walk in water rising above their shoulders, and of striking the legs while the body was supported by the hands bearing on the ground in shallow water, have swam very well upon the first trial to combine both together.

Short rules for swimming well.

The rules for swimming well, in our method, that is to say, swiftly, and with little fatigue, are few. The body must be as near the surface, and the head as low as conveniently may be; the knees must be kept wide asunder, in order that the obliquity of action in one leg may counteract that in the other, instead of their joint action producing a libratory motion of the body; and the stroke or impulse must be given with much more velocity than that employed in drawing the legs up again. It is not easy to make any mistake of importance in using the arms by imitation of other swimmers.

To swim on the back is extremely easy at first trial. How this is to be done.

To swim on the back is so easy, that I have several times taught it at the very first trial, to persons who had no previous notion of supporting themselves in the water. My first care was to assure my pupil that he might depend that his face would continue above water and not sink, when he should be placed in the horizontal position. I directed him to remain in that situation quietly, without the least motion, until he had recovered from any trepidation or confusion of mind, and had acquired confidence from finding that he did not sink. He was then gently to move his hands, in the way of paddling or skulling, which, though not absolutely necessary, would insure the support of his head, and prevent his feet from sinking. And lastly, he was instructed to draw up his legs very gently and strike them out; at the same time bringing his chin towards his breast, to prevent the water from flowing over his face. These instructions, illustrated with appropriate action, being

first impressed on the mind, I then assisted in supporting hi in assuming the horizontal posture. The effects took place a described; the learner found he could swim, and never afte wards forgot it.

Account of a man who had not learned to swim, but who, in a case of extreme danger, supported himself by acting conformably to instructions given him:

After this long, and perhaps garrulous, account of my ex perience in an art, of which the usefulness and value may affo a sufficient interest to apologize for any prolixity, I must ta leave, to introduce another story of a man's life being saved very simple instructions given him in the moments of extre danger. The ship Worcester was moored off Culpee, i the Ganges, in November 1770. One of the men, who w employed in some occupation forward about the cables (believe clearing hawse), slipped into the water, which I a sure was running seven or eight knots (or miles) an hour, a is very common in that river. On the alarm being given, mo of those who were upon deck ran aft, where we saw the man head rise above the water, at the same time that he held u both his hands, and, after a few seconds splashing, sunk agai Soon afterwards he rose a second time, and at that instant th commanding officer, who had a hand trumpet in his hand, ca

—the principal of which instructions was, to keep his hands down in the water.

led out to him "Keep your hands down in the water." H did so; and remained a considerable time afloat, while one o the boats, which were riding astern, was got along side an manned; and this relief was also retarded by a blunder fro too much haste, by which she was cast off without oars o board. His fears must naturally have increased, as his distanc from the ship became greater every moment; and I suppo this impression made him again forget his newly acquired art; for he renewed his elevation of hands and dashing of the water, and again sunk; but soon rose again, and for a short tim obeyed the incessant and unvaried instruction which was voci ferated to him through the trumpet. Whenever he deviated, h sunk; and he had disappeared in this manner at least fiv times, and had been carried almost out of hearing before th boat took him up; which, however, at last happened, witho any injury to his health, as he took an oar and assisted in ro ing back to the ship.

Men are drowned by raising their arms, the

The particulars of this accident shew clearly how it happe that man is drowned in circumstances where brutes alway swim; and why young children are not unfrequently taken u

saf

safe, after floating a considerable time on the water. In three words, man, by the natural action of raising his hands to save himself by grasping some object, adds to the unfloated weight, and sinks his head. But other animals (a few with small arms and powerful legs excepted) have no notion or ability to grasp, and, in their habitual manner of walking, take the prone posture, which is also very suitable for making progress in swimming. Infants, who have short arms, and little of the habit of seizing and grasping, are less likely to drown themselves than men, because they do not raise their arms.

weight of which depresses the head.

Other animals have no notion or ability to do the same, and therefore swim naturally.

We will conclude by noting the practical results. When a man falls into deep water, he will rise to the surface by floatage, and will continue there if he do not elevate his hands. If he move his hands *under the water* in any manner he pleases, his head will rise so high as to allow him free liberty to breathe. And if he move his legs, as in the action of walking (or rather walking up stairs), his shoulders will rise above the water; so that he may use less exertion with his hands, or apply them to other purposes. By following these plain directions, I have not the least doubt but that any person would swim as readily as the man who fell overboard from the Worcester; and when once the ability to support one's self in the water is acquired, other changes of position and action are performed with facility.

How a man, who knows nothing of swimming, may support himself in deep water.

VII.

Facts towards an History of Gold. By Professor PROUST.

(Concluded from page 247.)

WE are no longer ignorant that in this powder the gold is reduced to the metallic state, because the experiments of Pelletier have demonstrated, that tin, or its oxide at a minimum, applied to the muriate of gold, can never produce any other result. But as it is likewise known, that it contains also a portion of oxide, and this even pretty considerable, it has

Gold in the powder of Cassius in the metallic state.

 generally

generally been supposed, that the purple powder could be nothing but an intimate mixture of gold in the metallic state and oxide of tin.

If, however, we reflect on some properties, which eminently distinguish this purple from a simple mixture of powdered gold and oxide, and in particular the difficulty we find in separating the latter from it, we shall be inclined to suspect, that something more than simple mixture must take place in this precipitate.

Let us begin with establishing the metallic state of the gold, after which we will examine the degree of oxidation of the tin that accompanies it.

To analyse the simple powder, we must employ aqua regia, for the nitric or muriatic acid has only a very slow and very imperfect action on it. Scarcely is it wetted with this, before we perceive it lose its colour, yield a solution of gold, and leave the oxide of tin bare. This oxide is heavy, sandy, and transparent as powdered glass, which is the usual charater of the oxide at a maximum. But it may be said, the nitric acid of the solution of gold may have raised it to this degree. I answer, no: for if the purple powder be heated in muriatic acid, the oxide taken from it is equally vitreous, and its solution no longer precipitates that of gold; it only gives a yellow colour with hydrosulphurated water. There can be no doubt, therefore, that, if the tin accompanying the gold be at a maximum of oxidation, it is because it has taken to itself the oxigen of the gold as it threw it down. Oxide of tin at a maximum, therefore, and gold, are irrefragably the constitutent parts of the purple powder of Cassius.

Now to begin with showing that this oxide cannot unite with the gold during the precipitation of the purple powder, any farther then a particular affinity attracts it to this metal, I shall here recapitulate some of the properties of the oxide of tin.

Tin, passing from a minimum of oxidation to a maximum, diminishes in solubility: a fact that has been sufficiently shown in my last paper on tin. In this respect, tin follows the law of most of the metals that are susceptible of two degrees of oxidation. But this decrease of solubility is not the cause of its precipitation in the present instance; for, though less solu-

ble than the oxide at a minimum, it is still very soluble in the muriatic acid, and in aqua regia. For example, let fall a single drop of a very acid muriatic solution at a minimum, into a solution of gold, which is habitually surcharged with it, and you will not fail to produce a purple compound; a purple that certainly has nothing similar to the coloured powders produced by the sulphureous or phosphorous acid, sulphate of iron, &c. Thus, in the case we are examining, there is no reason to suppose, that a few atoms of oxide, which have just acquired a maximum, should thus quit a solvent, that attracts them on all sides, to unite in preference with gold, if the gold did not attract them by a particular affinity. Let me observe too, that the acidity of the liquids increases, in proportion as the gold and tin fall down.

If, therefore, the gold and oxide unite, notwithstanding the obstacle they must have to encounter in such acid menstrua, it is obviously necessary, that a particular attraction should intervene, to save the tin from its customary solubility.

The purple powder perfectly soluble in ammonia.

But a single fact will establish incontrovertibly the particular state of combination, which unites the oxide of tin to the gold. Throw some of the purple powder of Cassius, recently precipitated into a phial full of ammonia, it will dissolve immediately, and impart to the ammonia a vivid and intense purple colour. The solution will pass through the filter, without losing any thing. Water does not decompose it, as it does most of the ammoniacal solutions of metals, unless it be surcharged with the purple powder, and then part of it may separate. Distillation, too, occasions the powder to subside, by carrying off the ammonia; but as long as the liquid contains any ammonia, it continues to hold some of the purple powder in solution. Acids precipitate it from the ammonia in the same manner.

The metallic precipitates of gold are not soluble in ammonia. The oxide of tin at a minimum is but very imperfectly soluble in it, since the solution is always milky. If the purple powder, therefore, dissolve thus copiously and perfectly in ammonia; if it have properties, which neither gold nor the oxide of tin possesses, we must conclude, that these two substances form a real combination with each other: for real combinations alone can have properties strikingly different from those which characterize their component parts. The combination of a metal,

metal, the affinities of which are so
affinities of which are no less inju
cannot fail to appear singular; part
does not exist any that can be com
objected, that the combination of
ammonia will not appear, perhaps
closely investigated: for what can
general principles of chemistry, tha
stronger affinity to ammonia than t
or, than to see in fulminating g
combination, which no acid, or even

Mercury cannot take gold from the purple of Cassius.

Mercury, shaken in a phial with I
not take from it the gold, with
tageously on all occasions where the
however, actually metallic gold in
yield to the action of mercury, so
tainly interpose.

Differs from the precipitate by sulphate of iron:

It has been thought too, that the
between gold precipitated by tin,
sulphate of iron; and even that the
mixed or diluted with a white oxi
the purple powder of Cassius. T
have very little foundation: for if
have something of a purple hue whe
only in one point of view exclusivel
constantly purple, in whatever posit

—and from all other precipitates by combustible substances.

All gold, reduced to the metalli
bustible substance than tin, yields
of which refracts the light, so as to
the vessel containing it is placed t
Gold, precipitated by sulphate of ir
gen, by sulphureous acid, &c., ha
stand between the vessel and the
purple; but we distinguish the refl
riferous molecule, in which we cle
metal. But gold, precipitated by t
deep crimson, is purple, is a velvet
metallic reflection, under whatever
the tint of which differs from that
only by being more intense.

If the reader would be still more fully convinced of the real difference, that subsists between the auriferous precipitates and the purple powder of Cassius, it is sufficient to remind him, that, though the muriate of iron acts upon the muriate of gold in the same manner as that of tin, the oxide of iron, which occasions the precipitation, is by no means attracted by the gold like the oxide of tin, though its solubility is as strikingly diminished. If, then, the oxide of iron, raised to its maximum, do not unite with the gold when they come together under the circumstances in which that of tin infallibly does, certainly nothing but the power of affinity can account for the difference.

Further proofs of real difference.

Lastly, the purple prepared with tin fixes in silk, and dyes it of a violet colour; which is certainly not the effect of a powder of gold incorporated in the pores of its filaments.

Purple of Cassius dyes silk.

In the formation of the purple powder, the excess of the acids has an office very different from what might be supposed; the acids do not take from it any oxide of tin, as it was natural to expect, but they cause, or communicate to the purple powder, a kind of demisolution, which retards its subsidence, and which is on this account very inconvenient, particularly when we are in haste to obtain it. This action of the acids may easily be perceived, if we agitate in marine acid a precipitate recently washed: on doing this, you would say, a solution takes place, and still more if a little heat be employed; but this solution, which imposes on the eye by a kind of transparency, does not stand the test of the filter.

Effect of the excess of the acids.

The precipitation of the purple powder may be accelerated by adding potash occasionally to the liquor. If, at the expiration of a few minutes, its surface does not become clear, a little more may be added; and the operator will have the pleasure of seeing the purple powder collecting in clouds and visibly subsiding. In this precipitation, however, an excess is to be apprehended; but this is easily avoided when we are aware of it. The fact is, if more potash than is necessary to neutralize the surplus of acid be employed, it will act on the uncombined muriate of tin held in solution; and thus a portion of oxide will be added to the purple powder, which should make no part of it.

Potash hastens the precipitation.

—but an excess must be avoided.

Effect

Effect of Acids on t

Action of aqua regia on the purple of Cassius.

If to a hundred grains of wel
weak aqua regia be added, such
at 4° or 5°, and a few drops of
pretty quickly lose its colour,
This solution, precipitated by
grains of metallic gold. Only
remain, which shows, that the aq
The oxide is white, and vitreous,
mum. This result shows us, th
carries with it three times its wei
grains of oxide at a maximum,
at a mininum, it is probable, th
capable of loading itself with
grains of gold.

One part gold, to three of oxide of tin.

Further proofs of their chemical union.

If a portion of muriate of t
aqua regia, the oxide passes to t
precipitated, even by the actio
therefore, in quitting its solvent
ple powder, carries down with it
oxide, this must have resulted
affinity.

Muriatic acid takes the tin from the gold.

Muriatic acid of 10°, kept bo
decomposes it gradually, and r
gold. The power of aggregation
unites them in little lumps, whic
precipitated by iron, sulphureous

The acid, rendered transpare
of tin at a maximum of a slight
destroys this colour, but affords

Action of nitric acid.

Nitric acid of 32° takes some
and brightens its colour; but n
pure gold, whatever length of ti
the brightening proposed by Len

Lentin's brightening.

The nitric acid, when poured
maximum, and a little gold. A
strate this in an instant.

This purple, the brightened ti
of cinnabar, still contains tin.

If it be placed between the eye and the light, we shall perceive, by the blue tint, that the gold begins to mingle with the purple powder.

Sulphuric acid.

The aqueous sulphuric acid likewise enlivens the purple, because it takes from it a little tin; but its action goes no farther.

Sulphate of tin.

The sulphate of tin at a minimum likewise precipitates gold in the state of the purple powder of Cassius.

Of Gold precipitated by some Vegetable Juices.

Gold precipitated by vegetable substances.

I have shown elsewhere, that there are few vegetable juices, whether acids, gummy, saccharine, extractive, &c., which have not the property of disoxidating gold; but among the extractive and colouring juices, there are several which unite with this metal, and form with it purple lakes of a deep and frequently very fine colour. Such unions still farther confirm the disposition gold has to form combinations of a particular order.

Purple lakes.

With dragon's blood.

If a solution of gold be poured into a very clear solution of dragon's blood, and the lake be suffered to subside, washed several times in boiling water, and then dried, we shall have a lake compounded of metallic and colouring matter, united by actual combination.

100 parts of metallic gold chemically combined with 250 of colouring principle.

If 100 grains of this lake be burnt, and the ashes fused with a little borax, a button of gold, weighing 40 grains, will be produced. A hundred parts of gold, therefore, carry down with them 250 parts, or twice and half its weight of colouring matter. The following facts will show, first, that the gold is in the metallic state in this compound; and secondly, that a combination takes place.

Proofs of combination.

Water is a solvent of the principles contained in dragon's blood: yet water takes nothing from this auriferous lake. Alcohol, which dissolves dragon's blood completely, takes nothing from this lake; it is not even slightly tinged by it. Potash takes from it a great portion of colouring matter, but it does not completely divest it of this; for there still remains a lake of a very fine purple, in which we find the gold united to the colouring principle. Three applications of potash were incapable of reducing the gold to its original purity.

 Ammo-

Ammonia takes some of the
does not dissolve it.

The muriatic acid at 10° h
this lake, even when it is fre
therefore, is not oxidated.

Nitric acid attacks it, and is
&c.; and the lake is reduced
of the colouring principle.

Lake with pine-bark.

The juice of pine-bark, whic
leather, affords likewise a lake
having all its properties; but
parts of gold.

Gold, in the metallic state,
combinations with the colouring

On the State of Gold in the

Is it metallic gold that colours glass and enamels?

We have seen, that gold is in
powder of Cassius; but does it
gives to glass and to enamel?
the present day, it is time to ex
least to throw some light on it,
take things up a little higher.

Long used for this purpose.

The practice of using gold, ei
as a purple pigment, is of lon
artists of the seventeenth centu
fulminating gold, the oxide pr
gold disoxidated by tin or mer
pumice stone, &c. Homberg
ed, that this metal gave a purple
what supported it when it was
burning-glass. Ronelle and Da
electric spark, likewise gave this

Difference of opinion respecting it.

From the time of Stahl to
chemists were divided in opinion
Some thought it too repugnant
to admit, that gold, furnished wi
could dissolve in glass, and fu
combination, of which there w
other metals. They concluded,
capable of colouring glass, but

its phlogiston; and Macquer could do no less than take this side of the question. It appears, however, observed he, that it is a kind of calcination which renders gold fitted for becoming vitrified. Macquer.

Others, with Orschal at their head, seeing on one hand the facility with which gold simply attenuated produces this colour; and on the other, the low temperature at which its oxides return to the metallic state, concluded, that to divide it well, was sufficient to render it fit for painting. Macquer himself at length adopted this opinion, and is the first who said clearly, "all these facts prove, the purple colour is "natural to gold, whenever it is in a state of extreme divi-"sion." If it be hereafter discovered, that this doctrine is well founded, let it not be forgotten in France, that to Macquer the honour is to be ascribed.* Orschal. Macquer.

I will recall, to the attention of chemists, some facts, which may guide us to the opinion, apparently the more accurate of the two.

Silver is incapable of being oxided by the simple heat of our furnaces; and its extreme readiness to return to the metallic state, when it has been oxided by acids, is another obstacle to overcome. Yet, when an easily vitrifiable substance can dissolve the oxide as fast as it forms, the oxidation, favoured by this attraction, becomes permanent, and supports a heat which is no longer capable of reducing it. The application of leaf silver on glass, its oxide combines with phoshoric or boracic acid, are known proofs of this. The case appears to be the same with gold. If a vitreous surface can dissolve its oxide as fast as it is formed, the reduction is delayed, and the purple maintains itself, till a higher temperature forces it to give up its oxigen. Properties of silver. Oxide of gold may be dissolved in glass.

But the following fact, known in glass-houses, seems to decide the question. Dissolve in soft flint-glass any precipitate of gold, the result will be a brilliant glass, perfectly transpa- Gold unites with glass, without colouring it.

* To ascribe to Macquer the honour of a discovery, which, according to our author, yet remains to be made, and respecting which he confesses Macquer only adopted the opinion of others, shows Prof. Proust to be more zealous for the honour of France, than for the cause of truth and justice.—T.

rent and colourless. Will it be said, that the gold is simply divided in this glass? If fragments of it be heated in a retort, and consequently far from any dephlogisticating vapour, they will acquire a superb purple tint, without lossing any thing of their transparency. Can this result be called a metallic reduction? The purple of enamels, and of painting on porcelain, frquently disappears, and afterward reappears with the greatest facility. Do we here recognize a metal that never deviates from metallic simplicity in its different states? gold, dissolved in glass, does, or does not, produce colours. This is a phenomenon in its history of which we know not the cause. Let us then honestly confess with Macquer, "this purple state "of gold is not yet well understood."

—yet this afterward becomes coloured when heated.

This state of gold not understood.

VIII.

Account of an Appearance of Brighton Cliff seen in the Air by Reflection. In a Letter from Dr. BUCHAN.

To Mr. NICHOLSON.

DEAR SIR,

Introduction.

ON reverting to some notes made at the time I found the subsequent account of the appearance, which I mentioned to you in conversation yesterday, and which you seemed to think worthy of recording. Perhaps you, or some of your correspondents, may favour the readers of the Philosophical Journal with an explanation of the optical principles on which the phenomenon depends.

The cliff was seen at sunrise, perfectly represented by an opposite reflection.

Walking on the cliff, about a mile to the eastward of Brighthelmstone, on the morning of the 28th of November 1804, while watching the rising of the sun, I turned my eyes directly towards the sea, just as the solar disk emerged from the surface of the water, and saw the face of the cliff on which I was standing, represented precisely opposite to me at some distance on the ocean. Calling the attention of my companion to this appearance, we soon also discerned our own figures standing on the summit of the apparent opposite cliff, as well as the representation of a windmill near at hand. The reflected

images were most distinct precisely opposite to where we stood, and the false cliff seemed to fade away, and to draw near to the real one, in proportion as it receded towards the west.

This phenomenon lasted about ten minutes, or till the sun had risen nearly his own diameter above the surface of the ocean. The whole then seemed to be elevated into the air, and successively disappeared, giving an impression very similar to that which is produced by the drawing up of a drop-scene in the theatre. The horizon was cloudy, or perhaps it might with more propriety be said, that the surface of the sea was covered with a dense fog, of many yards in height, and which gradually receded before the rays of the sun. It disappeared as the sun rose.

Perhaps such appearances may frequently occur at sea; a point your own experience may enable you to determine. Is there any analogy between this phenomenon and the celebrated *Fata Morgana*?

I am, Sir,

Your obdient servant,

A. P. BUCHAN.

Percy-street,
July 18, 1806.

Remarks. W. N.

The cliff to the eastward of Brighton lies very nearly in the direction S. 72° E. and the sun rose on the 28th Nov. 1804, S. 55° E. The rays of the rising sun, therefore, fell upon the cliff in an angle of about 73° from the perpendicular, on the left hand of the observer who was looking to the southward. Hence the cliff would be considerably illuminated, and the observer not much prevented by the direct solar rays from examining the phenomenon before him. Observations how the cliff bore and the rising sun.

This fact is certainly of great value, as a proof that the effects described under the denomination of *Fata Morgana* (of which an account is given, from Minasi, in the first volume of our quarto series), are not produced merely by light transmitted through the air, or reflected at small angles; but also by direct opposite reflection from some medium suspended in the air. We possess few authentic facts of this last description, and, I believe, no theory at all. Something, probably water, appears to be arranged in the air, perhaps by that slow and regular This fact of high value. Conjectures.

regular deposition that precedes crystallisation,* whic
rates on the light in a manner similar to that of an ex
plane surface, disposed nearly in a vertical position; a
rising sun appears to disturb and elevate this mass. I
what laws these causes and their effects may be go
must be explained by more frequent and minute obser
and, perhaps, from the analogy of experiment.

IX.

Letter from Dr. Wilkinson, *respecting the supposed duction of Muriatic Acid from Water by Galvanism.*

To Mr. NICHOLSON.

Dear Sir,

Four ounces of water lost two drams by galvanising and the remainder shewed muriatic acid.

AT the period Pacchioni's experiments were published lative to the production of muriatic acid from the gal disoxigenation of water, I was engaged in some lectur Dublin. I employed my extensive apparatus with a view o certaining this curious circumstance: my friend Dr. B lecturer on chemistry in that university, provided me some pure distilled water, the purity of which was ascert by the usual means with the muriate of barytes and the n of silver: about four ounces of this water was subject the influence of 800 plates for thirty hours: about two d of the water had disappeared: into the residual water, a tion of the nitrate of silver was introduced, the white c precipitation, evincing the existence of muriatic acid, i diately followed. The appearance surprized us much; withstanding this, I doubted the accuracy of the experi and suspected that muriatic acid had, some how or o crept into the water. Upon reflection, it appeared to me bably to arise from the acid mixture which is employed i galvanic arrangements. When at Limerick, I repeate

In another experiment, performed with extreme caution, no acid was produced.

* Could it have been in frozen plates? Even if so, the I phenomenon could hardly have been at so low a temperature ex

experiment, with the precaution of having my galvanic apparatus in one room, and my wires, passing through two small holes in a door to the distilled water, in an adjoining room. I employed a similar power as in the former experiment, and decomposed the same quantity: in this there were not the slightest traces of muriatic acid. I am induced to think that such has been the cause of Pacchioni's error. In a periodical work I noticed some experiments of Mr. Peel, of Cambridge. Mr. Peel says he discovered this acid after decomposing two ounces of water. A thousand galvanic plates in troughs, if even replenished every twenty-four hours with fresh acid and water, would not effect the decomposition of so large a quantity of water in three weeks. I wish the particulars of Mr. Peel's experiments, as to the extent of his power, and the nature of the fluid he employs, had been described.

Doubts and queries respecting Mr. Peel's experiments.

I am, dear Sir,

Yours, respectfully,

C. WILKINSON.

X.

Letter from H. HAMILL, *jun. Esq. on the Measure of a Ship's Velocity.*

To Mr. NICHOLSON.

SIR,

HAVING lately had occasion to look over the various methods of ascertaining the distance run by a boat, particularly in short distances, such as in surveying harbours, it struck me that the following might answer the purpose.

In the method proposed by M. Pitot, described in Gregory's Mechanics, vol. ii. page 414, two tubes are used, one of which has at its lower extremity, a trumpet mouth, the other straight throughout. Now when the vessel makes way, in his method, her velocity is ascertained by the difference of height to which the water rises in each tube. But as the velocity of a vessel, impelled by oars or sails, especially in a sea not perfectly calm, cannot be perfectly uniform, the water

Proposed improvement in Pitot's method of measuring a ship's velocity.

water in the principal tube must have an undulating motion, which will make the velocity difficult to be accurately ascertained.

Now let us suppose the upper orifice of the trumpet-mouth tube to be precisely on a level with the water in the other, and placed in a tub or receiver; when the vessel gets way, the water will rise and flow over, and be caught by the receiver. Now, Sir, I would suppose, that the area of the orifice being exactly found, the space of time in which the water received was flowing in being exactly noted, we might find the velocity of the boat much more accurately (in the above mentioned circumstances) than by the original method of M. Pitot.

As far as I have read, or inquired, I did not find this method proposed by any one. I now submit it to you, assuring you, Sir, that I am

Your most humble servant,

31, *Dominick-street*. H. HAMILL, Jun.

XI.

Facts relating to the Art of Shaving. From a Correspondent.

HAVING noticed some useful hints in your Journal, on the subject of razors, and their best state of application, I am desirous to contribute one or two observations, to add ease and comfort to their use and operation.

Instructions for shaving, and for preparing the razor.

In the preparation for shaving, I have found it the better mode of applying lather in a thicker state than is usually done, or if laid on thin, to allow it a few seconds to half dry on the face; and, in either case, to rub or brush well into the roots of the hair, by the hand, or by a brush of some hardness, which gives a very useful and moderate resistance to the operation of the razor. In this last article, too, I reverse the common practice, which is to prepare the razor before shaving; on the contrary, I have found it best, from repeated and long experience, to strop it after shaving, and lay it up in that state. It then

then requires only a little rubbing over the palm of the hand, and a dip in warm water, to make it ready for use.

The rationale of this mode I thus explain: The edge of a razor ought to preserve a perfect polish, and this is best done, by letting it lye with a small portion of an oily substance, such as generally is on strops; but which cannot be, it laid by with the vapour or moisture used in shaving, so readily favouring a disposition to contract rust. Rubbing on the hand takes off the small oily matter on the edge, and probably removes a wire edge, common on razors much honed, besides preparing it to receive a small additional warmth and temper from the warm water.

A considerable time ago a razor powder was advertised here, under the title of the "Egyptian Razor Powder."* It is not now publicly sold; but having on its first appearance procured a quantity, I am enabled to speak of it from experience. At first I used it, as directed, in a dry state on the strop; but for a long time past I have mixed it with a small quantity of oil, and found it the best composition for giving an edge to razors, of any I ever tried. For above three years I have never used a hone, nor had a razor ground, and now find a few motions over the strop, after shaving, fully sufficient to preserve my razor, (having never occasion to use a second), in good order.

A razor powder; composition not mentioned.

To some, this may appear a trifling subject; but with many, who, like the addresser, have found a stiff beard and bad razor, very grating attendants on declining years, it will be of some value.

Dublin, July 9th, 1806. H. K.

XII.

Account of the late Eruption of Mount Vesuvius.†

ON the 31st of last May, we enjoyed, for the first time, the

Late eruption of Mount Vesuvius.

* Was this the black powder obtained by triturating tin with a little mercury? I have heard such a powder praised, but have not tried it.—N.

† From the Moniteur of June 22, 1806.

 specta-

Late eruption of Mount Vesuvius.

spectacle of an eruption of Vesuvius. A column of very black smoke rose from the crater about ten o'clock; flashes now and then burst from this column; at length the eruption appeared in a mass of flame, of immense diameter, and occupying the whole vast extent of the crater. This mass was kept up by successive emission of whitish flame, which, as it rose into the air, assumed a more intense red colour. Ignited or melted substances, some of which were opaque, were projected with violence above this body of fire, and often fell beyond the circumference of the crater. At midnight there was not as yet any current of lava, but frequent rumblings were heard.

On the 1st of June, the eruption continued the whole morning, and we resolved to visit the mountain the following night.

We set out at eight in the evening. We took horse at Resina, near the descent to Herculaneum, and proceeded towards the residence of the hermit. The house in which he lives is situated near the southern peak of Mount Somma, being an easy ride of an hour and a half from Resina.

On leaving the hermitage, we proceeded across the valley which separates Somma from Vesuvius, and is known by the appellation of Atrio del Cavallo. It is of no great depth, being almost entirely filled with the lavas of successive eruptions, piled one above another. At length we reached the foot of Vesuvius, where we left our horses, and began to ascend on foot.

The declivity is very steep, and difficult of ascent, on account of the moveable nature of the ground on which you walk, being nothing but a mixture of ashes and fragments of lava, without consistency. After great fatigue we reached the summit, and arrived at one of the edges of the crater.

We had been lighted the whole way by eruptions of the mountain, which were projected to a very great height. Violent rumblings that were continually heard, added to the grandeur and the awfulness of the spectacle, which appeared much more beautiful and majestic from the point to which we had climbed with so much difficulty.

Suspended as it were on the brink of the crater, nothing interposed to prevent our view of the eruptions. We beheld immense masses of flame issuing almost from under our feet, rising above the clouds, and carrying with them, to the same height,

height, showers of ignited stones, which generally descended, nearly in a perpendicular direction, into the very mouth of the crater; but sometimes falling beyond its brink, rebounded around us, and rolled, red hot, down the declivity which we had climbed. Columns of fire, clouds of smoke, and showers of stones, succeeded each other, without interruption, accompanied by continual subterraneous noises; the bowels of the mountain seemed convulsed; the ground on which we stood shook, and threatened to sink beneath our feet. Never had we beheld a more melancholy image of the convulsions of nature, and notwithstanding the risk we incurred from the continual falling of the stones, we could scarcely be prevailed upon to leave it.

Late eruption of Mount Vesuvius.

Our guides, who were better judges of the danger than ourselves, now became alarmed, and urged us to descend.

The violence of the volcano had increased since we reached the summit, and the Power that presides over the place, seemed inclined to punish us for our audacity, and for having presumed to violate his tremendous abode.

We accordingly descended, and in a few minutes arrived at the Atrio del Cavallo. We were out of the reach of danger, and were enabled to contemplate, without apprehension, the objects by which we were surrounded. What an admirable spectacle! Over our heads, the volcano, with its smoking lava rushing down the sides of the mountain; before us, the sea smooth and calm; the full moon illumining with her mild beams the extremity of the horizon; the clouds and the smoke, wafted around the summit of the mountain, and concealing, for a few moments, the vast conflagration, which appeared again more lively and more brilliant; this succession of lights and shades, this contrast of turbulence and tranquillity, this solitude in the midst of such a vast convulsion, produced a multitude of contrary impressions, that cannot be described, but the recollection of which will never be erased.

We returned about four in the morning to Naples, having spent eight hours in the excursion.

On the second, the eruption continued the whole day with much greater violence than before; two currents of lava were formed; one of these stopped in the morning; the other, taking an eastern course, spread with great rapidity, and de-

Late eruption of Mount Vesuvius.

luged the plain. As our excur
not enabled us to form any
the lava, we set out again
phænomenon.

Passing through the villages
del Greco, we entered inclosur
corn-fields, into which the lava h
ed the current, and I was surpri
lava so different from the concep

I had always imagined that
sembling melted glass, ran in the
uniformly like a river of fire; a
bable that in a great number o
this appearance. On the prese
an accumulation of stones, so
magnitude, heaped one upon a
or twenty feet, and about half a
able mass advanced slowly, foll
solely by the falling of the di
there was no adhesion, and which
they had received on issuing fro
most elevated point and covered
In this manner the stones roll
the front rank having attained
produced it, began in its turn to
that came tumbling upon it.

All this intestine motion w
resembling the decrepitation o
and brisk. The fire was fed by
as sulphur, bitumen, and meta
their flames; but there was no
fusion, or of the commencement
the pressure of a stick, which I s
in vain, to thrust into them.

Meanwhile the devastation o
torrent, presented a horrid spec
ported the vines, and the vines th
extreme heat of this mass of
them; and the bright and clear
bustion, indicated the exact con

The walls of inclosures and of houses, calcined by the heat, crumbled to pieces before this moving mountain, or were thrown down by the force of the impulsion. Sometimes, however, instead of overturning an obstacle, the lava turned aside, and left it standing; for this variety of action it is impossible to assign any reason.

Late eruption of Mount Vesuvius.

After we had contemplated this dismal and astonishing sight, we went up to the convent of the Camaldulenses, situated on a kind of peak, of considerable height, which overlooks the whole plain, that extends from the south to the west, from the foot of Mount Vesuvius to the sea. This building has hitherto been spared, as well as the thick wood in which it is embosomed. It is one of the nearest points to Vesuvius, and that from which you are best able to discover and trace the progress of the lava. It is the asylum to which the wretched inhabitants of the desolated plain have often fled with their most valuable effects; to which they have driven their flocks, and conveyed their wives and children.

Here we staid a considerable time: our view extended over the declivity of Vesuvius, from which ran several currents of lava, that issued from the sides of the mountain; while enormous flames of fire, of which we had a nearer prospect the night before, darted continually from its summit. We had likewise a view of the plain, in which appeared the long windings of the rivers of fire. The reddish reverberation of the lava, and the conflagration in the plain, illuminated the landscape. On every side appeared the image of desolation: but yet it exhibited a picture so splendid, a scene so magnificent, that the ravages with which it was attended, were entirely forgotten in the contemplation of its picturesque and poetic beauty. In short, when my mind figures to itself those fiery torrents, the motion of the lava, the subterraneous thunders, those continual hissings, so many wonders, so many subjects of grief and admiration, I should think that a dream had deceived me, if the imagination, which produces such dreams, were capable of creating images so awful and so grand.

On the 3d, the eruption continued, and the lava still advanced; the detonations were louder, and more frequent, than the preceding day. In the evening, the flames shot to a still greater

greater height, attracting the
clouds, which emitted splendid
On the 4th, the eruption was
On the 5th, Vesuvius began to
are assured, announces the concl

XIII

SCIENTIFIC

Galvanic Societ

The Electro-micrometer of de Launay,

ON the 6th of Febuary last, gave an account of the Transacti xii and xiii. The electro-micron described by Marechaux as an the smallest perceptible quantitie ism, and capable of being appli cal researches.

—is an old invention.

This electrometer does not structed many years ago, and of in the quarto series of this Journa 1797. It consists in the applic which, by means of a screw, a side of the points of the gold l The Professor D. L. published de Physique for July 1804. and difficulty of communicatio contrivance, there will be no g merits of the latter inventor.

Craniologic S

Craniology.

Mr. Klauer, a sculptor of W Schiller and Weiland, in plaster modelled on his face after Weiland, during the stay of D

complaisance to permit his head to be modelled entire. The writer of this article adds, that if craniology should continue to be fashionable in Germany, it will be but a point of common prudence in all celebrated men to follow this example, in order that their craniums may be suffered to remain in their graves. The same artist, Klauer, has executed in plaster a cranium, furnished with all the protuberances and organs for the use of students of the theory of Dr. Gall. Craniology.

We also learn, that Dr. Kelch, professor of anatomy, at Konigsberg, has examined the cranium of the celebrated Kant, and finds that it is amply provided with all the protuberances and indentations which Dr. Gall has announced as indicative of the talents which that German philosopher has displayed.

Greek Manuscripts probably existing in Russia.

The St. Petersburgische Monathschrift, a monthly Journal, published in the capital of the Russian empire, contains an article which is highly interesting to literature. It relates to the progress of knowledge and civilization in Russia, from the earliest ages to the time of Peter the Great. The most striking part, is that in which considerable hopes are entertained that some of the Greek manuscripts, which are supposed to be lost, will be recovered in that empire. Kotzebue, in his periodical work, gives the following abridged account. Probability that many Greek MS. may be recovered in Russia.

Jaroslas I. son of the great Waladimir, caused a great number of learned men to come from Greece, and employed them to translate into Sclavonian those Greek books of which the originals were at Kiow, in the church of St. Sophia. Constantine was so much attached to the sciences, that he possessed more than a thousand Greek manuscripts, many of which were translated by his orders into Sclavonian, and distributed in the schools. Alexis Michalowitz being desirous of comparing the translations with the text, purchased in Greece, particularly at Mount Athos, about five hundred manuscripts, which are still at Moscow, in the library of the Synod. If it be allowed that this last collection may consist almost totally of bibles, or the works of the fathers, it may nevertheless Narrative respecting them.

be

Greek manuscripts.

be asked, with Mr. Kotzebue,
scripts collected by Constantine
what may have become of the
schools? and whether the still
Jaroslas I. be not still in the
desire expressed by this learn
vents in Russia might be ordere
libraries, will be sincerely adopt
ture. It is more than probab
remains of Greek literature woul

TO CORRES

To correspondents.

*From the great accession of Ori
necessary to take notice of seve
not been immediately inserted;
the next and the Supplementary*

1. *Account of the Treatment
ings of Furnaces: By Mr. John
the Claims of Robert Hooke, H
the principal Inventions in Chron
of Edinburgh.*—3. *New Exper
Heat in Liquids; with a D
of Rumford.*—4. *Experiments a
sions of the Particles of Water
Count of Rumford.*—5. *Remar
By David Brewster, A. M.
me that he has lately invented
measuring distances from two
facility and accuracy.*—6. *A
By Lieutenant William Collin
from H. B. K. on Galvanic and
Speculations from a Correspondent
in the Specific Gravities of Wa
new Method of rendering the
an Helical Spring equal in Dur
An Improvement in the inflammab
Singer.*

body form a continued ascending current, which carries the whole of the heat immediately towards the surface of the liquid; so that the strata of the liquid situated at a small distance under the hot body are not sensibly heated by it.

A cold Body cools a liquid downwards and not upwards.

When a solid body is plunged in a liquid which is hotter than the body, the particles of the liquid in contact with the body being condensed by the cooling they undergo, descend in consequence of the increase of their specific gravity, and fall to the bottom of the liquid; and the strata situated above the level of the cold body are not cooled by it immediately.

The viscidity of fluids prevents their particles from moving singly

It is true, that the viscosity of liquids, even of those which possess the highest known degree of fluidity, is still much too great to allow one of their particles individually being moved out of its place by any change of specific gravity occasioned by heat or cold; yet this does not prevent currents from being formed in the manner above described, by small masses of the liquid composed of a great number of such particles.

Currents in fluids.

The existence of currents in the ordinary cases of the heating and cooling of liquids, cannot any longer be called in question; but philosophers are not yet agreed with respect to the extent of the effects produced by those currents.

Conductors and non-conductors.

In treating of abstruse subjects, it is indispensably necessary to fix with precision the exact meaning of the words we employ. The distinction established between *conductors and non-conductors* of heat is too vague not to stand in need of explanation. An example will shew the ambiguity of these expressions.

Instance of heat transmitted

If two equal cubes of any solid matter, copper for instance, of two inches in diameter, the one at the temperature 60°, the other at 100°, be placed one above the other; the cold cube will be heated by the hot one, and this last will be cooled.

—through a metallic plate;

If the cold cube be placed upon a table, and its upper surface covered by a large plate of metal, of silver for instance, a quarter of an inch thick, and if the hot cube be placed upon this plate, immediately above the cold cube, the heat will descend through the metallic plate with a certain degree of facility, and will heat the cold cube.

—more slowly through a board,

If a dry board, of the same thickness with the metallic plate, be substituted in its place, the heat will descend through the wood, but with much less celerity than through the plate of silver.

But

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER, 1806.

ARTICLE I.

Inquiries concerning the Mode of the Propagation of Heat in Liquids. By BENJAMIN COUNT OF RUMFORD, *F.R.S. Read at the National Institute of France, 9th June*, 1806. *Translated by* W. CADELL.

THE motions in fluids which result from a change in their temperature, give rise to so great a number of phenomena, that philosophers cannot bestow too much pains in investigating that interesting branch of knowledge. **Motion of fluids by heat.**

When heat is propagated in solid bodies, it passes from particle to particle, *de proche en proche*, and apparently with the same celerity in every direction; but it is certain that heat is not transmitted in the same manner in fluids. **Passage of heat through solids.**

When a solid body is heated and plunged in a cold liquid, the particles of the liquid in contact with the body being rarefied by the heat that they receive from it, and being rendered specifically lighter than the surrounding particles, are forced to give place to these last and to rise to the surface of the liquid; and the cold particles that replace them at the surface of the hot body, being in their turn heated, rarefied, and forced up; all the particles thus heated by a successive contact with the hot body **An heated solid does not heat a liquid downwards.**

cles; and it is certain, if this conjecture is founded, liquids must necessarily become conductors of heat (though very imperfect ones) in all cases where this mobility of their particles is destroyed, as well as in these rare but yet possible cases, where a change of temperature can take place in a liquid without giving its particles any tendency to move, or to be moved out of their places.

In all ordinary cases they *transport* heat.

The unequivocal results of a great many experiments have shewn, that in ordinary cases, and perhaps in all cases where heat is propagated in considerable masses of fluids, its distribution is accomplished precisely in the manner that the new theory supposes, that is to say, by currents. And it is certain that the knowledge of that fact has enabled us to explain in a satisfactory manner several interesting phenomena of nature, which before were enveloped in much obscurity.

It is known that an hot body heats a fluid upwards, but can it do this downwards?

When a hot solid body is plunged in a cold liquid, there can be no doubt concerning the existence of the vertical ascending currents which are formed in the liquid, and which convey to the surface the heat which its particles have received; but with respect to the strata of liquid situated under the hot body, *are they, or are they not heated by this body, by means of a direct communication of heat from above downwards, from particle to particle, these particles remaining in their places?* This is a question on which philosophers are not yet agreed. As it is a question of great importance, I have long meditated on the means of deciding it; and after several unsuccessful attempts, I have at last succeeded in making an experiment which I think is decisive.

New experiment.

As the apparatus which I used for this experiment, and which I have the honour of laying before the assembly, is somewhat complicated; and as it is indispensably necessary to be intimately acquainted with it, in order to form a judgment concerning the degree of confidence which the results of the experiment may deserve, it is necessary to give a detailed description of this machinery. The annexed figure gives a distinct representation of its principal parts. It is drawn on a scale of a quarter of an inch to the inch, English measure.

A B, Fig. 2, Plate X. is a board, of oak, seen in profile; it is 1¼ inch thick, 18 inches long, and 11 inches in breadth. It serves to support two square upright pillars, C C, 18¼ inches in

in height, and 1½ inch square. They are firmly fixed in the board, at the distance of 11 inches asunder, and serve to support the two cross-pieces, D E, F G, at different heights.

Apparatus for experiments in fluids.

These cross-pieces are each pierced with two square holes, at the distance of 11 inches one from the other, into which the upright pillars, C C, enter, and the cross-pieces are supported at any height that is required, by means of a screw of compression. These screws are represented in the figure.

The cross-piece, F G, which is represented in profile, is 17 inches in length, and 1½ inch thick, and 3 inches in breadth. It is pierced in the middle by a cylindrical hole of 2 inches in diameter.

The cross-piece, D E, is 17 inches in length, by 1½ inch in thickness. It is 3 inches wide at each end, and 6 inches in the middle, where it is pierced by a circular hole 5 inches in diameter.

The cross-piece, D E, serves to support the annular vessel, H I, of which a vertical section, passing through its axis, is seen in the figure. This vessel, formed of thin brass plates, is 5 inches in diameter without, 3 inches in diameter within, and 27½ inches in depth. This vessel is filled with water during the experiments to the height of 2½ inches; and its form is such, that if the water that it contains were frozen into a solid mass of ice, this piece of ice would have the form of a tube, or perforated cylinder, of one inch in thickness and 2½ inches high, by 5 inches in diameter without. Its cylindrical cavity would be precisely 3 inches in diameter.

K L is a vertical and central section of a cylindrical vessel of tin, of 10 inches in diameter, by 4½ inches in depth. It is filled with water to the height of four inches, as it is seen in the figure.

The cross-piece, D E, is placed at such a height that the bottom of the annular vessel, H I, is plunged a quarter of an inch under the surface of the water contained in the great cylindrical vessel, K L.

In the axis of this last vessel is placed a small hemispherical cup of wood 2 inches in diameter without, and half an inch thick. It is kept in its place by a short vertical tube of tin, soldered to the bottom of the cylindrical vessel, K L, into which the stalk of the cup fits tightly.

The

Apparatus for experiments on fluids.

The middle of the cavity of this cup is occupied by the bulb of a small mercurial thermometer of great sensibility. Its tube, which has an ivory scale, is laid down horizontally, and fixed in one side of the cup, through which the tube passes, in such a manner that the lowest part of the bulb is elevated $\frac{1}{10}$ of an inch above the bottom of the cup. The diameter of the bulb being $\frac{3}{10}$ of an inch, and the hemispherical cup having $\frac{1}{2}$ inch of radius within, it is evident that the upper part of the bulb is $\frac{1}{10}$ of an inch below the level of the brim of the cup that contains it. To avoid charging the figure with too many details, the scale of the thermometer is not drawn, but the tube is distinctly represented.

The horizontal cross-piece, FG, serves to support a very essential part of the apparatus which remains to be described.

This cross-piece supports, in the first place, a vertical tube of wood, M, 6 $\frac{6}{10}$ inches in length, and 2 inches in diameter without. Its interior diameter is 1 $\frac{1}{10}$ inch. This tube is supported by a projecting collar (represented in the figure) $2\frac{1}{2}$ inches in diameter, which rests on the cross-piece, FG. It is a vertical and central section of this tube that is represented in the figure, and it is dotted, in order to distinguish it from the surrounding parts of the apparatus.

The lower part of this tube is plunged $\frac{6}{10}$ of an inch under the surface of the water in the large cylindrical vessel, K L; and it is placed precisely above the wooden cup in the prolongation of its axis, the lower extremity of the tube being at the distance of $\frac{1}{10}$ of an inch above the horizontal level of the brim of the cup.

On the top of the tube of wood is placed a cylindrical vessel, N O, of sheet brass, 3 inches in diameter, $2\frac{1}{4}$ inches high; which has a lateral spout, P Q, placed a little above the level of its bottom.

From the middle of the bottom of this vessel there descends a cylindrical tube of brass, 6 inches in length, and 1 inch in diameter, which ends below in a hollow conical point, as represented in the figure.

R S is a vertical and central section of a funnel of brass, which ends below in a cylindrical tube of $\frac{3}{10}$ of an inch in diameter, and 6 $\frac{6}{10}$ inches long. This funnel is kept in its place, in the axis of the cylindrical vessel, N O, by the exact fitting of its upper edge upon that of the vessel into which it is adjusted.

The

The lower end of the tube of this funnel is surrounded by a projecting edge, or flanch, in the form of a hollow inverted cone. The diameter of this conical projecting brim above, at its base, is $\frac{7}{10}$ of an inch, and it is soldered below to the end of the tube.

Apparatus for experiments in fluids.

When hot water is poured into the funnel, this liquid, descending by the tube of the funnel, strikes against the inner surface of the hollow inverted cone, which terminates the vertical tube that belongs to the vessel, N O, and then rising up through this last tube into that vessel, it runs off by its spout. It was with a view to force this water to come into more intimate contact with the hollow cone, that the projecting edge, in form of an inverted cone, was added to the lower end of the tube of the funnel.

The object chiefly in view in the arrangement of this apparatus, was to give to the conical point which terminates the vertical tube of the vessel, N O, an elevated temperature, which should remain constant during some time, for the purpose of observing if the heat, which must necessarily be communicated by this metallic point to the small quantity of water with which it is in contact, and which is confined in the lower part of the wooden tube, M, would descend, or not, to the thermometer which was placed in the wooden cup.

There was still one source of error and incertitude against which it was necessary to guard. The heat communicated through the sides of the wooden tube to the water contained in the great cylindrical vessel, K L, might be transported to the sides of that vessel, and being then communicated from above downwards through these sides, might heat successively the lower strata of the liquid, and at last that stratum in which the thermometer was.

It was to prevent this, that the annular vessel, H I, was used; and it performed its office in the following manner.

The particles of water contained in the great vessel, K L, which, being in contact with the exterior surface of the wooden tube, were heated by that tube, could not fail to rise to the surface, and there they necessarily came into contact with the interior sides of the annular vessel to which they communicated the excess of heat they had received from the wooden tube.

This

This heat, passing readily th
that vessel, was given off as fas
ticles of cold water contained i
tact with its sides, and these p
the water contained in the an
their acquired heat and levity,
the wooden tube to the sides
interrupted; and all the hea
sides of the wooden tube was b
such a manner that it could no
the experiment, nor affect the c

The apparatus and experiment. A conical metallic piece pointing downwards in water, was kept hot. It gave heat to a small portion of water confined by a wooden cylinder, open beneath to a larger vessel. Provision was made to prevent heat being carried down by the sides of the vessel. A thermometer at a small distance below was not affected.

Before I proceed to give an a
quiry, I shall take the liberty t
sembly to the most important
ment.

On pouring boiling water in
into the funnel, the hollow coni
vertical tube belonging to the
kept at a constant temperatu
water.

This point was surrounded by
tained in the cavity of the lowe
as this water could not change
the surrounding cold water, b
the sides of the wooden tube, it
hot in a short time.

But this small quantity of h
a stratum of cold water, which
the thermometer, placed direct
only half an inch.

If heat could pass in the wa
would no doubt pass from the
contained in the open end of t
the thermometer, which lay in
small a distance.

Three experiments were ma
always with exactly the same r
of boiling water was poured into
In the second, during 12 minut
minutes.

The thermometer, whose bulb was in the wooden cup, remained *at perfect rest* from the beginning of the experiment to the end of it, without shewing the slightest sign of being in any way affected by the hot water which was so near it.

These experiments were made at Munich, in the month of July 1805, the temperature of the air and of the water contained in the vessel, K L, being 70° Farenh.

A small thermometer, placed in the water, contained in the annular vessel, H I, in such a manner that its bulb was scarcely submerged, marked that this water had received a little heat in each of the three experiments.

Another similar thermometer, placed in the water contained in the large vessel, K L, immediately under its surface and near one side of the vessel, shewed that this water had not acquired any sensible increase of temperature during the experiments.

The experiment shew that heat does not pass downwards in fluids free to circulate.

From the results of these experiments we are authorized to conclude, that heat does not descend in water to a sensible distance, in cases where the particles of the liquid which receive heat are exposed to be displaced and forced upwards by the surrounding colder and denser particles; that is to say, in all the cases (and they are the most common) where heat is applied to the strata of the liquid situated under its surface.

But they do not prove that fluids cannot conduct directly.

But the results of the experiments in question do not prove that heat cannot in any case descend in water; and still less can it be inferred from them, that all direct communication of heat, in this liquid, from particle to particle, *de proche en proche*, is impossible. They do not even prove that heat did not descend, *to a small distance*, below the level of the end of the wooden tube, in these experiments; for it is certain that that event could take place without the thermometer, which was situated a little lower, being in any way affected by that heat.

The fluid above the thermometer was not quiescent.

The particles of water, situated at a very small distance below the level of the lower end of the wooden tube, being heated by the stratum of hot water which rested immediately on them, might have been displaced by the surrounding colder and denser particles, and forced to rise to the surface; and these last being in their turn heated, forced upwards and replaced by other cold particles, it is evident that the heat could not make its way downwards so far as to arrive at the thermometer

meter through a stratum of liquid
rest, was nevertheless in part c
were continually changing.

Opinion of the author that the imperfect conducting power of fluids is owing to the mobility of their parts.

I have long suspected that the
direct communication of heat bet
of fluids depends solely on the g
cles (see a note, p. 202, tom. ii.
London, 1800); and if this su
certain that when this mobility ce
on it must cease likewise.

Not of their particles; but of sensible portions or parts.

When I speak of the mobility
amongst each other, I am very f
ed) from supposing that individu
tion. I was formerly of that opi
vestigation of the phenomena h
mistaken. But although one indi
never be put in motion in conseq
cific gravity occasioned by a cha
cannot happen to a single particl
cessarily, happen to small masse
great number of these partic
proved by the currents which are
tact of a hot or cold body plung

The particles of fluids have much adhesion.

The force by which the partic
is very great; and it is more than
of many very interesting phenom
the suspension of the heavy bodi
so frequently hold in solution.

Estimate of its quantity.

From the result of an experim
ago, in order to determine the m
want of perfect fluidity in wate
Farenh. I found reason to conclu
surface equal to 368 square i
only one grain troy more than an
remain suspended in that liquid
easy to find by calculation, wha
a small solid spherule of the hea
stance, in order to its remaining
quence of the viscosity of that liq

Having made this calculation, in order to satisfy my curiosity, I found that a solid spherule of pure gold, of the diameter of $\frac{1}{300,000}$ (or exactly $\frac{1}{306719}$ of an inch), ought to remain suspended in water in consequence of the adhesion of the particles of that liquid to each other. But I shall return to this subject on a future occasion.

Spherule of gold which would not work in water.

II.

An Account of the Invention of the Balance Spring, and the Determination of the Conditions of its Isochronism in wide and narrow Vibrations, by Robert Hooke, in the 16th Century, and of the first free Escapement by Du Tertre; together with various other Historical Details relative to Timepieces. In a Letter from Mr. THOMAS REID *of Edinburgh.*

To Mr. NICHOLSON.

SIR,

IN your Journals of late, I see you have given an account of the detached 'scapement, such as it is now generally applied to chronometers or timekeepers, and also of the different forms, and the properties of the pendulum spring, both as explained by Messrs. Arnold and Earnshaw to the Honourable Board of Longitude.

Reference to Arnold and Earnshaw's timepieces.

I beg you will allow me, through the channel of your interesting and useful Journal, to give you some account of the invention of the pendulum spring and of its properties, by their author, our countryman, the celebrated Dr. Hooke. I have been often provoked to see his name so much kept in the background in regard to these matters, and particularly by foreign artists, who, whenever they have occasion to speak or make mention of the pendulum spring, enter much on the merits of this important invention, and are full of enthusiasm in praise of M. Huyghens, for having made this wonderful discovery. M. Huyghens was undoubtedly one of the most profound geometricians that any age has produced, and Dr. Hooke must certainly be allowed to have been one of the greatest mechanicians; a bare recital of whose mechanical inventions are of

Robert Hooke, the inventor of the pendulum spring unjustly neglected.

Huyghens. Reasons against Huyghens' claim to this invention.

 themselves

... sufficient to form a catalogue. Dr. Hooke ... even by foreigners, to have been the first ... pendulum spring to the balance of a watch; but ... was straight in its form, and that it was M. Huyghens ... afterwards made the great improvement in it, or rather ... it, by that of giving it the spiral form. This ... has been scrutinized before, and the dispute which Dr. Hooke had with Mr. Oldenburgh on this subject is very well known. Yet there are many circumstances that seem to have been overlooked, which carry along with them such strong arguments in favour of Dr. Hooke, that I am much surprized how they have been passed over and never noticed by any of his friends. Had M. Huyghens been the genuine inventor of the pendulum spring, which I confess, from all circumstances taken together, I think there are strong reasons to conclude against; had he been really the inventor, I say, it is much more than probable that he would have seen its properties as well as Dr. Hooke did, and would have published them; and this might have prevented the serious quarrel that afterwards took place between two very celebrated and rival French artists, M. Le Roy and M. Berthoud. Extracts from them on this subject of quarrel I shall afterwards give you, and in the meantime shall state Dr. Hooke's case, with extracts from him; which, although they came not out until immediately after M. Huyghens had published his account of the pendulum spring about the years 1674 or 1675, are sufficiently conclusive. Dr. Hooke was so much hurt with it, that he gave such a full account of his experiments, and so complete a demonstration of the principle or properties of springs, that it is evident, that the subject was not *new* to him.

Galileo invented or first recommended the pendulum.

It is necessary to pay attention to dates. Galileo died in 1642, and had given an account of the equality of the wide and narrow vibrations of the pendulum, and strongly recommended it to astronomers, as infinitely preferable to the balance, which they were attempting to use in their observatories. Riccioli pressed this matter exceedingly; and it came into general use as a measure of time, the astronomers patiently sitting by it and counting the vibrations.

Robert Hooke's introduction at Oxford.

In 1655, Mr. Robert Hooke came to Oxford as a poor scholar,

scholar, and brought with him a number of mechanical nick-nacks which he had made at home. His mechanical genius soon made him known to the members of the invisible society there, who employed him to work for them, making apparatus for their experiments. Dr. Ward, afterwards Bishop of Salisbury, took a liking to him, and instructed him in mathematics and astronomy. He urged him to try his mechanical genius in contriving a 'scapement pendulum. It would appear that they found the 'scapement for a balance, which had long been in use, did not answer, probably because it required very wide vibrations, which were found not so equable; and Mr. Hooke invented this sometime before February 1656; for there are observations of a solar eclipse made in that month, at Oxford, by a pendulum clock.

Mr. Hooke got Riccioli's book from Dr. Ward, where mention is made of the proposal of Gemma Frisius to discover the longitude by a timekeeper; this he immediately proposed to do by a pendulum clock. But it is very remarkable, that Hooke had mathematics enough to see that even the smallest vibrations were not isochronous unless of equal width, although Galileo had asserted that no difference would be observed. Another remarkable instance of his great genius is, that though then only twenty-one years of age, he saw that every branch of human knowledge had a system of its own, and a set of principles on which it was regularly founded; and he said it was only by studying even *shoemaking* in this way, that one could be certain of improving it. He had already begun to form systems on the different subjects which had interested him. He called them algebras, because they enabled the possessor to invent and discover new things in their own line by rule, and with certainty of succeeding. Mechanics always was his favourite; and his mechanical algebra, or method of mechanic invention, he always considered as his greatest treasure. He says that no problem could be proposed to him in mechanics, but his algebra would immediately tell whether it was possible, and would put him and keep him in the right road for solving it. He told Dr. Ward that his algebra plainly showed him, that the only thing that could make equal vibrations, was an accelerating force proportional to the distance from the place of rest, and this was not true even of the smallest arches of a pendulum. But he had not mathematics

Attends to timekeepers.

His systems for inventions called by him algebras.

He shows the law of isochronal vibrations;

enough

enough to discover the cycloid, t
ful guess at it, and one of the best
has been given. He says that
you could make the pendulum s
The small arches *ab*, *bc*, *cd*, *de*, &
equal, the perpendicular heights
—which was afterwards fully investigated by Huyghens.
must increase as the numbers 1,
cisely the property of the cycloi
afterwards by M. Huyghens.
was speculating on this subje
been at Oxford, kept up a corr
there. Huyghens being a comm
investigated the motion of the pe
conceived the project of getting a
father was a member of the Sta
this very year that they offered a
is not improbable but he knew o
The first clock of Huyghens, with a pendulum.
a means of procuring this act o
1657, he (Huyghens) presented
cloid) to the States. It is not im
son, aiming at a monopoly and a
his secrets, or that Mr. Hooke,
of what he was doing. It is m
ments and projects of the curi
never made any secret of their
because their meetings were su
by Cromwell's soldiers) might
know for certain that Mr. Olde
cate every thing to his countrym
ghens at this time thought of no
Hooke, in 1658, discovered the isochronism of springs.
lum, nor for several years after.
about for a force proportional
of rest, and found, experiment
perty. He kept this a secret, b
following cypher: "*ce, i ii, n o*,
wards (in 1661) explained, "*U*
tension, so is the force. He to
secret for constructing pocket
showed him a watch, which was
within half a minute a day; a th
then known.

Immediately after the Restoration, Mr. Boyle recommended Mr. Hooke to the Duke of York (who was very fond of sea affairs), to Lord Brouncker, and Sir Robert Moray, the most eminent at that time for mathematical knowledge; and proposed to Mr. Hooke to apply for a patent. Charles the Second founded the Royal Society, and enjoined the members to turn much of their attention to the improvement of navigation, and established the Greenwich Observatory for this very purpose; and the Parliament decreed a reward of £2000. Oldenburgh became secretary of the Royal Society, and kept a close correspondence with Huyghens, both public and private; and Huyghens was elected member in 1662 or 1663. He was in England in 1663, and was much caressed by all the learned, and particularly by those now named. During all this time there was not the least mention of his longitude watches; and Hooke's was kept a secret for reasons now to be explained. Foundation of the Royal Society, &c.

Lord Brouncker, Sir Robert Moray, and Mr. Boyle were so much convinced of the superiority of Hooke's watches, that they aided him in procuring a patent. A warrant for one was actually signed by the king's orders for fourteen years.* Now these three gentlemen joined with Mr. Hooke in the prosecution of the affair, perhaps contributing the money wanted for carrying on the business of watch-making; and it was then that Mr. Hooke invented the engine for dividing and cutting clock and watch wheels now in universal use. Mr. Waller had several drafts of the mutual agreement, with various changes of the terms. It appears from them, that these gentlemen were to procure an act of Paliament for a duty of a groat per ton on all shipping sailing from any English port. And it was provided, that if the profits should exceed £6000, Hooke was to have three-fourths, and they to have the remainder. If it amounted only to £4000, he was to get only two-thirds, &c. It does not appear that all this while that Hooke disclosed his secret to them, further than by subjecting the watch's motion to their examination, along with Bishop Ward and Dr. Pell, and assuring them that the secret was contained in the cypher which he had long ago given to Mr. Boyle. But about the History of Hooke's discovery of time-pieces, and treaty with Lord Brouncker, Sir R. Moray, and Boyle.

* This patent was in possession of Mr. Waller, secretary of the Royal Society in 1705.

end

He disclosed his secret (in part), and was afterwards unworthily treated.

end of 1660, it appears that things were brought to suc bearing that he explained the cypher, and even showed construction of the watches. For in the register of the R Society it is recorded that Mr. Hooke had exhibited his poc watches, which were *regulated* by springs. A pocket wa can be moved no other way but by a spring; and therefore word *regulated* must undoubtedly apply or refer to the *re lation* of it by means of a pendulum spring. But the assoc tion now broke up. The three gentlemen insisted on anotl condition, that if they, or any other person, should remarkal improve this watch by any new principle introduced into they should be at liberty to enjoy the profits of the impro ment even during the currency of the patent. This Ho flatly refused, saying, that if once he showed them the princi it was easy to improve on it; and he himself saw several i perfections in it, arising from the very nature of metals, whi he was labouring to remove; and as he had no intention excluding them from the benefits of any improvements of l which might perhaps be still necessary before the watch good for any thing, he would not be excluded from the adva tages of any other improvements made on his invention. I not unlikely that he had thoughts of the effects of heat and c on the watch, and was thinking of adding a compensation pi of some sort or other to it.

The patent spring abandoned.

By this means the affair of the patent broke up and miscar ed; Hooke being exceedingly disgusted, and his manners bei extremely ungracious and fretful, probably displeased t partners as much. He became extremely close and jealo after this.

Disclosure of the invention of the balance spring by Huyghens: contested by Hautefeuille.

It was not till about 1674 or 1675, that Huyghens pu lished his discovery of the spiral spring, applying both to t States of Holland and the Court of France, for such an ext tion of his patent as should comprehend watches. He w opposed from all quarters: the watchmakers allowed him monopolize the pendulum, which they thought he had some measure invented; but they did not choose his encroac ment on watches. The Abbé Hautefeuille had also discove the regulating power of a spring, and claimed the inventio his opposition was so effectual, that the registration of t French patent was stopped, but the Dutch patent was co plete

pleted, and M. Huyghens tried all methods to get it extended to England, but it was there opposed. M. Leibinitz was in Paris at the time of Hautefeuille's process, and says he was cast: this may be so, and still the patent might not be granted to M. Huyghens.

Communication of Hooke's discovery by Oldenburgh.

The first appearance of M. Huyghens' claim was about 1665. Sir Robert Moray, in a letter dated Oxford, 30th of September, 1665, to Mr. Oldenburgh, presumes, that from his intimacy with M. Huyghens, he will be among the first to hear of his watches, and desires him to ask him, whether he does not apply a spring to the arbor of the balance? This will bring M. Huyghens to say something of the matter; and if you find from his answer that this is the case, you may then tell him what Mr. Hooke has done in this way, and that he promises still more. Here it appears that Hooke's secret was in some degree known; and as Sir Robert had no longer any interest in the secret, he gives Oldenburgh leave to communicate it. Hooke complained much to the Society of these communications of their secretary.

Resumption: That Hooke appears to have invented the balance spring many years before Huyghens.

From this account, which is all founded on well authenticated facts, and does no great honour to the three gentlemen, it is plain, that Hooke had invented the regulating spring as early as 1658 or 1659; although perhaps he had not then given it that form which it now bears. His first watches were furnished with two cork-screw or cylindrical springs,* which acted on the balance arbor by a silk fibre or thread lapped round it. It is extremely probable that Huyghens knew of Hooke's discoveries in general, although it cannot be said with any certainty that he borrowed or stole the invention from him. Yet Hooke frequently charges him with this theft, but without being able distinctly to support the charge. Huyghens did not publish it till 1675, or thereabouts.

Hooke also invented the revolving pendulum, and a balance of the same kind.

These circumstances I think fully establish Hooke's claim to the invention of pendulum or balance springs; and that they were invented by him like a man of science, from principle, and not by chance discovered. I would also observe, that Mr

* Cylindrical springs were used by Mr. Harrison in some of his essay timekeepers; and long afterwards, the late Mr. Arnold obtained a patent for them.

Hooke was long before him in the invention of the circular or conical pendulum, which he introduced for philosophical purposes, to represent the motion of the planets, and had proposed this pendulum regulated by springs instead of gravity, for a time measurer, before either Huyghens or himself thought of the balance spring. It was to consist of two balls, A B, exactly balanced round a centre, C, in the axis, C D, Fig. 3. Plate XI., when this was set a whirling round the axis, the balls would fly out at right angles at once, but they were to be prevented by a spring, E F G, coiled round the centre, G,* and so tapered as to produce an isochronous circulation, although the maintaining power should vary the width of the revolutions. This was exhibited at Oxford, in 1657, but did not answer; but it shows that Hooke was well acquainted with the force and theory of springs. Nay, in 1660, he published his *Micrographia*, where there is occasionally mentioned a curious and paradoxial theory (as it then appeared) about the forces being as the squares of the velocities, instancing a great number of cases, among which are expressly mentioned bows and other elastic bodies, whose forces are proportional to their tensions. In 1676, Dr. Hooke published *A Description of Helioscopes and some other Instruments*, to which he has a *Postscript*; in which, among other things, he says, "At the "earnest importunity of a dear friend of mine since deceased, "I did, in the year 1664, read several of my first Cutlerian "lectures upon that subject (meaning the longitude) in the open "hall at Gresham College; at which were present, besides a "great number of the Royal Society, many strangers unknown "to me. I there shewed the ground and reason of that application of springs to the balance of a watch, for regulating its "motions, and explained briefly the true nature and principle "of springs, to shew the physical and geometrical ground of "them. And I explained above TWENTY SEVERAL WAYS "by which springs might be applied to do the same thing, and "how the VIBRATIONS MIGHT BE SO REGULATED, as to "make their durations either all *equal*, or the greater, SLOWER, "or QUICKER than the less, and that in any proportion as-

Hooke, in his Cutlerian lectures, in 1664, explained numerous ways of applying springs to the balance, and rendering its vibrations equal in duration;

* Here we see the spiral spring applied to this machine; and it would be no difficult matter, after this, for Dr. Hooke to apply it to a watch balance.

"signed,

"signed. Some of these ways were applicable to lesser vibrations, others to greater, as of 2, 3, 4, 5, 6, or whatever number of revolutions was desired: the models of which I there produced; and I did at the same time shew wherein the aforesaid sea clocks (meaning Huyghens') were defective.

"All these particulars also were at several other times, at the public meetings of the Royal Society, discoursed, experimented, and several models produced. I did also, at the earnest desire of some friends, in the years 1664 and 1665, cause some of the said watches to be made, though I was unwilling to add any of the *latter applications of the spring* to them, as waiting a fitter opportunity for my own advantage."

—and before the Royal Society.

In 1678, Dr. Hooke published *Potentia Restitutiva*, or Spring; and says, "The theory of springs, though attempted by divers eminent mathematicians of this age, has hitherto not been published by any. It is now about *eighteen years* since I first found it out, but designing to apply it to some particular use, I omitted the publishing thereof.

"About three years since, his majesty was pleased to see the experiment, that made out this theory, tried at Whitehall, as also my spring watch. About two years since, I printed this theory in an anagram, at the end of my book of the *Description of Helioscopes*, viz. *Ut tensio sic vis*. That is, The power of any spring is in the same proportion with the tension thereof: that is, if one power stretch or bend it one space, two will bend it two, and three will bend it three, and so forward. Now, as the theory is very short, so the way of trying it is very easy." Then he proceeds with describing his manner of proving both the *cylindrical and the flat helix*, and he even tried *straight wires*. The apparatus to which he applied his flat spiral springs, in order to prove or show their isochronism, differs little or nothing from the *elastic balance*, as M. Berthoud calls it, and which he boasts much of having invented, in order to prove *his theory of pendulum springs*, which he forms in such a way, that when bending them up equal degrees of tension, they shall have their forces in an arithmetical progression, which is just what Dr. Hooke, above an hundred years before, shows he had invented and done.

Other points of the history.

Dr. Hooke used an instrument for springs, since re-invented by Berthoud

Le Roy observed the property on which the isochronism of springs depends, 100 years after Hooke.

M. Le Roy the eldest, in his *Memorial on the best Mean of Measuring Time at Sea,* which was published in 1770, at the end of the *Voyage by M. Cassini,* gives us, among other improvements which he had made use of in his timekeeper one, which regarded the isochronism of the pendulum spring and which would have remained, as he says, hitherto unknown, had he not discovered this theory. His own words are, "It is "only some time ago, that I have at last discovered, as I shall "more particularly explain, this important fact, which hence "forth must serve as a basis to the theory of watches, and as "a guide to the workmen to construct them; namely, *that "there is in every spring of a sufficient extent, a certain length "where all the vibrations, great and small, are isochrone: that this "length being found, if you shall shorten this spring, the great "vibrations will be quicker than the small ones: if, on the con "trary you lengthen it, the small arcs will be finished in less time "than the large ones.* It is from this important property of "the spring, hitherto unknown, that particularly depends, as "we have by it seen, the regularity of such a time watch. "After what has gone before, we perceive that the justness of "watches depends in a great measure on the length given to "the spiral or regulating spring; if with the same 'scapement "certain watches, or such as have, for example, the cylinder or "horizontal 'scapement, go ill, whilst that others of the same "sort are very regular, we here see the cause of it. This new "observation may be of great use in the construction of clocks, "whether small, or with second pendulums, where the pendu "lum is suspended by a spring: indeed, we see from what has "been said, that there ought to be *there,* such a length in the "suspension spring, where all the vibrations of these pendulums "may be made isochrone."

Resumption of Hooke's claims.

It is evident, from what has been stated, that Dr. Hooke has the fairest claim to the honour of these important inventions and discoveries. I mean that of the watch pendulum spring which he seems to have made in every *possible form.* And that he was master of the theory of springs, about which so much work of late years has been made, is equally evident. Among other reasons why he did not sooner publish an account of these inventions and discoveries, there is one, which though not generally known, it may be proper to mention here. After the dreadful conflagration, by which the greater part of Lon-

London was destroyed, 3d of Sept. 1666, Dr. Hooke was much engaged in surveying the waste ground left by the fire, and arranging the different claims and properties of the public and for individuals; by which it is probable he got much more money than he would have got by prosecuting the business of his longitude watches. I shall now give you some extracts from M. Berthoud, by which you will see what a serious quarrel took place between him and M. Le Roy, and, among other things, about the theory of pendulum springs, &c.

"I pass," says M. Berthoud,* "to a discovery of which M. "L. R. seems extremely jealous, that of *the isochronism by a* "*certain length of the spiral spring*, which he had proposed in "enigma, in 1768, in his *Exposé Succinct*, and which he "only divulged in 1770, in his *Mesure du Temps en Mer*: "he holds to it there, with so much the more reason, as he "is persuaded, that on this property of the spring, the regu- "larity of his marine watch particularly depended. I do not "dispute with M. L. R. that he has not discovered this "property of the spiral spring, by which *all the vibrations of* "*the balance* become *isochrone*: but I complain, that with "propriety and a just right, that he wants to dispute the dis- "covery of its having been made on my part; and dares to "accuse me of being only his copyist: it is very easy for me to "prove that I could not be so.

Account of the controversy between Le Roy and Berthoud respecting timepieces.

"The ill-timed jests that M. L. R. allows himself, and the "tone of raillery which he affects on this occasion, shall not "prevent the truth of that which, in my *Essai sur l'Horlogerie* "(tom i. page 168), I have said in speaking of my *elastic* "*balance*, and is this which follows:" "I had destined this "machine to make experiments on *the duration of great and* "*small vibrations of the same balance, which moves freely*: for "this purpose, I made the end of the pivot run on a very hard "stone; and to lessen the friction of the pivots, they each run "between three rollers. I observed the number of vibrations "which the balance made when it moved horizontally or ver- "tically, the velocity of the vibrations according to *the dif-* "*ference of temperature; and it must serve to measure the dif-*

* In his "Eclairssemens sur l'Invention et la Construction des "Horloges Marines."

"*ferent*

...ent of the ...oversy ...en Le Roy ...Berthoud ...ecting ...pieces.

" *ferent degrees of force of the same spiral, accordingly as it is* " *more or less bent up.*"

" The experiments which I pointed out are pretty clearly " designed, so much so, that they might assist artists " who know how to make use of materials when set before " them. *The duration of the great and of the small arcs of* " *vibration of the* FREE *balance*, and *the different degrees of* " *force of the same spiral accordingly as it is more or less bent* " *up.* Here is the origin of my theory on the *isochronism of* " *the vibrations by the spiral.* I had no occasion to make " use of it, when I wrote the first part of my *Essai sur* " *l'Horlogerie*, because the oscillations of the regulator, in " my first marine clock, were necessarily isochrone, from the " construction of the machine; first, because the 'scapement " corrected the inequality of the time or duration of the great " and small vibrations, which might result from changes in the " motive force, from variations of friction, and from the thick- " ening of the oil, &c. (*Essai*, No. 2116); secondly, because " the regulator being composed of two balances, the agitation " of the ship could not change the extent of the arcs of vibra- " tion, (see *Essai*, No. 2097). Thus, in all cases, the dura- " tion of the great or small vibrations must be equal, whether " by the assistance of the 'scapement, or by the nature of the " regulator. But since I had suppressed one of the ba- " lances, it became necessary to seek to correct the inequalities " of vibrations, which might result from the agitation of the " ship, and which were corrected by the double balance in " the first construction. I came back then to my original " ideas, and sought to correct by the spiral the alteration " which the agitations of the ship might produce on the extent " of the arcs, and on the inequality of the vibrations. Such " is the origin of my theory of the spiral: a theory which is " my own, as is obvious, and very easy to see; and I do not " dispute with M. L. R. his having made the discovery on " his part also. We must remark, however, an essential " difference between the importance which he attaches to " the property which the spiral spring has, of rendering the " vibrations isochrone, and the utility of which I have thought " that this property might be. It is *chiefly on the isochronism by*

" *the*

Account of the controversy between LeRoy and Berthoud respecting timepieces.

" *the spiral*, that M. L. R. founds, as he tells us, the just-
" ness of his marine watches; whilst I have never considered
" this property of the spring but as an useful accessary;
" and the justness of my marine clocks depends so little on
" it, that my clock, No. 8, whose spiral was not isochrone,
" has however succeeded very well in two trials of a year
" each.

" But I will suppose that I had not announced, in my
" *Essai*, the experiments which have led me to the disco-
" very in question; at least, M. L. R. will not deny that,
" *the 10th of February*, 1768, I deposited, or lodged with
" the Academy, my *new theory of the spiral*, in which I deduc-
" ed this proposition: *the oscillations of any balance whatever*
" *may be rendered isochrone by the spiral.* Nor can he deny
" that the publication of his *Exposé Succinct* was posterior
" to the date of my deposit; he would not then be well
" founded, to say that I have borrowed this theory from
" him, or the idea of the discovery, when even, as he falsely
" pretends, this discovery had even been divulged in his
" *Exposé Succinct*. But where do we find it *divulged* there?
" How has he announced it there? Here is all that he
" says of it (page 27 of the *Exposé Succinct*):" " I have
" discovered a property in the spring, by means of which
" I can easily come at the most perfect isochronism."

" What could these enigmatical words teach me? What
" is this *spring?* What is this property? We find, at the
" end of the *Exposé Succinct*, a copy of his project of 1754,
" in which he likewise said, that his balance would be sus-
" pended by a *straight regulating spring*, whose property
" was to render all the vibrations *isochrone*. Is it still
" about a *straight regulating spring* which M. L. R. would
" speak of in 1768?—or of a *spiral spring?* And I
" ask it of himself: Who could divine that these vague
" words, a *property in the spring*, announced *a certain*
" *length in the spiral spring?* But again, even if he should
" have announced his discovery clearly in the *Exposé Suc-*
" *cinct*, as he has lately done it in 1770, in his memorial
" on the *Measure of Time at Sea;* could I ever be sus-
" pected of having *copied* M. L. R. when I had deposited
" my

Account of the controversy between Le Roy and Berthoud respecting timepieces.

" my discovery with the A
" *cinct* was public.

" After these facts, which
" every one, why has M. I
" lows (page 18 of the *Precis*
" *discovery of the isochroni*
" *deed: but if he had atte*
" *would have been a hard t*
" *in my* Memorial; *it woul*
" *priate it to himself in so*
" *senting it.*" " M. L. R
" such delicate raillery woul
" we shall have observed, tha
" here, is that of 1770; wh
" agrees that I had deposite
" of 1768; when we shall h
" trouble to notice it, he su
" what he did not shew till
" will be on his side.

" M. L. R. continues (pa
" *then, of resting the fact*
" *experiment shewed them*
" *scientific air, of which*
" *thing.*"

" I am sorry that M. L.
" will sincerely allow that i
" ever, encouraged by the p
" has given to this part of
" here is what he wrote n
" of this year:" " *This art*
" force of the spiral must b
" *expresses perfectly the tr*
" And, in speaking of the ex
" my elastic balance, he ad
" *tainly of infinite interest*
" *of the isochronism, and*
" *tent that we can give t*
" *principle: this is where*
" *begin to descend in an a*

"confess that I am more flattered that M. Bernouilli has understood me, and has approved of me, than if I had only been barely understood by M. L. R.

Account of the controversy between Le Roy and Berthoud respecting timepieces.

"Although M. L. R. has not understood me, he understands how to reason upon my theory, and censures the course that I have taken." "Mr. B." "says he (page 18 of the *Precis*)," "supposes, from the beginning, that the force of long and weak springs increases in a less ratio than the spaces described in its different tensions, since he concludes from it that the great vibrations are, in this case, slower than the small ones, and *vice versâ* for the short and strong spring: but *he should not suppose, he should show that the things are so in nature, of which it is only experiment can instruct us.*"

"I confess, that my course has always been the opposite of the precept of M. L. R. In all my researches, I have begun by adopting or assuming principles: I have endeavoured to sift these well, and have called experiments to my assistance with a view to confirm these principles. It is true, that by this method we lose the advantage of meeting sometimes with lucky chances, which discover what we were not seeking; but, in return, when we have made a discovery, we know to what principle we must attribute it, and are not in the state of him who lends one to it purely imaginary.

"M. L. R. is not satisfied with attacking me on my property in my theory; he wants even to attack the solidity of it. I have defended this property by facts, which prove that I could not know the researches of M. L. R. I shall now defend the solidity of it by reasons, at the risk of not being understood by the author of the *Precis*.

"The criticism that M. L. R. has made on my theory, obliges me to enter here into some discussion. It is necessary, first, to bring under one point of view all that he has said on the *isochronism by the spiral*, in his *Mémoire sur la Mesure du Temps en Mer*, printed in 1770, at the end of the *Voyage de M. Cassini fils*.

"*I have always discovered*," (says M. L. R.) "*as the most famous philosophers and artists have done, that the*

" *great vibrations were slower than the small ones.*" " (page " 14. of the *Mesure du Temps en Mer*).

" I have, in general, proved the contrary, and shall adduce " proofs of it hereafter."

" I have likewise remarked" (adds he), " that on a double " arc the difference was for the most part $\frac{1}{140}$th. *This effect " comes, I believe, from the mass of the spring when bending " and unbending, or perhaps from the obstacles that it finds " internally when bending and unbending itself.*" (Ibidem).

" What are these internal obstacles? This explanation does " not seem very intelligible. M. L. R. must undoubtedly be " understood; the internal properties of the spring must be " known to him; since, from 1750, he had announced to " us that he would forthwith give" " *A complete Treatise on " the Nature of the Spring, and on its Effects*, &c."

" M. L. R. continues:" "*It is only very lately that I " have discovered, as I shall explain it more particularly,*" " (We find this explanation no where in his *Mémoire*)." " *this fact, so important, which henceforth must serve as the " basis of the theory of watches, and as a guide to work- " men; to wit, that there is in every spring of a sufficient ex- " tent, a certain length, where all the vibrations, great and " small, are isochrone.*"

" First—What is this extent? Secondly—This proposi- " tion is not generally exact; for we find a great number " of springs, which are such by their nature, that whatever " be their *sufficient extent*, they never will be isochrone.

" *I have discovered,*" (adds M. L. R.) " *that this length " being found, if you shorten the spring, the great vibra- " tions will be quicker than the small ones. If, on the con- " trary, you lengthen it, the small ones will be finished in " less time than the great ones.*"

" This second part of the proposition of M. L. R. is ge- " nerally exact; and on this point we are agreed."

" *It is*" (adds he) "*from this important property, hither- " to unknown, that the regularity of my marine watch par- " ticularly depends.*"

" M. L. R. thinks that this property was universally un- " *known*; and it would have been indeed so, if it had been

" only

" only known by what he said of it before 1770. But I " have proved that I did know it; since, as we have " seen, that on the 10th of February, 1768, I had lodged " with the Academy my *Theory of the Isochronism of the* " *Spiral*.

Account of the controversy between Le Roy and Berthoud respecting timepieces.

" Is (it there) all that M. L. R. has told us, in 1770, of " this property of the spiral spring, in his *Mémoire sur la* " *Mesure du Temps en Mer*, which contains the description " of his present watches. I have sufficiently proved by the " dates of our productions that I could not be his copier; " but I can yet prove in another manner that I could not " be so, since we agree not, either in the fundamental prin- " ciple of our theory on the isochronism by the spiral, or in " all its consequences.

" First, M. L. R. says, that in all the experiments which " he has made on the time or duration of the vibrations of a " balance with the spiral spring, he has almost always found " that *the great vibrations are slower than the small ones*.

" All the experiments, on the contrary, which I have re- " lated in my *Traité des Horloges* on spiral springs, and " a still greater number which I have made, and which " are not mentioned in that work, prove that, in general, " the spiral renders *the great vibrations* of the balance " *quicker than the small ones*. See, in the *Treatise on Ma-* " *rine Clocks*, the experiments of No. 137, 206, 212, 215, " 216, 217, 218, 219, 220, 225, 227, 228, 230, 232, the " first of 233, and the No. 234, 928. The experiments of " 207, 221, 226, and the second of 233, are the only " ones which could give the great vibrations slower than the " small ones: and still it is only by a long and difficult " task that spirals can be brought to that point which alone " can assure us, that the spiral is susceptible of being made " *isochrone*, a property which we obtain then by shortening " it. Less fortunate than M. L. R., who tells us (page 34 " of his *Mémoire*) that " *this operation* (of seeking the point " where a spiral is isochrone) *seemed at first tedious, but* " *that practice renders it so easy, that at once he now knows* " *pretty nearly the length of the spring where all the vibra-* " *tions are of equal duration*." " I confess, on the contrary, " that, though aided with an excellent instrument in my

" elas- ("

Account of the controversy between Le Roy and Berthoud respecting timepieces.

" elastic balance, it is only with much trouble that I have " found some spiral springs fit to succeed by their isochronism. And again have I found them quite altered on attempting to *temper them when turned up*. This operation " is, however, indispensable, if we want to give them the " quality of keeping a constant figure, a quality that is preferable by much to that of the isochronism by the spiral, in machines destined to undergo all the changes of " temperature, which never fail to alter the figure of springs " when they have not been tempered after being turned up.

" I must add, that if my experiments do not agree with " those of M. L. R. they agree with those of Mr. Harrison: this celebrated artist always found, as well as I did, " that *the great arcs of vibration were quicker than the small* " *ones*.

" M. L. R. after having set out with a principle contradicted by experiment, makes the isochronism to consist " only in the more or less of length of the spiral spring; " whereas I have proved in my *Traité des Horloges Marines*, " first, that we can arrive at isochronism without rendering " the spring longer, but by making it *broader and thinner*, (No. " 143). Secondly, that we can come at it by a great number of close turns, (No. 154); or by rendering the spring " *stronger or weaker at the centre or outwards*, (No. 157): " I have shewn, that the lamina or wire must be made " like a whip or lash, strongest at the centre, (No. 159). " See also the Nos. 159, 222, 235, &c.; and, in general, " see in the table of matters of the *Treatise of Marine Clocks*, " at the word *spiral*, all the articles where it is treated of.

" The quality of isochronism is precious, without doubt, " in a spiral spring, and I have insisted on it in my *Traité des* " *Horloges Marines*, not, as M. L. R. pretends, because the " justness of my marine clock is founded, like that of his " watches, on the isochronism of the spiral; not because I " believe that, *without this method, we shall ever have only* " *feeble success in marine clocks*. I have neither said nor thought " so; but because this method can render the making, regulating, or timeing of these machines, more expeditious and " more easy. I have always looked on it merely as an useful " accessary, which, perhaps, might render my clocks still

" more

"more perfect. But two indispensable qualities in the spi-
"ral are, first, the *constancy of force*, (abstraction being
"made of the accidental changes produced by the action
"of heat and cold); secondly, the *constancy of figure*; and
"it is on these two qualities, of the first necessity, that I have
"always insisted, and that I still insist on.

M. Le Roy gave the isochronism to the balance by a certain length in the flat spiral spring: and M. Berthoud, to obtain the same, condemns the cylindrical helix as being unfit for this purpose, and uses the flat helix tapered thinnest outward. Mr. Arnold used the cylindrical helix; and Mr. Earnshaw recommends tapering the spring thinnest inward. These diversities of opinion still serve to confirm what Dr. Hooke observed in the numerous experiments which he made with springs; namely, that he could obtain the isochronism by twenty different ways.

As you had requested me, I shall now endeavour, Mr. Nicholson, to furnish you with some accounts of the detached 'scapement, and shall give you, as far as I have been able to discover it, the history of *its invention* and subsequent progress.

Observations of Berthoud respecting the first free escapement.

"I pass" (says M. Berthoud *) "to the detent 'scapement
"having free vibrations (the detached), over which M. L. R.
"pretends to have such an incontestible and exclusive right."
"M. B." (says he) "relates, that the late *M. de Camus* had
"told him that the deceased *M. du Tertre* was the first who
"had this idea. He assures us besides, that in 1754, he him-
"self had contrived one of this sort; and that when in Lon-
"don, in 1766, Mr. Mudge had shewn him one similar, or
"nearly so. We perceive clearly for what purpose he makes
"all these quotations, but that they may only prevent the
"truth being known, of what, in 1748, the Academy had de-
"clared, in speaking of the first 'scapement which had
"appeared with free vibrations, and which I had presented to
"it, that the idea of it seemed new to the Academy, and
"susceptible of many advantages."

* In his "Eclairssemens sur l'Invention et la Construction des
"Horloges Marines."

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Farther particulars and remarks.

"It is very true, that in 1748, M.L.R. presented a 'scapement "to the Academy; it is very true, that the Academy said then, "that the idea of it seemed new. What was this 'scapement? "The Academy does not tell. But can M. L. R. assure us "now, supposing that this was a 'scapement with free vibra-"tions, that the idea of it was *new?* Can he require that we "should believe that his 'scapement of 1748 was *the first* of this "sort *which had then appeared?* And could it be possible that "he had forgot, what he has himself said of it, in his *Etrennes* "*Chronométriques* for the year 1759? I shall now set his own "words before him." "Convinced" (said he) "of the verity of "the sentiment of *Descartes*, I undertook, in 1751, to make "a clock to go eight days with one wheel only in the movement. "What gave me the idea of this construction, was the 'scape-"ment of a watch with a rest or dead-beat, and a *detent*, "which I presented to the Academy in the year 1748, with "whom it carried or received a favourable opinion, as may "be seen in the Memoirs for that year. *My contrivance was* "*not so new as I had imagined: M. M. du Tertre's sons, con-*"*siderable artists in many respects, shewed me, soon after, a* "*model of the watch of their late father's, and which the* "*eldest M. du Tertre must still have.* This model, though "very different from my construction, is, however, the same "as to the end proposed; the motion in both is only re-"stored to every other vibration, &c. And lower down we "read what follows:" "The liberty or freedom procured to "the regulator in the 'scapement of M. du Tertre, by a "*detent* formed like a long lever, which was stopped * dur-"ing two vibrations by the arbor of the balances, and "moved by an anchor, seemed to me at that time very "advantageous, &c."

"The late M. Camus was not then so very wrong, when "I shewed him, in 1754, my 'scapement having *a detent* "*and free vibrations*, in telling me, that long ago the de-"ceased M. du Tertre had proposed and made use of one "like it. We shall find ourselves, M. L. R. and me, in

* The description of Du Tertre's 'scapement, as given here by Le Roy, is unintelligible and obscure. Du Tertre was much engaged in improving 'scapements about the year 1724.

"the

" the same situation, by being left behind in an invention
" which had presented itself long before to several artists;
" but M. L. R. wants to appropriate it exclusively to
" himself; and, on the contrary, I have done homage to
" M. du Tertre, as to him who had the first proposed it;
" although, assuredly, I had no knowledge of any 'scapement
" of this sort when I proposed mine, executed and in a
" finished state, to M. Camus. We readily perceive why
" M. L. R. is so jealous of this invention: he is persuaded
" that *it is by the 'scapement that the most part of the*
" *trials have miscarried which have been made to discover*
" *the longitude by timekeepers*, and that the 'scapement, with
" the detent and free vibrations, is exempt from all faults.
" I am very far from thinking on this subject as he does; but
" this is not the place to enter into a discussion, which would
" lead us too far. I believe, moreover, that we might make
" use of, and with equal success, 'scapements of a very differ-
" ent nature: and this is not an opinion, it is a fact proved by
" experience. The marine watch of Mr. Harrison, that of
" M. L. R., and my clock, have each a 'scapement, which
" differ essentially from one another, both in their principle
" and in their action. Moreover, I shall make no hesitation
" to use the 'scapement with free vibrations, if the experiments,
" which I propose to repeat, ever prove to me that it is prefer-
" able to any other; and in that I will make use of it as my
" own right, and shall not think or believe myself to have co-
" pied, in any manner, neither that of M. L. R. nor that of
" M. du Tertre, for I know not the construction of the 'scape-
" ment of this last; and those of this kind, which I have con-
" trived, may be seen in my *Traité des Horloges Marines*,
" differing from the 'scapement of which M. L. R. has given
" us the description of in his *Mesure du Temps en Mer*. I
" will likewise not dispute against the preference which
" M. L. R. thinks that his 'scapement ought to have over
" mine; it is so natural to love our children even when they
" are only adopted.

Different 'scapements have performed well.

M. Berthoud, in his *Supplément au Traité des Horloges Marines*, says, that he executed five marine clocks having the 'scapement with the spring detent, *(détente-ressort)*, which were begun in 1768, and completed in 1782. Those who

Timepieces of Berthoud.

who wish to see more on the subject of the isochronism pendulum springs, and of detached 'scapements, may c sult his *Traité des Horloges Marines*, published in 1773, the *Supplément au Traité des Horloges Marines*, published 1787. There is, in Thiout, an idea of a sort of detac 'scapement; this work of his was published in 1742.

In M. Berthoud's *Supplément*, he says, "Such are the " servations which I made the 17th of March, 1763, in co " posing my first astronomical watch, which was finis " about the beginning of 1764, the designs and the descript " of which were lodged with the Academy in August 176 " See *Traité des Horloges Marines*, Append. page 533." "I " this watch, when in London, in 1766, to Mr. Pinchbeck, " purchased it for the king of England: it was the first poc " watch that had been made in Europe having a compensat " for heat and cold."

Le Roy's balance of compensation by solid parts,

M. Le Roy, in his *Mesure du Temps en Mer*, gives the draw of a compensation balance, which is the first,* as it were, th have seen or heard of, the idea of which he confesses to h taken from Mr. Harrison's compensation bar. But the invent of the compensation balance *itself* is due to Mr. Harrison, wh

—is presumed to have been previously made by Harrison.

may be presumed from what follows: "You will now per " me to speak a word or two, as to the compensation for h " and cold in the balance. It is the original method by wh " Mr. Harrison attempted to correct the error, which, as " was pretty tenacious of his own opinion, he carried into e " cution contrary to the advice of Mr. Graham, but fou " by experience, that Mr. Graham was right, and was for " to throw it all away,† and to contrive his method of app " ing it to the balance springs." See Mr. Mudge's Letters Count Bruhl.

Great merits of Arnold.

And farther, in honour of our country, it must be ackno

* I suppose this expression to imply the compensation bala of two metals, in contradistinction to the fluid thermometer l lance, which has no relation to Harrison.—N.

† The late Mr. Arnold, by making the compensation in the l lance, and by its being now carried into general practice, and, it were, confirmed, proves that Mr. Harrison's original idea w good.

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ledged that the late Mr. Arnold, as well for his own inventions as from his ability for improving whatever came before him, deserved all the encouragement he met with from his private business, and from the public rewards which he may from time to time have obtained, if it was no more but for his exertions in persevering and shewing a track which others might follow; and this he did with an enterprizing and ingenious spirit, of which few men were capable. All circumstances considered, the business of making chronometers stands more indebted to him than any other man since Dr. Hooke; with whose merits Mr. Arnold was so well acquainted, and whom he thought so much of, as to set him on a par, nay, even above the most celebrated Sir Isaac Newton. When Mr. Arnold began his career in life, he was the first who brought watch jewelling, and the application of stone to the places of action, into more general use than ever had been done before his time; and, although *these* may not have any thing of a mechanical principle in them, yet they certainly render any principle connected with the holes, &c. as, for example, the pitchings and 'scapements are made more permanent than they would otherwise be, by which our watches have acquired a stability and character (from jewelled holes, &c.) that in all probability they would not have had without them. I speak from experience of the utility of these things; the pallets of my astronomical clock being of stone, and it has been going with me for sixteen years, without the smallest application of oil to them in any manner whatever.

Arnold extended the practice of jewelling.

I am, Sir,

With much respect,

Yours,

THOMAS REID.

Edinburgh, July 12, 1806.

P. S.—In the work published by M. Thiout, in 1741, there is a large collection of the various 'scapements then known; and, among others, there is one which seems to have been invented by himself, and is a sort of free or detached one, the same in its principle as the one invented by M. Le Roy in

A detached escapement by Thiout, in 1741, prior to Le Roy's, and not essentially different.

1748. It may be said to operate thus: After one of the balance wheel teeth has given impulse to the pallet, and has just quitted it, there is a *detent*, which is made to come in and catch or stop the wheel, by interposing itself in the way of one of the wheel teeth. Here the wheel rests during this vibration; and, on the balance returning, there is on its axis an arm or *pallet*, which gets into a sort of fork fixed on the arbor of the detent, and which serves, by means of this fork, both to lock and unlock the wheel. The unlocking is never completely done until such time as the pallet (which receives the impulse) has got near or against the point of the next impelling tooth, which then gets a recoil from the pallet, by the momentum of the balance on its return, and on the wheel being free, and again pressing the pallet forward, it again becomes locked, and so on.

Le Roy and Berthoud must both have known Thiout's escapement.

How has it happened, that both M. Berthoud and M. Le Roy have overlooked this 'scapement of M. Thiout's? It could hardly be unknown to them in the course of their dispute concerning the priority of invention of the free or detached 'scapement.

Description.

In the figure, Plate X. Fig. 1., A B is the balance wheel; G the pallet in which the teeth act; *d* the arbor of the detent; *e* the locking and unlocking pallet, which is done by means of the fork *f*; *h* the hook which locks the wheel teeth; *i* is the verge or axis of the balance. M. Le Roy made the balance wheel a sort of contrate one, having the teeth standing upright. This is a small improvement on M. Thiout's, which seems to be also the origin of the *Echappement à Virgule*.*

* Some of the information contained in the above letter has been, by a singular coincidence, anticipated in an article in the last number of this Journal. It may, therefore, be necessary to mention, that what regards Dr. Hooke in the present letter, was composed fourteen years ago, and that the whole was in possession of Mr. Nicholson previous to the publication of that essay.—*Note of the author.*

III.

A Method of rendering all the Vibrations of the Balance of a Timepiece equal. In a Letter from Mr. WILLIAM HARDY.

To Mr. NICHOLSON.

SIR,

New method o connecting th vibrations of a balance. Th stud is a sprin which allows play endwise.

I BEG leave to communicate to your readers a new and easy method of correcting the long and short arches of vibration in the balance of a timekeeper; that is to say, that they shall all be performed in equal times, so short as the angle of escapement will admit.

The spring, *a b*, Fig. 4, 5, 6, Plate XI., screwed to the under part of the cock, *h*, lies over the upper part of the pendulum spring, proceeding in a right line to the axis, *e*, of the balance, *g*, having a bend to clear the verge, and so passes on to the other side, where the end of the pendulum spring is fastened to it. This straight spring is reduced to such a consistency, as to allow it to be brought into action a little before the pendulum spring. The other piece, *c*, which projects down from the under side of the cock, lies in a line with this spring, and is screwed to the cock on the opposite side of the centre to that where the spring is fastened. It is of an oblong form, and has a slit cut down, with an adjusting screw, *d d*, on each side, whose points face each other in the same right line to receive the small projecting piece, *b*, which is at the end of the straight spring, so as to move freely between them. The points of the screws should be at equal distances from the spring. When the balance is at rest, the space between the two screws must be considerably less than the angle of 'scapement, but the proper quantity must be determined by trial. As this straight spring is weaker than the pendulum spring, it will be first brought into action; therefore, if the balance be made to move only so far as to cause the spring to act between the two adjusting screws, the motion of the balance will be prolonged; but on being stopped by the adjusting screws, the action of the straight spring will cease, and that of the pendulum spring will commence, and consequently progressively accelerate the vibrations of the balance. It will, therefore,

always oppose the accelerating effects of the wheel in the short vibrations, and so cause the whole of them to be performed nearly in the same time.

I am, Sir,

Your most obedient humble servant,

WILLIAM HARDY.

IV.

Remarks on Achromatic Eye-pieces. By DAVID BREWSTER, *A. M.*

To Mr. NICHOLSON.

SIR,

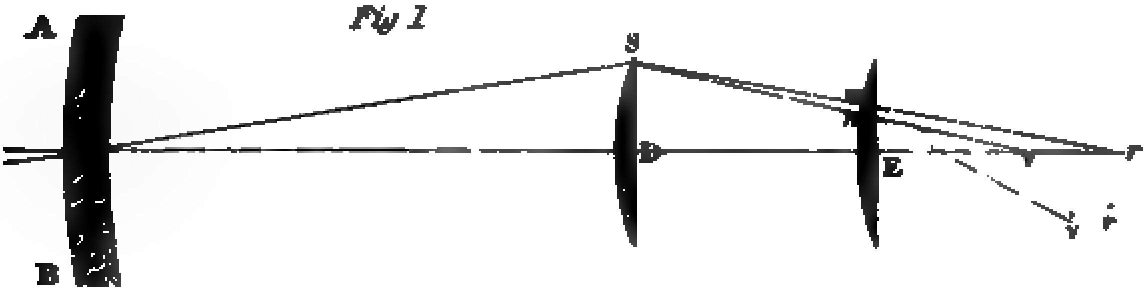
Introductory remarks.

I OBSERVE, in the last number of your Journal, a query respecting a rule for achromatic eye-pieces, contained in my appendix to *Ferguson's Lectures.* Your correspondent seems to question the accuracy of that rule, and to imagine that no combination of lenses whatever can form an eye-piece capable of removing the chromatic aberration. Were this principle to be admitted, the rules which I have given for achromatic eye-pieces, composed of three or four lenses, would likewise be inaccurate; and the ingenious eye-pieces by which Dolland and Ramsden have rendered their telescopes superior to those of every other artist, would be liable to the same imputation. I could easily demonstrate, were it necessary, that an eye-piece consisting of two lenses, whose focal lengths, reckoning from that next the object, are as the numbers 3 and 1, and whose distance is equal to the difference of their focal lengths, will almost wholly remove the aberration of colour. But I imagine it will be a sufficient answer to the query of your correspondent, and a sufficient vindication of the rule to which he alludes, if I can explain to him the reason why the error, arising from the different refrangibility of the coloured rays, may be corrected by a judicious combination of lenses of the same refractive and dispersive power.

Explanation of an achromatic eye piece.

Let A B, Fig. 1. Pl. XI., be an achromatic object-glass, and D, E two lenses of the same kind of glass. Let C D E be the axis

Fig 1



Robt. Hooke

Fig 3

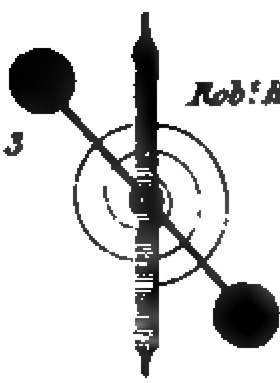
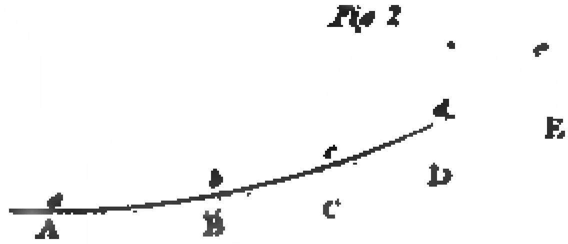


Fig 2



Mr. Hardy's Improvement on Pendulum Springs.

Fig 4

Fig 5

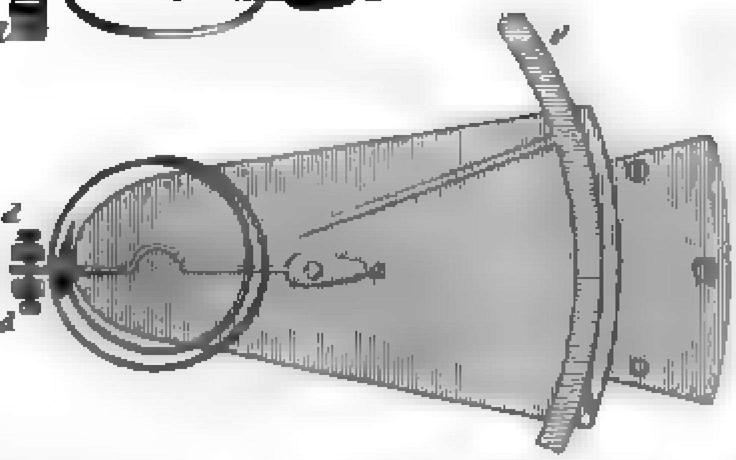
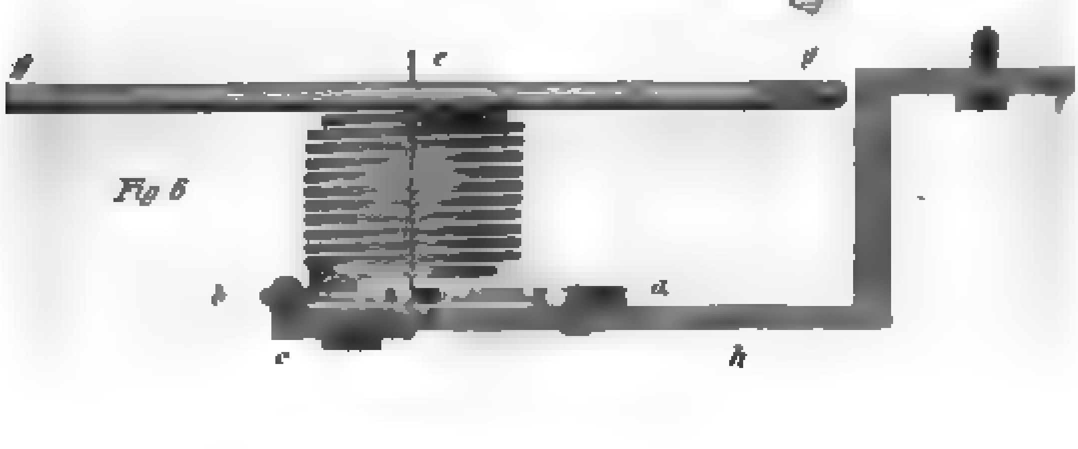


Fig 6



axis of the telescope, and, R S, a ray passing through the centre of the object-glass. This ray will fall, in an uncompounded state, upon the eye-glass, D, even if the object-glass were not achromatic; for, as it passes through its centre, it undergoes two equal and opposite changes, and is therefore not separated into the prismatic colours. After refraction through the lens, D, the rays, R S, will be decomposed, and the red portion of it will meet the axis in *r*, and the violet ray will meet the axis in *v*. But when the second eye-glass, F, is interposed, the red ray will meet its anterior surface in *m*, and the violet ray at a point, *n*, nearer the axis. Now, as the refracting angle of the lens is greater at *m* than at *n*, the red ray, *S m*, will be more refracted than the violet ray, *S n*; and this increase of refraction will compensate for its inferior degree of refrangibility. From this cause the refraction of the second lens, E, will render the resulting rays, $m\,r'$, $n\,v'$, parallel, and thus destroy the chromatic aberration, which is always proportional to the angle formed by the resulting rays, $m\,r'$, $n\,v'$. In order that distinct vision may be obtained with this eye-piece, the rays must fall converging on the first eye-glass, D. This can be effected only by placing the lens, D, between the object-glass and its principal focus, and by this means the telescope is rendered shorter than if only one convex lens had been employed. The equivalent lens, or the lens which would produce the same magnifying power as this eye-piece, is equal to *half* the focal length of the glass, D.

I am, Sir,

Your obedient humble servant,

Mount Annan, July 21, 1806. DAVID BREWSTER.

V.

Chemical Experiments on Guaiacum. By Mr. WILLIAM BRANDE. *From the Philosophical Transactions*, 1806.

Guaiacum possesses very singular properties.

AMONG the numerous substances which are comprehended under the name of resins, there is perhaps no one which possesses so many curious properties, as that now under consideration; and it is remarkable that no more attention has been

been paid to the subject, since many of the alterations which it undergoes when treated with different solvents, have been mentioned by various authors.

External and obvious characters of guaiacum.

SECT I.—Guaiacum has a green hue externally; is in some degree transparent; and breaks with a vitreous fracture.

When pulverised it is of a gray colour, but gradually becomes greenish on exposure to air.

It melts when heated, and diffuses at the same time a pungent aromatic odour.

It has when in powder a pleasant balsamic smell, but scarcely any taste, although when swallowed it excites a very powerful burning sensation in the throat.

Its specific gravity is 1.2289.

Aqueous solution imperfect.

SECT. II. 1.—When pulverised guaiacum is digested in a moderate heat with distilled water, an opaque solution is formed, which becomes clear on passing the whole through a filter.

The filtrated liquor is of a greenish-brown colour; it has a peculiar smell, and a sweetish taste.

It leaves on evaporation a brown substance, which is soluble in alcohol, nearly soluble in boiling water, and very little acted upon by sulphuric ether.

This solution was examined by the following reagents.

Filtered solution examined by reagents.

Muriate of alumina occasioned a brown insoluble precipitate after some hours had elapsed.

Muriate of tin formed a brown flaky precipitate under the same circumstances.

Nitrate of silver gave a copious brown precipitate.

Suspecting the presence of lime in the solution, I added a few drops of oxalate of ammonia, when the liquid immediately became turbid, and deposited brown flakes, which, after having been treated with boiling alcohol, yielded traces of oxalate of lime.

These effects, therefore, indicate the presence of a substance in guaiacum, which possesses the properties of extract;* th

* By the terms extract, I mean that substance, which by chemists is called the extractive principle of vegetables. Vide Thompson's Syst. of Chemistry, 2d edit. vol. iv. p. 276.

acti

tion of the reagent is however somewhat modified, by a small quantity of lime which is also in solution.

One hundred grains of guaiacum yielded about nine grains of this impure extractive matter.

2. Alcohol dissolves guaiacum with facility, leaving some extraneous matter, which generally amounts to about 5 *per cent*.

Alcholic solution of guaiacum considerably perfect.

This solution is of a deep brown colour; the addition of water separates the resin, forming a milky fluid which passes the filter.

Acids produce the following changes:

A. Muriatic acid throws down an ash-coloured precipitate, which is not re-dissolved by heating the mixture. In this case the resin appears but little altered.

Changes produced by acids.

B. Liquid oxy-muriatic acid when poured into this solution, forms a precipitate of a very beautiful pale-blue colour, which may be preserved unaltered.

C. Sulphuric acid, when not added in too large a quantity, separates the resin of a pale green colour.

D. Acetic acid does not form any precipitate. This acid is indeed capable of dissolving most of the resins.

E. Nitric acid diluted with one-fourth of its weight of water, causes no precipitate till after the period of some hours. The liquid at first assumes a green colour, and if water be added at this period, a green precipitate may be obtained; the green colour soon changes to blue, (when by the same means a blue precipitate may be obtained); it then becomes brown, and a brown precipitate spontaneously makes its appearance, the properties of which will be afterwards mentioned.

The changes of colour produced by nitric, and oxy-muriatic acids, in the alcoholic solution; are very remarkable, and I believe peculiar to guaiacum: there is moreover much reason to suppose that the above alterations in colour are occasioned by oxigen.* It likewise appears from that which has been stated,

—very remarkable, and peculiar to Guaiacum.

* The following experiments appear to verify this supposition:

Fifty grains of freshly pulverised guaiacum were introduced into a glass jar containing 60 cubic inches of oxy-muriatic acid gas. The resin speedily assumed a brown colour, having passed through several shades of green and blue. Liquid ammonia was poured on this brown substance, while yet immersed in the acid; the whole became

stated, that the blue and green oxides (if they may be so called by way of distinction) are soluble in the mixture of nitric acid and alcohol, while the brown precipitate is insoluble.

Alkalis do not precipitate the alcoholic solution.

F. Alkalis do not form any precipitate when added to the solution of guaiacum in alcohol.

Direct action of ether and of the acids upon Guaiacum.

3. Guaiacum is less soluble in sulphuric ether than in alcohol; the properties of this solution nearly coincide with those just mentioned.

4. Muriatic acid dissolves a small portion of guaiacum, the solution assuming a deep brown colour; but if heat be applied, the resin melts into a blackish mass, preventing any farther action from taking place.

5. Sulphuric acid forms with guaiacum a deep red liquid, which, when fresh prepared, deposits a lilac coloured precipitate on the addition of water; a precipitate is also formed by the alkalis. If heat be employed in forming this solution, the resin is speedily decomposed; and if the whole of the acid be evaporated, there remains a black coaly substance, together with some sulphate of lime.

Action of nitric acid upon guaiacum.

6. Nitric acid appears to exert a more powerful action on guaiacum than on any of the resinous bodies.

100 grains of pure guaiacum previously reduced to powder, were cautiously added to two ounces of nitric acid, of the specific gravity of 1.39. The resin at first assumed a dark green colour, a violent effervescence was produced, attended with the emission of much nitrous gas, and the whole was dissolved

became green; it therefore seemed thus to be deprived of part of the oxygen which it apparently had acquired by the preceding experiment. An equal portion of the same guaiacum was exposed under similar circumstances to the action of oxy-muriatic acid, excepting that the glass in which the experiment was made, was covered with a black varnish, and placed in a dark apartment. On examining the result of this experiment, the resin was found to have undergone precisely the same changes as when exposed to light. Ammonia had also the same effect.

Guaiacum was also exposed over mercury to oxygen gas; the resin assumed after some days the green colour which a longer exposure to the atmosphere produces: this change was likewise found by a second experiment to be effected without the presence of light.

without the assistance of heat, which is not the case with the resins in general; for when these bodies are thus treated with nitric acid, they are commonly converted into an orange-coloured porous mass.

The solution thus formed, yielded while recent, a brown precipitate with the alkalis, which was redissolved on the application of heat, forming a deep brown liquid.

Muriatic acid also separated the guaiacum from this solution, not however without having undergone some change.

Sulphuric acid caused no precipitate.

After this solution of guaiacum in nitric acid had remained undisturbed for some hours, a considerable proportion of crystallized oxalic acid was deposited.

Dilute acid.

When guaiacum was treated with dilute nitric acid, the results were somewhat different. A slight effervescence took place, and part of the resin was dissolved, the remainder being converted into a brown substance, resembling the precipitate obtained from the alcoholic solution as above mentioned. (2. E.)

This brown substance appears to be guaiacum, the properties of which are materially altered, by its combination with oxigen; and I am led to think that the changes of colour produced by nitric and oxy-muriatic acids, are the consequence of the different proportions of oxigen with which the guaiacum has been united; for we know that the colours of metallic, and many other bodies, are greatly influenced by the same cause.

The brown substance was separated by filtration; the filtrated liquor yielded yellow flocculent precipitates with the alkalis, and on examination was found to hold nitrate of lime in solution.

The undissolved portion was of a deep chocolate-brown colour. A similar substance may also be obtained, by evaporating the recent nitric solution to dryness, taking care not to apply too much heat towards the end of the process.

The substance obtained by either of these means, possesses the properties of a resin in greater perfection than guaiacum; it is equally soluble in alcohol and sulphuric ether, insoluble in water, &c.; but when burned it emits a peculiar smell, more resembling animal than vegetable bodies. If, however, fresh portions of nitric acid be added three or four times successively;

sively; or if a large quantity be employed to form the solution, the product obtained by evaporation is then of a very different nature; for it has lost all the characteristic properties of a resin, having become equally soluble in water and alcohol; the solution of it in this state having an astringent bitter taste.*

Alkalis and their carbonates dissolve guaiacum.

7. Guaiacum is copiously soluble in the pure and carbonated alkalis, forming greenish brown liquids.

Two ounces of a saturated solution of caustic potash took up rather more than 65 grains of the resin; the same quantity of liquid ammonia dissolved only 25 grains.

—precipitable by acids.

Nitric acid formed in these solutions a deep brown precipitate, the shades of which varied according to the quantity of acid which had been employed.

This precipitate was found on examination to possess the properties of that formed by nitric acid in the solution of guaiacum (2. E.) in alcohol.

Dilute sulphuric acid, when poured into any of the above alkaline solutions, formed a flesh-coloured curdy precipitate. Muriatic acid produced the same effect.

The two last-mentioned precipitates differ from guaiacum in being less acted upon by sulphuric ether and more soluble in boiling water: their properties therefore approach nearer to extract. Moreover, when these precipitates were dissolved in ammonia, and were again separated by muriatic acid, the above-mentioned properties became more evident.

Products of distillation upon guaiacum.

SECT. III.—100 grains of very pure guaiacum in powder were put into a glass retort, to which the usual apparatus was adapted. The distillation was gradually performed on an open fire, until the bottom of the retort became red hot.

The following products were obtained:

	grains.
Acidulated water	5.5
Thick brown oil, becoming turbid on cooling	24.5
Thin empyreumatic oil?	30.0
Coal remaining in the retort	30.5
Mixed gases, consisting chiefly of carbonic acid and carbonate hydrogen	9.0
	99.5

* Vide Mr. Hatchett's two papers on an artificial substance which possesses the principle characteristic properties of tannin. Phil. Trans. 1805, p. 211, and 285.

The

The coal, amounting to 30.5 grains, yielded on incineration 3 grains of lime. To discover whether any fixed alkali was present, 200 grains of the purest guaiacum (that in drops) were reduced to ashes: these were dissolved in muriatic acid, and precipitated by ammonia: the whole was then filtrated, and the clear liquor evaporated to dryness, but not any trace of a neutral salt with a basis of fixed alkali was preceptible.

Enumeration of the difference between guaiacum and resins.

SECT. IV.—From the action of different solvents on guaiacum, it appears, that although this substance possesses many properties in common with resinous bodies, it nevertheless differs from them in the following particulars:

1. By affording a portion of vegetable extract.

2. By the curious alterations which it undergoes when subjected to the action of bodies, which readily communicate oxigen, such as nitric and oxy-muriatic acids: and the rapidity with which it dissolves in the former.

3. By being converted into a more perfect resin: in which respect guaiacum bears some resemblance to the green resin which constitutes the colouring matter of the leaves of trees, &c.*

4. By yielding oxalic acid.

5. By the quantity of charcoal and lime which are obtained from it when subjected to destructive distillation.

Guaiacum appears to be, or to contain, a peculiar substance.

SECT. V.—From the whole therefore of the above-mentioned properties, it evidently appears that guaiacum is a substance very different from those which are denominated resins, and that it is also different from all those which are enumerated amongst the balsams, gum resins, gums, and extracts: most probably it is a substance distinct in its nature from any of the above, in consequence of certain peculiarities in the proportions and chemical combination of its constituent elementary principles; but as this opinion may be thought not sufficiently supported by the facts which have been adduced, we may for the present be allowed to regard guaiacum as composed of a resin modified by the vegetable extractive principle,

* This substance was found by Proust to be insoluble in water, and soluble in alcohol. When treated with oxy-muriatic acid, it assumed the colour of a withered leaf, acquiring the resinous properties in greater perfection. Vide Thompson's Syst. of Chemistry, 2d edit. vol. iv. p. 318.

 and

and as such, perhaps the definition of it by the term of an *extracto-resin* may be adopted without impropriety.

Oxigenation of that part of matter which alcohol takes up.

P. S.—I have observed that the action of oxigen on some of the other resinous bodies is very remarkable. It is well known that by digesting mastich in alcohol, a partial solution only is formed, and there remains an elastic substance, which is generally said to possess the properties of pure caoutchouc; it appears however to differ from this substance in becoming hard when dried by exposure to air. Moreover, I have remarked that the part of mastich which remains dissolved by alcohol, may be again precipitated by water, and, on examination, I found the precipitate to possess the properties of a pure resin: but when a stream of oxy-muriatic acid gas was made to pass through the solution, a tough elastic substance was thrown down, which became brittle when dried, and was soluble in boiling alcohol, but separated again as the solution cooled: its properties, therefore, somewhat approached to those of the original insoluble part.

VI.

On Silver Coins. By THOMAS THOMSON, *M.D. F.R.S. E.* *Communicated by the Author.*

Silver as a medium of exchange.

SILVER has been employed by most nations as a medium of exchange. The ancients appear to have coined the metal pure, and the same practice is still followed, I believe, in some of the Eastern nations. But in Europe it is always alloyed with copper; pure silver being considered as too soft for coin.

Analysis of silver coins.

The analysis of silver coins is not attended with much difficulty. The metals which they contain are silver and copper, and almost always a little gold. The method which I use to separate and estimate the relative weights of these metals was the following:

The method.

1. The silver coin was first well cleaned by means of soap or an alkaline ley. It was afterwards weighed, and its specific

fic gravity ascertained. The specific gravity of silver varies, as is well known, according to circumstances. I melted two ounces of pure silver, and let it cool in the bottom of a black-lead crucible previously heated. Its specific gravity was 10.3946. The same mass was fused a second time, and cast into a thin plate. Its specific gravity was now reduced to 10.1790. When this plate had been passed between rollers, its specific gravity was found to be 10.4812. By hammering the round button, its specific gravity became 10.4177. *Specific gravity of pure silver.*

The coin after being cleaned was put into a matrass with a sufficient quantity of pure nitric acid previously diluted with about twice its bulk of water. The solution as soon as the acid ceased to act was poured off, and the black powder which usually remained, was repeatedly digested on a sand bath with small quantities of nitric acid. It was then washed with distilled water, and dissolved in nitromuriatic acid. The solution was mixed with liquid sulphate of iron, and the blackish powder which fell was washed, and formed into a solid mass, sometimes by amalgamating it with mercury, and driving off the volatile metal by heating the amalgam in a small porcelain crucible; sometimes by forming it into a ball with calcined borax and fusing it into a button before the blow-pipe. The metals thus obtained was the gold. It never exceeded $\frac{1}{200}$th part of the coin, and seldom amounted to $\frac{1}{1000}$th part. In some coins, no gold whatever could be detected. In some of the following analysis, the black powder which remained when the coin was first dissolved in nitric acid, was melted into a button, and weighed. This button was then treated with nitromuriatic acid: if it dissolved completely, it was considered as pure gold; but if it left any residue of muriate of silver, this muriate was carefully dried and weighed, and the proportion of silver thus indicated was subtracted from the weight of the button; the remainder was considered as the weight of the gold contained in the coin.

Solution of the coin in nitric acid diluted.

The undissolved gold dissolved in nitromuriatic acid and precipitated, &c.

3. The nitric acid solution was mixed with a solution of common salt, more than sufficient to separate the whole of the silver. The muriate of silver was allowed to settle at the bottom of the vessel, and the liquid carefully decanted off. Distilled water was poured upon the precipitate, the mixture was well stirred with a glass rod, and left at rest till the muriate was deposited; then the water was decanted off, and a new

The nitric solution precipitated by com. salt and the muriate of silver weighed.

new portion substituted. This
came off perfectly pure. The
out of the vessel into a glass
water being drained, it was dri
bath, heated nearly to the temp
then carefully weighed, first wi
after that substance had been
the exact weight of muriate o
under 400°.

Reduction of the muriate.

In the earlier analyses, the si
riate by fusing it with common
found liable to some uncertai
raised, a portion of the muriat
ized, even though covered with
ceed in preventing this, it is bu
silver is united in a single mass.
ly sink into the crucible, and
For these reasons, I found it m
cise, to estimate the weight
muriate.

Standard experiment to reduce the silver contained in the muriate

A hundred grains of pure sil
were dissolved in nitric acid,
whole was brought to a state nc
The weight was 157.18 grains
nued, fumes of nitrous acid
silver was reduced, appearing
flakes. A hundred grains of
nitric acid, evaporated to dry
precipitated by muriate of sod
washed, was placed for two hou
to 400°. It was allowed to co
again placed on the sand bat
weight was not altered. It
days, exposed to the open air,
in its weight took place: the w
put the glass capsule, containi
surrounded it with sand, and
it now weighed only 128.67
follows, that muriate of silver,
contains 0.7554 of silver; a
repetition of the experiment

sult. Hence, to find the quantity of silver in muriate of silver dried at a heat of nearly 400°, we have only to multiply its weight by 0.756. This was the method which I followed. It corresponds very nearly with the result of former analyses as made by others.

4. The solution thus freed from the silver, and containing a considerable excess of muriate of soda, was mixed with all the water employed to wash the muriate of silver, and evaporated to dryness in a porcelain capsule. The dry mass was dissolved in water. Sometimes a little muriate of silver separated during the solution. It was always carefully washed, and added to the precipitate of silver previously obtained.

Residue of the solution evaporated and redissolved

A polished plate of iron was then put into the liquid, which was diluted with water, if necessary, till it just covered the upper end of the plate; it was then laid aside till the whole of the copper was thrown down. Two days were usually required for this separation; sometimes longer, sometimes a shorter time sufficed. Care was taken not to disturb the liquid during the process; for when the copper falls down, the separation is always more tedious. When the process was finished, the plate of iron was withdrawn, and the copper washed from it in distilled water. A portion of the copper often fell down when the plate was withdrawn. As soon as it had subsided, the liquor was decanted off, and water, acidulated with muriatic acid, poured upon the copper. This also was poured off after a few minutes, and pure water substituted in its place. The portion of copper washed off the iron plate was edulcorated in the same manner with water acidulated with muriatic acid. The whole was then collected on a filter, and carefully washed. It was allowed to dry in the open air, and afterwards placed for some hours on a steam bath. It was then weighed, and considered as the proportion of copper contained in the coin.

Precipitation of the copper by iron.

As copper in the metallic state does not combine with water, the powder thus obtained is easily dried. Indeed, if it be thoroughly dried in the open air, it loses no sensible weight afterwards, though heated to 300°. It cannot be heated to redness, because even though this be done in a covered crucible, it very rapidly combines with oxygen, and is converted into a black powder. If it be heated to redness in an open crucible, 100 grains generally increase in weight to 120 grains.

Desiccation of the copper.

Almost

Clean polished iron precipitates almost all the copper, &c.

Almost the whole of the c
its solutions by means of a
be well polished and pure.
copper gets into the interstic
dissolved 50 grains of pure g
in sulphuric acid, evaporated
and put into the solution a
copper precipitated weighed
which adhered to one side o
was only 1 per cent. 100 gr
riatic acid, were recovered wi
employed a rough plate of iro
6 per cent.; for 68 grains of co
gave only 65.43 grains.

When copper is held in solu
down partly in the metallic
muriatic acid be poured upon
solved almost immediately, an
riate of copper. But if comm
acid solution, and the whole b
sipate any excess of acid, and
if necessary, with muriatic aci
copper in the metallic state.

I did not succeed so well in
per by means of a cylinder o
came porous, and it was diffic
it. Besides, part of the copp
of an alloy, for it effervesced w

Curious fact.

A curious phenomenon repeate
atic acid was poured upon cop
by zinc. The effervescence wa
separated was *nitrous gas*, as b
generated.

The specific gravity of the fi
from acid solutions by iron, I f
rature of 62°; but, by simply f
cific gravity became 8.535.

Coins which were analysed.

SECT. II.—The following Ta
coins which I have analysed, pl
together with the products obta

1. An English half-crown. It was a coin of Charles II. dated 1671, and was one of the beautiful pieces coined by Simeon. It weighed 220.5 grains, and had of course sustained a loss of 11 grains.

English half-crown: Silver 12, copper 1.

Muriate of silver, 267.51 gr. =	201.23 silver
Copper	17.00
Loss, including a trace of gold	2.27
	220.5

2. A French half-crown. It was a coin of Louis XV. dated 1761. It weighed 211.5 grains.

French half-crown: Silver 10, copper 1.

Silver obtained by reducing the muriate	190.5 grains
Copper	19.2
Gold	0.3
Loss	1.5
	211.5

3. A rupee. It was brought from India by Mr. Philip Dundas, and given me by his nephew, Mr. Colt. It weighed 178 grains. By an accident, the glass containing it was overturned and broken soon after the solution in nitric acid began. When washed and dried, the rupee still weighed 152.5 grains. It was this portion only that was analysed.

Rupee: Silver 32½, copper 1.

Silver reduced from the muriate	146.5 grains
Copper	4.5
Loss, including some gold	1.5
	152.5

4. A Spanish pisterine. It was a coin of Louis I. and dated 1724. It was brought from Spain by Mr. Farquharson, of Haughton, from whom I obtained it. It weighed 85.5 grains.

Spanish pisterine: Silver 5½, copper 1.

Muriate of silver 94 gr. =	71 silver
Copper	13
Loss, with a little gold	1.5
	85.5

Portugal half-crusada: Silver 8½, copper 1.

5. A half-crusada nova of Po
called a twelve vintem piece.
Peter III. dated 1782. It wei

Silver reduced from th
Copper··············
Loss················

Sardinian coin: Silver 9¼, copper 1.

6. A Sardinian coin of Vi
It weighed 135.5 grains.

Silver············
Copper············

Coin of Berne: Silver 3¼, copper 1.

7. A coin of the canton of I
B surmounted with a crown.
bit. *Reverse,* the arms of Ber
publica Bernensis.—*Cr.* 20. I

Muriate of silver 7
Copper, by estima

For this and the four precedi
lowing, I am indebted to Franci
ton, who brought them from th

Ancient coin, Greek: Silver nearly pure.

8. A coin of Crotona. This
silver coins. It is supposed t
years before the commencemen
rude, thick, not quite round; h
legs, somewhat in the shape of
the appearance of letters, bu
phered. The weight was 113.

Muriate of silver, 144.
Copper ···········
Gold ·············
Loss ·············

9. A Dutch guilder, dated 1791. *Obverse*, the arms of the Republic, with the words, *Mo. Arg. Ord. Fœd. Belg. Holl.* *Reverse*, a female figure leaning on a pillow, and holding a spear. Inscription: *Hanc tuemur, hac nitimur.* It weighed 163.93 grains. It was brought from Holland by William Sligo, Esq., from whom I got it for analysis.

Dutch guilder: Silver 15, copper 1.

Muriate of silver, 198.90 gr. =	150.37 silver
Copper	10.72
Gold	.06
Loss	2.78
	163.93

10. A Russian piece of 15 copecs, dated 1789. *Obverse*, the head of Catharine II. with the usual title. *Reverse*, the arms of Russia, with the figures 15. It weighed 52.97 grains. It was brought from Russia by Mr. Hatchett, from whom I got it for the purpose of analysis.

Russian coin: Silver 4, copper 1.

Muriate of silver, 52.97 gr. =	40.04 silver
Copper	11.39
Loss	1.54
	52.97

In this coin there was not the smallest trace of gold to be detected.

11. A Scotch coin of Charles I. *Obverse*, the head of the king, with the number XL. (40 shillings Scotch). Inscription: *Car. D. G. Scot. Ang. Fr. et Hib. R.* *Reverse*, a thistle with a crown, and the motto, *Salus Reipublicæ Suprema Lex*. It weighed 27.05 grains. Its specific gravity was 10.000.

Scotch 40 shilling: Old silver 12, copper 1.

Muriate of silver, 33.02 gr. =	24.96 silver
Copper	1.90
Loss	0.19
	27.05

This coin was found in digging the foundation of one of the houses in the new town of Edinburgh by a common mason, who brought it to me.

Hambro. coin: Silver 11, copper 10.

12. A coin of Hamburg, dated 1780. *Obverse*, the [...] of the coin; 12 *einen Thal.* *Reverse*, a horse. This coi[n] likewise given me by Mr. Hatchett. It weighed 50.44, [...] specific gravity was only 9.0154.

Muriate of silver, 33.53 gr. =	25.35 silver.
Copper	22.71
Loss	2.36
	50.44

The great quantity of copper in this coin led me at fi[rst to] suspect that it might have been accidentally debased. [This] made me anxious to examine another of the same kind. [Pro]fessor Jameson furnished me with one which he had b[rought] from Germany. The result was as follows:

The same coin, rather worse.

13. A similar coin, rather smaller in diameter, but th[is] dated 1794. It weighed 46.7 grains.

Muriate of silver, 31.52 gr. =	23.83 silver
Copper	22.60
Loss	0.27
	46.7

Having been informed by Mr. Milligan, watch-case m[aker] in Edinburgh, that the Spanish coins differed from each [other] in purity, I examined the two following. The first wa[s fur]nished by that gentleman as a specimen of the purest S[panish] silver coin; the second was a common Spanish dollar.

Spanish pisterine: Silver 18, copper 1.

14. A Spanish pisterino of Philip V. dated 1740. [It] weighed 99.07 grains.

Muriate of silver, 120.30 gr. =	90.95 silver
Copper	5.53
Loss, including some gold	2.59
	99.07

15. A Spanish dollar, dated 1801. It weighed 415.16 gr. Its specific gravity was 10.291.

Spanish dollar, very new: Silver 9, copper 1.

Muriate of silver, 490.33 gr.=370.69 silver		
Copper	42.29	
Gold	0.29	
Loss	1.89	
	415.16	

16. A Danish 60 schilling piece, dated 1789. *Obverse*, the head of the king of Denmark, with the inscription, *Christianus VII. D. G. Dan. Norv. V. G. Rex.* *Reverse*, the arms of Denmark, with the words, 60 *Schelling. Schlesw. Holst. Courant.* It weighed 444.55 grains. Its specific gravity was 10.2667. I received it from Professor Jameson, who brought it from Germany.

Danish coin: Silver 7½, copper 1.

Muriate of silver, 516.98 gr.=390.63 silver	
Copper	53.68
Gold	0.09
Loss	.15
	444.55

17. A Roman denarius at the time of the republic. *Obverse*, the head of a warrior. *Reverse*, Diana drawn in a car by two stags; below, a crescent, and the word *Roma*. It weighed 60.06 grains. Its specific gravity was 10.463. I received this, as well as the following coin, from the collection of Francis Farquharson, Esq. of Haughton.

Roman denarius: Silver nearly pure.

Muriate of silver, 78.94 gr.=59.68 silver	
Gold	0.29
Copper, by estimate	0.09
	60.06

The presence of copper was rendered manifest when the last solution was concentrated and mixed with ammonia. But I could not succeed in separating and weighing it.

18. A

Denarius of Domitian: Silver 4, copper 1.

18. A denarius of the emperor Domitian. On one side the head of Domitian. Inscription, *Domitianus Aug. P.M. Imp.* and four letters effaced. *Reverse*, a warrior with a spear and shield. The inscription too much effaced to be read. It weighed 52.28 grains. Its specific gravity was 10.092.

Muriate of silver, 55.35 gr. =	41.84 silver
Copper	10.02
Gold	.30
Loss	.12
	52.26

Austrian crown: Silver 10, copper 1.

19. An Austrian crown, dated 1612. *Obverse*, the head of Matthias II. with the inscription, *Matthias II. D. G. Hung. Bohe. Rex.* *Reverse*, the arms of these countries quartered, with the words, *Arch. Aust. Dux. Burg. Marg. Mo. Got.* It weighed 445.96 grains. Its sp. gr. was 10.233. I received it from Professor Jameson.

Muriate of silver, 533.5 gr. =	403.3 silver
Silver separated from the gold	.12
Copper	41.86
Gold	.40
Loss	.28
	445.96

SECT.

SECT. III.

e following TABLE exhibits the composition of 100 parts of each of the Coins examined, according to the preceding analysis.

Names of the Coins.	Silver	Copper.	Gold.	Loss.	Total.
1. ANCIENT.					
ecian Coin of Crotona . . .	96.27	0.88	0.11	2.74	100
man denarius of the republic	99.37	0.15	0.48	—	100
—— denarius of Domitian	80.03	19.17	0.45	0.35	100
2. MODERN.					
pee	96.06	2.95	—	0.99	100
itish. English half-crown .	91.26	7.71	—	1.03	100
——Scotch 40 shilling piece	92.41	7.03	—	0.36	100
ench half-crown	90.07	9.08	0.14	0.71	100
anish pisterine	83.04	15.20	—	1.76	100
—— pisterine	91.80	5.58	—	2.62	100
—— dollar	89.28	10.18	0.07	0.47	100
rtuguese. A 12 vintem piece	88.03	10.25	—	1.72	100
itch guilder	91.72	6.54	0.04	1.70	100
rdinian	90.04	9.96	—	—	100
iss	78.77	21.22	—	—	100
erman. Hamburgh	51.03	48.39	—	0.58	100
—— Ditto	50.25	45.02	—	4.73	100
—— Austrian crown . .	90.47	9.38	0.09	0.06	100
anish. A 60 schelling piece	87.87	12.07	0.02	0.04	100
ussian. A 15 copec piece . .	75.59	21.50	—	2.91	100

The

Gold accidental in silver coins.

The small quantity of gold must be considered as altogeth added to the silver when we The quantity of gold found in Roman, considerably exceeds the denarius of the republic it of the whole.

Ancient coins pure.

The ancient Greek and R formed of pure silver. The sm contain was doubtless accide could not have been added on Domitian contains 19.5 of co emperor then, it seems, the alloying their gold with coppe

Rupee almost pure.

The rupee likewise appears alloy, which amounts only t accidental than added on pur of rupees current in India, an purity.

European silver coins all alloyed, &c.

All the European silver coi the proportion varies consider examined, and the Hamburg Table exhibits the proportion according to their purity. I the copper, sometimes to the according as I suspected fron analysis, that the deficiency or other metal, or of both.

Relative purity of coins.

British	
Dutch	
French	
Austrian	
Sardinian	
Spanish	1 1
Portuguese	1
Danish	1
Swiss	2
Russian	2
Hamburgh	5

The first column of this Table gives the supposed proportion of alloy in 100 parts of the respective coin; the second gives the weight of silver contained in each coin, on the supposition that the weight of the copper with which the silver is alloyed is always 1.

VII.

On the Direction of the Radicle and Germen during the Vegetation of Seeds. By THOMAS ANDREW KNIGHT, *Esq. F.R.S. In a Letter to the Right Hon. Sir Joseph Banks, P.R.S.**

MY DEAR SIR,

Vertical position in which vegetables grow,

IT can scarcely have escaped the notice of the most inattentive observer of vegetation, that in whatever position a seed is placed to germinate, its radicle invariably makes an effort to descend towards the centre of the earth, whilst the elongated germen takes a precisely opposite direction: and it has been proved by Du Hamel,* that if a seed during its germination, be frequently inverted, the points both of the radicle and germen will return to the first direction. Some naturalists have supposed these opposite effects to be produced by gravitation: and it is not difficult to conceive that the same agent, by operating on bodies so differently organized as the radicle and germen of plants are, may occasion the one to descend and the other to ascend.

—ascribed to gravitation:

Not yet proved by facts.

The hypothesis of these naturalists does not, however, appear to have been much strengthened by any facts they were able to adduce in support of it, nor much weakened by the arguments of their opponents; and, therefore, as the phenomena observable during the conversion of a seed into a plant are amongst the most interesting that occur in vegetation, I commenced the experiments, an account of which I have now the honour to request you to lay before the Royal Society.

* Philos. Trans. 1806. † Physique des Arbres.

Probability that trial might be made of this by constant change of the position of the seed.

I conceived, that if gravit
descent of the radicle, and of th
act either by its immediate inf
and vessels during their formati
sequent distribution of the true s
and as gravitation would produ
seed remained at rest, and in tl
attraction of the earth, I imag
become suspended by constant s
of the germinating seed, and th
the agency of centrifugal force.

Beans were fastened in all positions to the circumference of an upright revolving wheel.

Having a strong rill of wate
I constructed a small wheel, sir
corn, adapting another wheel c
formed of very slender pieces
Round the circumference of the
in diameter, numerous seeds o
been soaked in water to produ
pansion were bound, at short d
radicles of these seeds were ma
others as tangents to its curve ;
others forwards, relative to its
opposite directions in lines para
The whole was inclosed in a b
a wire grate was placed to p
capable of impeding the motion

—which performed 150 revolutions in a minute.

The water being then admitt
thing more than 150 revolution
of the seeds relative to the ear
fectly inverted within the sam
conceive, that the influence c
wholly suspended.

The seeds grew with the germens directed to the centre, and the radicles in the opposite direction.

In a few days the seeds begar
of some of the opinions I had
many others which I had long
result of the experiment, I w
anxiety, though not with much
the pleasure to see that the radi
were protruded from the positio
outwards from the circumfere

subsequent growth receded nearly at right angles from its axis. The germens, on the contrary, took the opposite direction, and in a few days their points all met in the centre of the wheel. Three of these plants were suffered to remain on the wheel, and were secured to its spokes to prevent their being shaken off by its motion. The stems of these plants soon extended beyond the centre of the wheel: but the same cause which first occasioned them to approach its axis, still operating, their points returned and met again at its centre.

Repetition of the experiment with an horizontal wheel.

The motion of the wheel being in this experiment vertical, the radicle and germen of every seed occupied, during a minute portion of time in each revolution, precisely the same position they would have assumed had the seeds vegetated at rest; and as gravitation and centrifugal force also acted in lines parallel with the vertical motion and surface of the wheel, I conceived that some slight objections might be urged against the conclusions I felt inclined to draw. I therefore added to the machinery I have described, another wheel, which moved horizontally over the vertical wheels; and to this, by means of multiplying wheels of different powers, I was enabled to give many degrees of velocity. Round the circumference of the horizontal wheel, whose diameter was also eleven inches, seeds of the bean were bound as in the experiment which I have already described; and it was then made to perform 250 revolutions in a minute. By the rapid motion of the water-wheel, much water was thrown upwards on the horizontal wheel, part of which supplied the seeds upon it with moisture, and the remainder was dispersed, in a light and constant shower, over the seeds in the vertical wheel, and on others placed to vegetate at rest in different parts of the box.

The radicles grew obliquely outwards and downwards, and the germens obliquely inwards and upwards, and more so the swifter the motion.

Every seed on the horizontal wheel, though moving with great rapidity, necessarily retained the same position relative to the attraction of the earth; and therefore the operation of gravitation could not be suspended, thought it might be counteracted, in a very considerable degree, by centrifugal force: and the difference, I had anticipated, between the effects of rapid vertical and horizontal motion, soon became sufficiently obvious: the radicles pointed downwards about 10 degrees below, and the germens as many degrees above, the horizontal line of the wheels' motion; centrifugal force having made both

to deviate degrees from the perpendicular directi would have taken, had it vegetated at rest. Graduall ishing the rapidity of the motion of the horizontal wh radicle descended more perpendicularly, and the germe more upright; and when it did not perform more than volutions in a minute, the radicle pointed about 45 below, and the germen as much above, the horizontal li one always receding from, and the other approaching t axis of the wheel.

Remarks on the degree of accuracy of the experiments.

I would not, however, be understood to assert that locity of 250 or of 80 horizontal revolutions in a min always give accurately the degrees of depression and el of the radicle and germen which I have mentioned; rapidity of the motion of my wheels was sometimes dimi by the collection of fibres of conferva against the wir which obstructed, in some degree, the passage of the and the machine, having been the workmanship of mys my gardener, cannot be supposed to have moved with regularity it might have done, had it been made by a p sional mechanic. But I conceive myself to have fully p that the radicles of germinating seeds are made to descend their germens to ascend, by some external cause, and any power inherent in vegetable life; and I see little re doubt that gravitation is the principal, if not the only employed in this case by nature. I shall therefore ende to point out the means by which I conceive the same agen produce effects so diametrically opposite to each other.

Observations on the means by which gravity produces the vertical position in the radicle.

The radicle of a germinating seed (as many naturalist observed) is increased in length only by new parts succe added to its apex or point, and not at all by any genera tension of parts already formed: and the new matter wh thus successively added, unquestionably descends in a state from the cotyledons.* On this fluid, and on the veg fibres and vessels whilst soft and flexible, and whilst the m which composes them is changing from a fluid to a solid gravitation, I conceive, would operate sufficiently to gi inclination downwards to the point of the radicle; and as radicle has been proved to be obedient to centrifugal forc can scarcely be contended that its direction would r uninfluenced by gravitation.

* See Philos. Trans. 1805.

And also in the germen.

I have stated that the radicle is increased in length only by parts successively added to its point. The germen, on the contrary, elongates by a general extension of its parts previously organized; and its vessels and fibres appear to extend themselves in proportion to the quantity of nutriment they receive. If the motion and consequent distribution of the true sap be influenced by gravitation, it follows, that when the germen at its first emission, or subsequently, deviates from a perpendicular direction, the sap must accumulate on its upper side; and thence it follows, that the point of the germen must always turn upwards. And it has been proved, that a similar increase of growth takes place on the external side of the germen when the sap is impelled there by centrifugal force, as it is attracted by gravitation to its under side when the seed germinates at rest.

The same effect of gravitation takes place in the branches of trees.

This increased elongation of the fibres and vessels of the under side is not confined to the germens, nor even to the annual shoots of trees, but occurs and produces the most extensive effects in the subsequent growth of their trunks and branches. The immediate effect of gravitation is certainly to occasion the further depression of every branch, which extends horizontally from the trunk of the tree; and when a young tree inclines to either side, and thus occasions an increased longitudinal extension of the substance of the new wood on that side,* the depression of the lateral branch is thus prevented; and it is even enabled to raise itself above its natural level when the branches above it are removed; and the young tree, by the same means, becomes more upright, in direct opposition to the immediate action of gravitation, nature, as usual, executing the most important operations by the most simple means.

Inversion of the seed after protrusion of the germen does not prevent the effect.

I could adduce many more facts in support of the preceding deductions; but those I have stated, I conceive to be sufficiently conclusive. It has, however, been objected by Du Hamel (and the greatest deference is always due to his opinions), that gravitation could have little influence on the direction of the germen, were it in the first instance protruded, or

* This effect does not appear to be produced in what are called weeping trees; the cause of which I have endeavoured to point out in a former memoir. Philos. Trans. 1804.

were

were it subsequently inverted, and made to point perpendicu-larly downwards. To enable myself to answer this objectio I made many experiments on seeds of the horse-chesnut, an of the bean, in the box I have already described: and as th seeds there were suspended out of the earth, I could regularl watch the progress of every effort made by the radicle an germen to change their positions. The extremity of the radi-cle of the bean, when made to point perpendicularly upwards generally formed a considerable curvature within three or fou hours, when the weather was warm. The germen was mor sluggish; but it rarely or never failed to change its directio in the course of twenty-four hours; and all my efforts to mak it grow downwards, by slightly changing its direction, were in-variably abortive.

Another, and apparently a more weighty, objection to th preceding hypothesis (if applied to the subsequent growth an forms of trees), arises from the facts that few of their branche rise perpendicularly upwards, and that their roots always spre horizontally; but this objection, I think, may be readil answered.

Causes why the branches of trees do not exactly obey this operation.

The luxuriant shoots of trees, which abound in sap, i whatever direction they are first protruded, almost uniforml turn upwards, and endeavour to acquire a perpendicular di-rection; and to this their points will immediately return they are bent downwards during any period of their growth their curvature upwards being occasioned by an increased ex-tension of the fibres and vessels of their under sides, as in th elongated germens of seeds. The more feeble and slende shoots of the same trees will, on the contrary, grow in almo every direction, probably because their fibres, being more dr and their vessels less amply supplied with sap, they are le affected by gravitation. Their points, however, generally sho an inclination to turn upwards; but the operation of light, i this case, has been proved by Bonnet * to be very consider-able.

Why the roots of trees extend horizontally.

The radicle tapers rapidly as it descends into the earth and its lower part is much compressed by the greater solidity of the mould into which it penetrates. The true sap als

* Recherches sur l'Usage des Feuilles dans les Plantes.

continues to descend from the cotyledons and leaves, and occasions a continued increase of the growth of the upper parts of the radicle; and this growth is subsequently augmented by the effects of motion when the germen has risen above the ground. The true sap, therefore, necessarily obstructed in its descent, numerous lateral roots are generated, into which a portion of the descending sap enters. The substance of these roots, like that of the slender horizontal branches, is much less succulent than that of the radicle first emitted, and they are in consequence less obedient to gravitation; and therefore meeting less resistance from the superficial soil, than from that beneath it, they extend horizontally in every direction, growing with most rapidity, and producing the greatest number of ramifications, wherever they find most wamth and a soil best adapted to nourish the tree. As these horizontal or lateral roots surround the base of the tree on every side, the true sap descending down its bark, enters almost exclusively into them; and the first perpendicular root having executed its office of securing moisture to the plant whilst young, is thus deprived of proper nutriment, and ceasing almost wholly to grow, becomes of no importance to the tree. The tap root of the oak, about which so much has been written, will possibly be adduced as an exception, but having attentively examined at least 20,000 trees of this species, many of which had grown in some of the deepest and most favourable soils of England, and never having found a single tree possessing a tap root, I must be allowed to doubt that one ever existed.

The notion of a tap root is unfounded.

As trees possess the power to turn the upper surfaces of their leaves and the points of their shoots to the light, and their tendrils in any direction to attach themselves to contiguous objects, it may be suspected that their lateral roots are by some means directed to any soil in their vicinity which is best calculated to nourish the plant to which they belong; and it is well known, that much the greater part of the roots of an aquatic plant which has grown in a dry soil, on the margin of a lake or river, have been found to point to the water; whilst those of another species of tree which thrives best in a dry soil, have been ascertained to take an opposite direction. But the result of some experiments I have made is not favourable to this hypothesis; and I am rather inclined to believe that the roots disperse

Trees have a power of shooting towards moist or dry places according to their necessities.

disperse themselves in every c
numerous where they find n
adapted to the species of pl
however, been sufficiently vari
question, which I propose to i
tigation.

Elton, Nov. 22, 1805.

V

On Thunder Storms. I

To Mr. N

SIR,

Edge Lane,

THE many thunder storm
brought to my mind a few re
cate to you, nearly nine yea
at that time prevented me.
ed by any one else, I now trou
think them worthy of a place
will oblige me by inserting t
may be kind enough to mak
I am

Franklin's discovery that lightning and electricity are the same.

Though the effects of thun
served by all ages, yet the ca
matter of conjecture, until
ments were made on lightnin
doubt, that lightning, and the
same, as he was enabled to ch
ments by the electrical fluid
great facility as by means of t
Having proved the similari

cal fluid, the doctor naturally concludes that thunder is the sound caused by the discharge of a cloud containing an immense quantity of electrical fluid accumulated together, and says, "if two gun-barrels electrified, will strike at two inches "asunder, and make a loud report, what would be the effect "of perhaps ten thousand acres of electrified cloud."

Remark, that thunder is not merely from electricity,

If the discharge of an electrified cloud were the sole cause of thunder, and the clouds were charged to so large an extent, it is probable that the effects would be much more tremendous, and that fatal accidents would more frequently occur than they do at present; but it appears almost impossible that so large a quantity of the electrical fluid could be accumulated, particularly after the first discharge, when the equilibrium is restored, and the clouds, being full of wet vapour, are consequently become good conductors.

—but from the explosion of hydrogen and oxigen.

It seems more probable, that thunder is caused by the explosion of hydrogenous and oxigenous gas fired by the electrical fluid. It is well known that both these gases are constantly produced on the surface of the earth; that stagnated ponds, swamps, &c. emit carbonated hydrogen gas; and that plants are continually decomposing the air of the atmosphere, by imbibing the nitrogen, and emitting the oxigenous gas. This decomposition, and the formation of these gases, goes on more rapidly in warm than in cold weather.

Observations on thunder storms.

On the commencement of a thunder storm, it is observable that the clouds are not very dark, but that after one or two explosions they become very black, owing to the gases being converted into water: the rain then becomes heavy, and small detached clouds fly towards the storm in every direction, not, I imagine, by the attraction of the electrical fluid, but to restore the equilibrium, and to fill up the vacuum caused by the gases having exploded and formed water, and consequently occupying a much less space than they did when in the state of gas. These clouds, by containing the mixed gases, may serve to feed the storm, or the moisture in them may become decomposed by the electrical fluid,* and

* "Common fire, as well as electrical fire, gives repulsion to "the particles of water, and destroys their attraction of cohesion; "hence common fire, as well as electrical fire, assists in raising "vapours."—Franklin's Works, vol. i. page 206.

Observations on thunder storms, &c.

may form volumes of hydrogen and oxigen, which explode upon the next discharge of an electrified cloud.

If a thunder storm at a few miles distance be observed when the rest of the sky is tolerably clear, a quantity of white clouds, the bottom of them flat, but arched on the top, (owing, probably, to the resistance of the atmosphere against their rising, supposing them to contain the mixed gases, which are lighter than the common air of the atmosphere), will be seen floating above, and at the sides of the mass in which is the storm; by observing these white clouds attentively, some of them will be seen suddenly to become dark, and soon after to unite in the general mass. In this case, it appears that a partial explosion of the gases contained in those clouds must have taken place.

If thunder were caused merely by the discharge of an electrified cloud, and not by the gases exploding, the heaviest clap would always attend the largest and most vivid flash of lightning; but that is not the case, for a small clap of thunder frequently follows a large flash of lightning.

When the lightning takes an horizontal direction, there is frequently either no thunder, or a very small clap; but when it takes a perpendicular direction, the thunder is the heaviest. This is probably owing to the gases, which are formed during the storm, being collected above in considerable quantities. When I mention the lightning taking these different directions, I do not mean the electrical fluid, which appears blue, and frequently in a zig zag form, but that reddish light which surrounds it, and which has the appearance of a shadow, and may be frequently observed to extend to a great distance from the blue light, and which I conceive to be the gases in the act of exploding.

From the number of clouds that join the storm, the gases must frequently be in detached portions; and, when several of them are fired nearly at the same time, the explosions will be heard in several distinct claps, which may be observed in almost every thunder storm.

In hot weather, when it is rather windy, lightning is frequently observed unaccompanied by thunder; in this case, it appears that the wind has either blown the mixed gases abroad, or prevented their union.

From

From the above observations it appears probable, that the sound of thunder, and the formation of rain during a thunder storm, are caused by the explosion of volumes of hydrogenous and oxigenous gases fired by the lightning; and I am not aware of any phenomenon attending a thunder storm, but what may be easily accounted for upon the same theory. Thunder storms, &c.

IX.

Farther Communication from E. D. *respecting the Discoveries of Mayow.*

To Mr. NICHOLSON.

SIR,

I FORWARDED to you, a few days ago, some additional remarks on Lavoisier's claims, and stated some opinions of Mayow's concerning *acidification.* On looking again into that author, I have met with such further facts as I thought might be welcome to you, and request, Sir, that you will make such use of them as you think fit.

I am, Sir,

Your obedient servant,

E. D.

Speaking of the generation of nitrous acid, Mayow says: "Ut autem intelligatur, quo ritu spiritus acidus nitri in terra "generatur, liceat non nulla de *spiritu sulphuris*, cæterisque "liquoribus acidis præmittere: quippe spiritibus acidis qui-"buscunque similitudo maxima, et affinitas intercedit."—*Tractat. quinq.* p. 32. Mayow's induction, that sulphuric acid is formed by oxidation like the nitric.

He then combats the notion of the acidity of vitriol arising from an acid salt contained in the sulphur, and thinks the acid spirit may be formed during the deflagration of sulphur, from the mutual action of the sulphureous particles of the deflagrating matter, and the nitro-aerial particles of the air; which action he considered, however, to be of a mechanical nature. *Ibid.* p. 34. In support of these opinions, he refers to some circumstances occurring in the distillation of oil of vitriol, where he has these words: "Quippe experientiâ constat, quòd, si "distillatio vitrioli per decem, aut etiam plures dies cum igne That the acid is not in the sulphur.

 "maximè

" maximè intenso continuetur, spiritus tamen acidus inqu
" receptaculum prodibit: verum enimvero vix credend
" est, spiritum quemvis acidum adeo fixum, et ponderos
" esse, qui tam diu in igne quàm violentissimo permanere p
" sit: sed potius putandum est, particulas ignis nitro-aere
" longâ illâ distillatione vitrioli cum sulphure congredi et
" fervescere."—*Ibid.* p. 36.

That vegetable empyreumatic acids are formed during distillation.

His opinions concerning the formation of the vegetable ac are thus expressed: " Præterea nescio, an non spiritus aci
" ligni ponderosis, veluti ligno guaiaco, idque genus aliis d
" tillati, simili ratione per ignis operationem inter distill
" dum fiant: quippe lignum guaiaci ante distillationem ne
" quam sale acido, sed potius sale fixo donari videtur. H
" etiam facit, quòd particulæ nitro-aereæ ignis cum partic
" ligni sulphureis inter distillandum congressæ ad fluorem p
" ducant. Illud etiam obiter annotamus, quòd spiritus ac
" e saccharo, et melle distillati, haud multum absimili m
" per actionem spiritus nitro-aerei ignis fieri videantur."—
Ibid. p. 38.

Spontaneous formation of sulphuric acid.

As to the spontaneous formation of vitriolic acid, he st that if vitriol, from which the whole acid spirit has been e pelled by calcination, be exposed again for some time t moist air, it will be impregnated anew with the acid spirit.— *Ibid.* p. 38. Vitriols, he adds, are formed out of a saline su phureous earth, which, when committed to the fire, will yi flowers of sulphur, and when exposed a due time to air a moisture, spontaneously ferments, and becomes richly impr nated with vitriol. " Nimirum spiritus nitro-aereus cum s
" phure metallico marchasitarum istarum effervescens, part
" earum fixiorem in liquorem acidum convertit."—*Ib* p. 39. He further considers rust of iron to be produced the action of these same nitro-aerial particles, which, co bining with the metallic sulphur of the iron, form an ac that corrodes the metallic particles.—*Ibid.* p. 40. And, al luding to all these opinions in another part of his tract, has these words: " Quanquam enim spiritus nitro-aer
" acidus non sit, ab eodem tamen ferrum corroditur, vit
" ola conficiuntur, salia fixa ad fluorem perducuntur, r
" rumque compages tanquam ab universali menstruo solv
" tur."—*Tractat. quinq.* p. 55.

Sul

Such, Sir, are the facts noticed by Mayow, which appeared to me so striking, that I have been induced to send them to you, to make hereafter what use of them you please. I have avoided, as much as possible, introducing the absurd theories by which he endeavoured to account for the facts he observed, and which do not in the least affect the truth of the observations themselves; and I have sent them mostly in his own language, that you might give them in that, or in an English dress, as you think fit, as well as for the purpose of presenting them to you in the most authentic form.

E. D.

X.

SCIENTIFIC NEWS.

New Map of Scotland.

MR. Arrowsmith has been for more than a year engaged in constructing a new map of Scotland, from original materials, to which he has obtained access by means of the Parliamentary Commissioners for making roads and building bridges in the Highlands of Scotland. New Map of Scotland.

The elaborate military survey of the main land of Scotland made in the middle of the last century, and preserved in his majesty's library, has been copied, and reduced for the present map; and the several proprietors of the Western Islands have communicated all their surveys, most of which have been very recently executed.

In addition to the astronomical observations heretofore known, many latitudes and longitudes have been purposely ascertained for this map, as well as a considerable number of magnetic variations.

This map is to be accompanied by a memoir, explanatory of the several documents on which it has been constructed.

The publication may be expected to take place in January or February next.

Medical

Medical Theatre, St. Bartholomew's Hospital.

Medical Theatre.

The following courses of lectures will be delivered at this theatre during the ensuing winter:

On the theory and practice of medicine, by Dr. Roberts and Dr. Powell.

On anatomy and physiology, by Mr. Abernethy.

On the theory and practice of surgery, by Mr. Abernethy.

On comparative anatomy and the laws of organic existence, by Mr. Macartney.

On chemistry, by Dr. Edwards.

On midwifery, and the diseases of women and children, by Dr. Thynne.

The anatomical demonstrations and practical anatomy, by Mr. Lawrence.

The anatomical lectures will begin on Wednesday, October 1, at 2 o'clock, and the other lectures in the course of the same week.

Further particulars may be known by applying to Mr. Nicholson, at the Apothecary's shop, St. Bartholomew's Hospital.

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END OF THE FOURTEENTH VOLUME.

TO THE BINDER.

Cancel the first Leaf of Sheet O o, page 231, and insert the repr[inted] Leaf, which is the last in this Volume.

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